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MEDICAL GAZETTE;

BEING A

Weekly Journal

OF

MEDICINE AND THE COLLATERAL SCIENCES.

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SATURDAY, APRIL 5, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XXVI.

LABOUR.

AFTER TREATMENT.

We have fully considered the duties which devolve upon the obstetrical attendant, down to the termination of active labour; still, as I before remarked, the process of parturition is not to be considered as finished, even on the birth of the placenta; for the same action which expelled the child is continued for some time, for the purpose of closing the uterine cavity, and contracting its dilated vessels. The woman, therefore, cannot be looked upon as safe, even from the immediate effects of the labour, for many hours; and the puerperal state is not to be regarded as terminated until the womb has re-acquired its small, original, unimpregnated size.

Medicine.—Having made her person as comfortable as we can, by surrounding the lower part of it with dry and warm napkins, and allowed her some tepid drink, our attention is to be directed to the necessity of medicine.

It is the custom in some parts of the kingdom, and by some practitioners in London, to give a large dose of laudanum immediately after delivery, to quiet the system, to lull the excitement, to still the after pains, and to procure sleep. I hold this practice as a principle to be even more injurious than the exhibition of large doses of stimuli, because, besides acting as a strong stimulus for the moment, opium exerts a powerful narcotic effect afterwards; and by this effect must interfere with those proper and indispensible contractions which the uterus is taking on itself. It is true we can relieve the patient from the annoyance of after-pains; but at the same time that we remove the pain, we are incurring danger; we are cramping nature, by depriving her of the only power she possesses for insuring the woman's continued safety. The same objections, indeed, do not apply to opiates in a small quantity; they are, in minute doses, likely to do good rather than injury, because they may soothe irritability without interfering with the necessary changes going on in the uterine system. If, then, we can give such doses of opium, and repeat them at such intervals as will just induce a state of gentle quietude, and yet not suspend the uterine contractions, we shall be rendering the best service in our power. I think that by the exhibition of four, five, or six minims of laudanum, or a corresponding quantity of any other sedative drug, repeated every four or six hours, we shall be most likely to effect this object.

Medicine of any kind may often not be required after delivery; but in many cases it is useful; and in few can even opium do harm, if exhibited in small quantities, unless there exist a peculiar idiosynerasy of constitution, unfavourable to its action. It is as well, then, that something should be ordered;—not simply because it is expected;—not merely because the patient may consider herself neglected if it be omitted, and may attribute any inconvenience she may afterwards suffer to that omission;—but because it tends to keep down excitement, and to induce repose.

The old-fashioned spermaceti draught used to be a favourite medicine after labour; and some of my friends are still in the habit of prescribing it. It was first administered under the idea that spermaceti was a specific for inward contusions, and that under labour the neck and mouth of

the uterus, and the vagina, were necessarily bruised by the passage of the child. Both the positions, however, on which this practice was founded, are erroneous; neither is spermaceti a specific for inward bruises, nor is it usual for any inward bruising to take place under labour. But spermaceti is a lady-like, elegant sort of draught; and the only objection to it is the difficulty of preparing it;—it requires a good deal of manipulation, and a little art, in order to mix it well. The medicine that I prefer is quite as elegant and pleasant; it consists of an ounce of almond mixture, half drachm of syrup, five or six minims of laudanum, and three drachms of cinnamon water. If a febrile condition be present, perhaps we may be inclined, instead of giving almond mixture, to prescribe a saline. Should the skin be dry and hot, we might prefer administering three or four drachms of liquor ammoniæ acetas; but it is seldom that this state prevails, as the woman generally has been perspiring profusely during labour; and there is therefore no necessity to stimulate the cutaneous vessels further.

Before the house is left, after the patient's person has been protected in the way recommended at our last meeting, a draught, such as I have just mentioned, may be prescribed; the time occupied in writing instructions for which may probably be ten or fifteen minutes; and it is then right to make another examination of the uterus, through the parietes of the abdomen, to ascertain that it has not become relaxed in the interval; the napkins, also, round the hips and on the vulva, must be again inspected, that we may assure ourselves no external hæmorrhage is going on. This latter inquiry is better left to the nurse. If upon this examination we find that the uterus is still small, and almost as hard as a fœtal head—if the linen be but little soiled—if not more than two or three coagula, the size of a walnut, have passed—we need be under no alarm with regard to the state of the patient; so far as hæmorrhage is concerned, she is safe, most probably, for that labour; at any rate for the present moment. If, on the contrary, we observe a considerable discharge of blood upon the bed, if the uterus be large, soft, and flaccid; or if, on pressure being applied, a coagulum escapes, or a quantity of fluid blood passes, with a gurgling noise, she is then flooding; she must not be left, but will require careful superintendance, probably for many hours.

But we will presume the case to be of the more common kind—one in which the uterus is small and contracted, in which there is a slight discharge from the external parts,—the napkins being but partially soaked,—and in which the feelings are comparatively, in other respects, comfortable. We may then take our leave, giving instructions to the nurse with regard to her future management, until our next visit; and these instructions should be clear, positive, and definite. It is not unlikely that this important and bustling personage may consider her dignity offended, or her province invaded, by the orders you may think it right to leave; but your patient's welfare and comfort so much depends on proper attention being paid her during the next few hours, that nothing should be left to the caprice or prejudices of a nurse.—The first injunction, then, to be given is as to the length of time she should be allowed to remain quiet, until her linen is changed, and she is removed from her position. If there be no hæmorrhage or faintness, she may lie for an hour after your departure, supposing you have staid in the house at least half an hour from the removal of the placenta.—The next must be with regard to the mode of removal. She must not be allowed to get off the bed, either to sit or stand, nor must she of her own accord move hand or foot in the way of exertion; she must have the dress in which she was delivered taken off as quietly as possible, fresh linen placed on her person, and she must be lifted, with the least possible assistance on her part, into the place previously prepared for her.

Bandage.—Again, we must not omit to give directions about a bandage, or *safeguard*, as it is usually called, in the idiom of the puerperal chamber. Most frequently, indeed, the medical man's attention is called to the propriety of its application, either by the nurse or the patient herself, so that it seldom becomes necessary for him to give orders respecting it: for women have an idea that the more tightly their persons are braced after delivery, the more likely are they to preserve the symmetry of their form; and this is a point very near their heart. There are few, indeed, who are careless about possessing a good figure; and so long as this prejudice prevails—provided the female breast continues to throb with its present passions and desires—so long nothing will be neglected by them to improve those corporeal graces with which nature, in her prodigality, has enriched them. Some practitioners in this country,—men of eminence, and whose names carry with them great authority,—have suggested, that from the first commencement of labour, a bandage, formed with four tiers of tape, should be placed round the waist; that the first may be tied loosely when labour begins; the second pretty tightly after the membranes have ruptured, when the uterus is supposed to be considerably diminished in size; the third when the child is born; and the

fourth after the placenta is expelled: so that a regular pressure should be kept up on the uterus as it contracts. Now I have a great objection to this plan, because, in the first place, it renders our duty complicated. I do not oppose it on account of the trouble it brings with it, but it may interfere with other matters of more consequence. Another great objection is, that the bandage becomes soiled by the discharges, or is soaked with perspiration; and after the patient is quietly put into bed, she has for some time a wet and dirty piece of calico next her skin, instead of a newly-adapted bandage, or clean linen; and more serious evils might result to her than even her present uncomfortable feelings.

But although I strongly object to a bandage being placed on the person of the woman previously to the commencement of labour, I think the application of one after delivery highly useful. Its management, however, had better be left entirely to the nurse; for there is surely something highly indelicate in its being applied by a man;—much more so, indeed, than any of the duties we are ordinarily called upon to perform under natural labour. To be of service, it should be next the skin, must be sufficiently broad to reach from the pubes to above the navel, and cannot be properly adapted unless the abdomen be more or less uncovered. It ought not to be put on immediately after the expulsion of the placenta; because it is right that perfect quietness should be preserved, until the first changes in the uterus are effected, that no disturbance may interrupt their progress. The principal object which the bandage serves is that of bracing the bowels, and giving an artificial support, in lieu of that which they have lost through the laxity of the abdominal muscles; and to prevent the faintness necessarily induced on the sudden removal of a certain degree of pressure. It may to some extent, indeed, stimulate the uterus to more perfect contraction; but if that organ be unnaturally flaccid, it would be wrong to rely on compression by a bandage, to insure its more powerful action, or prevent its cavity becoming distended with blood;—in such a case, the only safe means of exerting sufficient external pressure is by the grasp of the hand steadily, and, for some time, unremittingly, applied.

All the apparently, perhaps, unimportant cautions I have given you regarding the management of a person immediately after delivery, are highly necessary to be attended to; because very many women have lost their life, or have been rendered miserable for the remainder of their existence, through the stupidity, ignorance, or obstinacy of their nurse. Many a woman has lost her life from syncope,—the mere consequence of being placed in the erect posture, either on her feet or in a chair, while her attendants have been preparing the bed for her reception. In many an instance a sudden and fatal gush of blood has followed the same injudicious proceeding; and I have personally known two cases in which an aggravated procidentia of the uterus occurred, owing to the same cause, which continued as long as the patients lived. Both these women died early in life, and in both the origin of the disease which destroyed them was attributable to such injudicious management immediately after the exertion of labour. We therefore require that she should lie still a certain time, and not be moved into the upright posture, in order that,—besides other evils,—we may prevent syncope, flooding, and procidentia uteri.

Interval between leaving the patient and the next visit. — After taking our leave, it will depend upon many circumstances how long should intervene between the present and our next visit,—at any rate, that should not be defered beyond twenty-four hours; but it is much better that it should be made within twelve.

Information to be gained at the first visit.— Upon our first visit there are a number of questions which it is necessary to ask, and a great deal of information which it is necessary for us to gain. There are other questions, perhaps, which may be put *pro forma*. Those points which it is necessary to inquire into, and without gaining positive information upon which we must not be satisfied, are—whether she has been much harassed with pain, and what sleep has been obtained; for sleep, the grand restorer of wearied nature, is especially requisite after labour. It is fortunate if our patient tells us that she has had two or three hours quiet rest. We do not expect uninterrupted rest, because she will be disturbed by the after-pains; but if she has not suffered much from this cause of annoyance, and has had three or four hours sleep during the night, we consider it as a good average. Of the nurse we require to learn whether any water has passed from the bladder, (for that is a matter of great consequence;) and what sort of a discharge there has been. The sanguineous discharge does not cease as soon as the placenta is expelled, nor ought it to disappear suddenly; but a continual oozing of blood goes on from the uterine vessels, more or less, for some time after delivery. In scientific language, this flow is known by the name of the *lochia;* among women, in general, by that of *discharge;* and by the vulgar it is called the *cleansings*. For some days this dis-

charge continues to possess all the constituent parts of the blood; but it gradually loses the firmer portions and red globules, and before its final departure it becomes of a serous character, possessing a greenish tint; and is then known, in the language of the lying-in room, by the name of the *green waters*. This change in its charaeter and appearance is the result of the continued contraction going on in the uterus. At first, when the uterine parietes are comparatively lax; when the vessels are of large diameter, and their apertures perfectly patulous; all the essentials of the blood are allowed to escape through them, and the discharge is consequently purely sanguineous: but after a time, in proportion as the uterus contracts,—as the vessels are diminished in their calibre,—as the openings through which the blood exuded become smaller,—the fibrin and red globules, by degrees, are prevented escaping, until at last the serum only oozes out, carrying with it the smallest possible quantity of the colouring particles. On any exertion indeed being used, and sometimes, merely on the first rising from the bed, the discharge may assume a more florid hue, and be more copious than it had been for some time past: unless, however, this be to a debilitating extent, it is not usually necessary to enjoin any stricter confinement in consequence. If, then, on our first visit, we learn that the bladder has acted freely,—although, perhaps, with some trifling pain; that the discharge has been sufficient to have required the removal of four or six napkins, —and that a small coagulum or two has also passed; we may consider the actions of the pelvic viscera so far to be going on in a healthy manner. We are not to expect that a motion will have been voided; it is very rarely that the bowels act within the first twenty-four hours after delivery, unless diarrhœa have existed previously to the accession of labour.

After information on these points is obtained, we may require to place our hand on the abdomen, to ascertain whether the uterus is still contracted, and whether pressure upon it gives pain; and we may, at the same time, learn whether the bandage is properly applied. If it has shifted its position upwards towards the bosom, as it frequently does, we must require the nurse again to adapt it. We must, of course, make our observations on the tongue, pulse, and countenance: from the appearance of the latter, we shall gain more information than can be described. If the patient looks pale, haggard, anxious, and weary; if her features are shrunk, something is wrong: if, on the contrary, she is placid,—her countenance resuming somewhat of its natural expression, even although more than usually pallid, while the pulse is 70 or 80; the tongue and mouth moist and clean; then every thing is most likely going on correctly.

It is not right that we should leave the house, without taking some notice of the infant. All women are flattered by having their children noticed, and especially young mothers. Partly out of compliment, therefore, but principally for information, we should be particular in inquiring after the child. We must learn from the nurse whether it has passed urine and stools; and should the answer not be satisfactory, we should make a personal examination, that we may early detect any malformation which may exist in the rectum or urinary organs.

I have before mentioned, that the meconium which has been accumulating in the large intestines during the latter months of utero-gestation, is evacuated as the first stools of the infant after birth; and that it possesses a dark green colour, and is of a viscid, sticky consistence; very different in every property from the light yellow-coloured fœcal evacuations, which take place after digestion has commenced. We must inquire, also, whether the child has been put to the breast. Most nurses have a disinclination to put the infant to the breast during the first few hours after labour, but this is a very injurious prejudice. We are told "there is no use in teazing the poor little creature, for it sucks *nothing but wind!*" How can it suck wind?—if it gets nothing at all, even so far from the early application being hurtful, it does good; because the stimulus of the mouth upon the nipple produces a more speedy secretion of milk than would otherwise take place. Besides, the first-formed fluid itself is rather of a stimulating quality, and acts as a slight purgative; so that, both for the mother's sake and the child's, it is advisable that it should be applied to the breast within twelve hours after labour at the farthest.

We must also direct our attention to the state of the mother's bowels. It is the custom in London to give an aperient draught on the morning of the third day after labour. Castor oil is one of the best medicines we can have recourse to, if she can be persuaded to take it; but many women cannot at any time retain it on their stomach; and others have taken such a quantity during pregnancy, that the very mention of it produces nausea: to such a common aperient may be given, and the best, perhaps, is the compound infusion of senna and jalap. The draught I generally use is the following:—comp. infu. senna, 1 ounce; jalap, 8 grs.; tart. of potass, 1 or 2 drachms; peppermint water, 3 drachms; manna, a drachm or two; and tinct. of ginger, or senna, half a drachm. This is

seldom complained of as being a nauseous draught, and it mostly operates speedily and satisfactorily, without producing much griping. It should be repeated every four or six hours, till the bowels act; for it is highly desirable that evacuations should be obtained some time in the course of the third day.

Diet.—Nor must diet be omitted in our instructions. Nothing should be allowed but tea, toast, or farinaceous food, until the bowels are freely opened; and after the operation of the laxative on the same day, a little beef-tea, mutton or chicken-broth, may be given. Such kind of nourishment is all that is required to sustain the system, under any depression the action of the bowels may have caused.

Temperature.—Neither should the temperature of the room be overlooked. You will often, even in the midst of summer, find the curtains drawn close around the bed, and a fire in the chamber; and you observe, if you put your hand on the pulse, that it is quickened by the application of external heat, while, at the same time, there is a profuse perspiration on the skin. The curtains should be undrawn, that free ventilation may be permitted, and directions should be given that no larger fire be kept than is required for the purposes of the lying-in room. It is as well to hang a thermometer constantly in the apartment, that the temperature may be regulated every day. Between 60° and 65° will be found the most suitable warmth, both in winter and summer.

Till the middle of the last century, it used to be the practice to force a woman's system with spices and cordials, immediately after she was delivered; to prevent her enjoying a single breath of fresh air; to put sand-bags under the chink of the door; to nail the windows round with list, and take every possible precaution to oblige her to breathe over and over again the same vitiated atmosphere. A more sure method of exciting fever could scarcely be adopted. In more early times, plaisters, fumigations, fomentations, cataplasms, ointments, and oils, mostly composed of stimulating or odoriferous drugs, were applied to the abdomen and vulva, with the view of promoting a free lochial discharge: and we are told that those women who had the misfortune to be in affluent circumstances, were compelled to submit to the infliction of a sheep skin, warm and reeking from the carcase of the newly-slain animal, placed round the abdomen, to cherish and protect them. This practice was adopted under the belief that the child, in its exit from the uterus, had deprived the mother of a certain degree of animal heat, which it was the intention of this loathsome application to supply. It is not wonderful that inflammatory, typhoid, miliary, and other fevers, were in those days rife; we can only be astonished that, in any case, nature had power to avert the dangers which such an interference with her laws, and subversion of her intentions, must have created.

These observations, however, refer particularly to the middle ages and succeeding years; for the ancients treated puerperal women as though they had suffered some violent and extensive accident, as we learn from the recommendations inculcated by Celsus. They were confined for a certain number of days to the sparest diet, and severest regimen. Of the two methods, that advised by Celsus must be regarded, on the whole, as most consonant with reason;—nevertheless, no general plan can be universally applicable, but a deviation from it must in some instances be necessary.

On the third day, then, after delivery, the patient may take for nourishment some solution of animal matter; the next day, or day after, nothing forbidding, she may add to this a light pudding; and in a week she may be allowed a small quantity of solid meat. Stimulants of any kind, unless there be an actual necessity for them, never should be permitted until the end of a fortnight, and then a glass of wine and water, or mild malt liquor, may be taken.

Posture.—With regard to position, we must keep her recumbent as much as possible, for at least a week. She should not sit up, even to have the bed arranged, for that time. She may be moved daily from one side of the bed to the other, and lie on each alternately. In this manner she can have the advantage of a change every day. If the bed, however, heats her, or lying on it is very irksome, she may be moved daily, preserving the horizontal posture, for an hour, to a sofa. The ninth after delivery is looked upon, by women, as a critical day: many consider that if they have so far escaped the danger of the puerperal state, when that day is past they are safe from all the perils of their condition; and some think that however much they may have indulged their appetites before, and although they may have been up for some hours for the two or three preceding days, on that they are bound to fast and keep their bed. Although this prejudice is founded on error, it is as well to favour it; because it is highly desirable that every woman should be kept in a state of perfect rest, and should submit to be treated strictly as an invalid—at any rate, until that period of time has gone by.

After a week, then, she may get up, and lie the principal part of the day on a sofa. After a fortnight she may begin to put her

feet to the ground, and she may take an occasional walk about the room: but the liberty allowed in this respect must depend very much on the continuance of the lochia. So long as the discharge is flowing at all profusely, the necessary changes going on in the uterus are by no means perfected; but if it has almost ceased at the end of fourteen or eighteen days, we may suppose that the uterus has nearly re-acquired its small unimpregnated size, that the parts are restored to their original tone, and consequently that the risk of prolapsus uteri no longer exists.

Continuance of visits during the puerperal month—Ablution.—We are expected, in this country, to give our attention both to the mother and her infant during the whole puerperal month; or at least until she has quitted her chamber: it is necessary that a visit should be made daily, until the end of a week; after which time, the attendance may be regulated according to the circumstances of each case. At every visit the state of the bowels must be particularly inquired into, and care must be taken that they act regularly. They are usually torpid while the woman is inactive, and it is requisite to repeat the aperient draught occasionally. The bandage should be tightened, and the vulva should certainly be sponged daily. Women of the lower class have a great prejudice against the use of water while the lochial discharge is flowing; and many nurses will permit the whole month to pass without proposing to wash the external parts at all: a practice as hurtful as it is filthy. If, then, we merely give general directions that warm water should be used liberally, the order will very likely be neglected; because the nurse will say, "I know the use of water as well as the doctor: it is not proper." But if we particularly direct her to add to the water one-fourth of gin, she will think that addition is for some peculiar purpose, and will probably obey us. We are told, indeed, by some respectable authorities, that spirit exerts a specific influence in this state. It is thought that it stimulates both the absorbents and the uterus itself to increased action, so that that viscus is restored more quickly to its unimpregnated volume. I do not exactly believe in this doctrine, but for the sake of cleanliness it is highly desirable that water should be used, and there is not the slightest objection to the addition of a small quantity of spirit. After three or four weeks, cold water may be substituted, and the parts may be liberally sluiced with it; especially if the time of year be summer.

Suckling.—It is not generally that we are asked the question whether a woman should suckle her child or not; or are called upon to interfere. If the patient be well, and she does not mean to suckle, she will not consult her medical man about it; because she knows his advice will go exactly contrary to her intentions: but if she be ill, and cannot suckle, it is then our duty to prevent her, and to require that a wet-nurse should be procured for the child. Some women are averse from suckling, because of the trouble and confinement it necessarily occasions; but others, on the contrary, regard it as the most grateful and pleasing office they can perform. No one will deny that it is the bounden duty of every woman, provided she has health and strength, and means, to nurse her own child, in whatever station of life she may be placed. She should forego the pleasures of society, give up the necessity of appearing in public, and waive the etiquette even of a court, if those pleasures, or that etiquette, interfere in any degree with her duties to her infant. I cannot allow that any physician would be honestly and conscientiously fulfilling the trust reposed in him, who did not, even in the highest grade of society, point out the dangers that may spring from this most natural and engaging office being abandoned; and I should always think better of that woman's feelings, both towards her husband and her infant, who gave it the advantage of the breast.

No doubt it is much both to the mother's and child's happiness, comfort, and health, for the process of suckling to go on. Every thinking person will agree that milk, being the nourishment afforded by nature, is much more congenial to the child's wants than any extraneous food; that it is most likely to afford suitable sustenance, and preserve the system in a healthy state. Nor is the process of suckling, indeed, less beneficial to the mother than her infant, although its advantages to her may not be so immediately apparent: for, putting out of the question the more obvious ill effects that flow from a suppressed secretion,—such as inflammation of the glands of the breast, and consequent suppuration,—many less evident evils arise, among which may be enumerated, congestion of the abdominal and pelvic viscera, and undue determination to the head,—the consequence of that blood which ought to be drained away from the general system by the breast, for the formation of milk, being suddenly thrown into other channels, and upon other organs:—so that, independently of the strong natural inclination which would prompt every woman to suckle, the child's safety and her own health should also stimulate her to undertake the grateful and important office of nurse.

One of the most frequent causes inducing women to decline suckling, is the existence of sore nipples; and it certainly appears cruel to insist on a continuance of what produces so much pain. But we have means to defend the tender organ; and we can cure the ulceration: and this in itself is seldom of sufficient importance to warrant our allowing a mother to put her child away.

Sometimes, however, we find,—especially among the poorer classes,—that women will suckle longer than is desirable for their own strength, and for the health of their infant, under the belief that they are not susceptible of pregnancy so long as the least secretion of milk is kept up by the lacteal glands. To a certain extent this idea is correct; women are certainly not so likely to become pregnant while nursing, as after the cessation of that function, provided they continue to suckle for the period only that nature intended: but if they exceed the just limit, the continuance of the child at the breast affords them little or no protection. Thus among the lower orders it is not very uncommon to see a woman suckling her last infant till within three or four months of her next confinement, much to the destruction of her health, and the undermining of her bodily powers. We mostly observe, indeed, that the milk in twelve or fourteen months after delivery decreases in quantity, and becomes deteriorated in quality; and the child now evidently requires some other nourishment besides what the breast affords.

Some line, then, must be drawn at which the infant should be weaned; and perhaps, as a general principle, twelve or thirteen months will be found the most fitting time. We cannot wonder, either, that suckling, protracted to such an extent, is also injurious to the infant; and my friend, Dr. Morton, has endeavoured to prove, that children kept at the breast beyond nine or ten months are more particularly subject to some of the infantile diseases, especially hydrocephalus, than if they were weaned at the proper time. In a very ingenious essay on the subject, he has brought forward a number of cases, and adduced good reasoning to establish the truth of his doctrine.

Recapitulation.—Before we proceed to the consideration of the irregularities of head presentations, we will recapitulate a few of the leading facts regarding the management of natural labour.

You must not neglect to attend the summons immediately on being sent for to a case of labour; to take with you a catheter, a lancet, and, if you choose, a little laudanum; to endeavour to ascertain, as soon as you are admitted into the house, how the labour is advancing—which you can *perhaps* do, by questions addressed to the nurse and the patient herself, and by general observations; and which you can *positively* do by making an examination per vaginam. Under this examination,—supposing pregnancy to exist,—you ascertain whether labour has begun; and if so, you learn how far the os uteri is dilated,—whether it is soft and flabby,—whether the membranes are broken,—what part of the child presents,—and the state in which the vagina and perineum are, with regard to their distensibility. On gaining this information, if you find the os uteri dilated beyond the size of half a crown, it is right that you should stay in the house; but it is not necessary that you should remain in the patient's chamber. Frequent examinations should not be made during the first stage, lest you should rupture the membranes, or deprive the parts of their natural lubricating mucus; and,—to prevent the probability of breaking the membranous cyst before it has performed the intentions nature has evidently assigned to it,—this examination should be made when the pain is on the decline. After the membranes are broken, the os uteri is well dilated, and the head has descended into the pelvic cavity, the bed-room should not be left; and when the head is on the perineum, extending the structures at the outlet of the pelvis, the left hand, covered by a napkin, should be steadily applied to support them, and avert the chance of injury.

When the child's head is born, the mouth must be wiped with a clean napkin, in order to remove any mucus that may hang about the lips. The finger must next be passed around the neck, to ascertain whether the funis umbilicalis is encircling it; and if so, it must as speedily as possible be liberated. The body of the child must by no means be hastily extracted; but time must be given for the natural expulsion of the shoulders. On their passage the perineum must be protected, as when the head was emerging. When the child is entirely born, it must be removed a small distance from the mother's person, and two ligatures are to be applied upon the funis umbilicalis, which must be divided in the centre between them. The hand must then be placed upon the abdomen, to ascertain the state of the uterus; and, provided there be not a second child, there are five conditions, in each of which it is occasionally found; three in which it is small, and two where it is large: the former prove that the placenta is expelled wholly or partially into the vagina, and the latter shew that it is still retained within the uterus. You are not to interfere with the placenta as long as it remains

in utero; you are not to pull at the funis, with the hope of facilitating its extraction; but allow it to be expelled by uterine action, unless hæmorrhage, or lapse of time, prompt its removal from the uterine cavity. But as soon as it is entirely external to the uterus, and is lying loose in the vagina (which you know to be the case when you can not only detect the insertion of the funis into the substance of the mass, but encompass its general body, by the finger introduced into the vagina, as in a common examination, and carried around it), you may remove it by gentle traction at the cord; and, being careful to bring away the membranes entire, deposit it in a basin prepared for the purpose.

Immediately after the placenta is removed, you must place your hand again on the abdomen, to ascertain that the uterus still continues in a contracted state; and you will usually find it almost as small as a fœtal head, and tolerably firm. You may then remove the soiled napkins from the patient's person, and replace them by others that are warm and dry.

If the uterus be thus firm and well contracted, you may suppose that there is scarce any danger of flooding, and you may give a little medicine, containing a few minims of tincture of opium, allowing nothing but cool gruel or diluents. You should remain in the house after the placenta has passed, at least half an hour, lest relaxation should occur, and flooding supervene; and the last thing before you retire, you should again place your hand on the abdomen, to assure yourself of the woman's continued safety. Before the child is washed, you must observe that the ligature is tight around the funis, and that no bleeding is going on. These are the principal duties you have to perform during the progress of a natural labour*.

ON THE USES OF THE GASTRIC JUICE,

AND THE SUBSTITUTION OF VOLTAIC ELECTRICITY FOR THE NERVOUS INFLUENCE.

To the Editor of the Medical Gazette.

Sir,

Having been led, in reply to a passage in Dr. Prout's Bridgewater Treatise, to the subjects which form the title of the present communication, I beg to make a few additional observations respecting them.

I mentioned, as a prominent instance of the adaptation of means to their end, so strikingly displayed in many of the functions of the living animal, the manner in which the muscular power of the stomach is employed in the application of the gastric juice to the food.

I may now add, that such are the principles on which the supply of this juice depends, that the quantity supplied must always, in a healthy stomach, be proportioned to the demand for it, there being neither defect nor redundancy; and that its uses well illustrate the observation, that although in human endeavours many attempts often fail to effect one purpose, in the works of God, each arrangement not only

> "———————— can its end produce,
> But serves to second to some other use."

The gastric juice is not only the immediate instrument by which that part of the digestive process which belongs to the stomach is effected, but (as has been proved experimentally) is the means of informing us both of the want of food, when a fresh supply is necessary, and, if we listen to its dictates, of the quantity required. Nor is this all, for its secretion being influenced by all those causes which would render a supply of food injurious, instead of beneficial, it fails, and with it the appetite, when the other processes of assimilation being suspended, the chyle prepared in the stomach would only prove a source of irritation; nay, the appetite, excited by the gastric juice, is so modified, as, under certain circumstances, even to point out to us the kind of food at the time required, giving us, at one time, a high relish for the stronger kinds of food, and at another for the more cooling and less nutritious kinds; and it would be well if the dictates of the physician could with the same certainty be relied on.

If we can trace such proofs of design in the formation of, and the circumstances relating to, one secreted fluid, what conception can we form of the wisdom and power which would be displayed in all the functions of the living animal, so complicated, and yet so correctly adapted to their ends, if we had knowledge suf-

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* In Dr. F. H. Ramsbotham's last lecture, in the fifth line from the end, *for* "no sanguineous discharge," *read*, "but little sanguineous discharge."

ficient to trace all the workings of its Maker!

In endeavouring to elucidate the nature of the nervous influence, the first step was evidently to ascertain its functions, to distinguish its functions from those of the sensorial power, with which, in many instances, they are so intimately blended. For this purpose an extensive set of experiments was made, a detailed account of which the reader will find in my Experimental Inquiry into the Laws of the Vital Functions. It appears from these experiments that, besides the excitement of the muscles of voluntary motion, universally acknowledged to be a function of the nervous influence, its functions are, the occasional excitement of the muscles of involuntary motion; the act of forming from the blood the various secreted fluids; that of supporting all the other assimilating processes by which the healthy structure of every part is maintained; and that of causing an evolution of caloric from the blood, for the purpose of preserving the due temperature of the animal body.

Having determined the functions of the nervous influence, we are prepared to ascertain whether there be any other power capable of all these functions, if made to operate under the same circumstances, that is, applied in the same way to the same parts, and while the vital principle is unimpaired.

Voltaic electricity, it had been found, is capable of exciting the muscles of involuntary as well as those of voluntary motion. And it appears, from the experiments referred to in my former paper, that it is not only capable of forming the secreted fluids from the blood, when applied in the same way, and under the same circumstances as the nervous influence, but of maintaining all the other functions on which the healthy stricture depends; and in both instances, as far as we can see, as perfectly as the nervous influence itself.

These points having been ascertained, it only remained to determine, whether it is capable of causing an evolution of caloric from the blood. Thus the determination of this point became an *experimentum crucis*, because, if it be determined in the affirmative, voltaic electricity is thus proved to be capable of all the functions of the nervous influence. It appears to be so determined by experiments related in the twelfth chapter of the second part (third edition) of my Inquiry into the Laws of the Vital Functions.

A circumstance occurred in the course of these experiments, which strikingly illustrates some of the statements in my former paper.

It is evident, that in determining the question before us, arterial blood must be employed: venous blood having already undergone the operation of the nervous influence, is no longer capable of any of the functions maintained by this influence, till its vital properties are renewed by the changes effected in the lungs. Arterial blood from a rabbit was received into a cup, placed in water of the temperature of 98°, namely, that of the animal, and the bulb of a very sensible thermometer, and the wires from the opposite ends of a powerful galvanic trough—the wires being placed one on each side of the bulb—were immersed in the blood, which had been in the cup about two minutes before the apparatus was adjusted. But although the appearance and consistency of the blood was greatly changed by the effects of the electricity, there was no extrication of caloric; the thermometer even fell faster than that in another cup, the arrangement and contents of which were the same, with the exception of the galvanic wires. The experiment was then repeated in the following manner:—The cups being arranged as in the preceding experiment, and, in one, the galvanic wires placed as before, the same artery in two rabbits of the same temperature (98°) was then opened, and the blood allowed to flow into the cups; a thermometer, raised to 98°, being put into each of the cups at the moment the blood began to flow into them. One minute after the blood began to flow into the cup without the wires, the thermometer stood at 97°; in a quarter of a minute more—that is, a minute and a quarter after the blood had begun to flow into the cup—at 96°; and so on gradually falling; for it is to be observed, that although the cups were raised to 98°, the air in them was ten degrees lower. One minute after the blood began to flow into the cup with the wires, the thermometer stood at 100°; in half a minute more at 102°; in half a minute more at 100°; and in half

a minute more at 99°, in another half-minute at 98°—that is, three minutes and a half after the blood had begun to flow into the cup. After this the thermometer continued gradually to fall. In this experiment the evolution of caloric, under the influence of voltaic electricity, had been such as to raise the temperature of the blood four degrees. In repeating the experiment on the blood of a rabbit, whose temperature was only 96°, the galvanised blood rose three degrees. It was possible that the great difference of result observed when the blood had stood two minutes in the cup, before the introduction of the galvanic wires, and when it was allowed to flow from the vessel upon the wires, although no change in its appearance had taken place, might arise from some elastic fluid having escaped previously to the introduction of the wires in the former instance. It was ascertained, however, by an experiment detailed in the chapter just referred to, that no elastic fluid is disengaged from arterial blood under such circumstances. From the whole of the circumstances, it would appear that voltaic electricity fails to cause an evolution of caloric from blood which has been extravasated even for two minutes, for the same reason that it fails to excite the muscular fibre of an animal which has lain dead for a certain length of time; the difference of the length of time required in the two cases, only proving that the vital principle is sooner extinguished in the blood than in the muscular fibre. As we might have foreseen, it is here, as in all other instances, only where the vital principle remains that voltaic electricity can perform any of the functions of the nervous influence. On what principle could it be expected to perform those functions, unless in all respects made to operate under the same circumstances under which the nervous influence itself operates. It is not without surprise that we see so obvious a position overlooked.

It is evident, that if voltaic electricity in the preceding experiments, operate on the same principle as the nervous influence, it should occasion no evolution of caloric from venous blood, which has already undergone the operation of that influence. On repeatedly exposing venous blood to the influence of the wires, while it flowed from the vessels, no caloric was disengaged, the temperature of the blood being uniformly the same, whether the wires were employed or not.

I am, sir,
Your obedient servant,
A. P. W. PHILIP.

Cavendish-Square, March 27, 1834.

CASE OF

TUMOR IN THE MEDULLA OBLONGATA.

BY THOMAS WM. CHEVALIER,

Consulting Surgeon to the Westminster General Dispensary, and to the Royal Union Association.

MR. C. V. R., a youth of great mental vivacity and elegant figure, temperate in his habits, and of good general health, began, on the 8th of July, to complain that "*his head felt very strange.*" He had lately returned from a voyage to Calcutta, and proposed very shortly to undertake another.

There was an expression of anxious curiosity on his countenance when he made the complaint above quoted, which, though hardly sufficient to attract attention at the time, is now recalled to my remembrance, as indicative of more grave apprehensions in his own mind than he ventured to declare, or his symptoms would at that period have seemed to justify. Some such slight and almost imperceptible trait, either in the moral deportment or physical constitution, is not uncommonly the only warning we receive of impending mischief in the brain, of the most serious and even fatal nature, from a chronic cause.

This young gentleman's tongue was furred, the conjunctiva of both eyes deeply tinged yellow, and his right side slightly tender: so that, with the single exception of the anxiety denoted in his looks, his symptoms were those of an obstructed liver; and a blue pill, to be followed with a senna draught, were the medicines prescribed. The next day he called the sensation with which he was still troubled, "*a headache*;" the draughts had been partially rejected, and had acted but moderately. He had vomited a transparent fluid, as green as grass; he had cramps of the extremities; his hands were cold *alternately;*

sometimes the right hand and left foot, or *vice versâ*. His pulse small and oppressed; his tongue still furred equally on either side. He took a pill, composed of six grains of calomel and half a grain of powdered opium; and, at intervals of four hours, first a Seidlitz powder and then three grains of calomel with a quarter of a grain of opium. I also applied to the pit of the stomach a plaister, composed of one-third extra belladonnæ and two parts empl. hydrargyri; a remedy which I have often found effectual in allaying nausea.

The sickness continued, and on the third day there was a degree of drowsiness and stupor, which, if not produced by the opium, would have sufficed of itself to direct my attention to the head, and, indeed, gave rise to the remark, that "the state of the brain was the principal object of my solicitude."

As I am informed, the green vomit continued, or only ceased at intervals, for ten days; the bowels were opened with difficulty. The inattention of the patient occasionally increased to positive stupor: he staggered on attempting to walk. His thirst was intense, so that on one occasion he got out of bed in the dark (which he was then thought too weak to accomplish), and drank off some water with which the common utensils of the bedchamber had been rinsed out. On this and other grounds, rendering it sufficiently possible, he was naturally suspected of a desire to avoid going to sea again; and it became the opinion of those around him that part of his symptoms were assumed with that object. Hitherto he had not been positively delirious, nor exhibited any decided cause of alarm, with respect to the state of his brain, to the observation of Mr. A. C. Hutchison, who now attended him. Afterwards, however, we learnt from his relations, that his temper was unusually irritable, and that he was far more profane in his language than his habits might account for, while at moments he expressed his contrition for such occurrences. It would appear that he evinced also a degree of slyness and cunning quite foreign to his character under such circumstances, and a shade of unnatural antipathy toward some of his relations, which induced them to entertain apprehensions for his reason.

He had been treated with mercury as an occasional purgative, opium, effervescing draughts, blisters to the nucha and epigastrium, and bleeding, and his symptoms were so far mitigated on the morning of July 22d, that after eating a hearty breakfast, he talked of driving into the City to attend to his outfit for the East.

About noon on that day he suddenly became convulsed and insensible; his mouth was drawn to the left, and he had lost the use of his right arm. Drs. Maton and Forbes, with Mr. Hutchison and myself, attended. Our patient had both pupils largely dilated, and perfectly insensible to light. He had but little stertor; but the action of winking was arrested to a great extent. By Dr. F.'s advice, a strong cathartic clyster had already been administered; cataplasms of mustard and linseed applied to the head; his head shaved, and cold lotion applied. He was cupped to twelve ounces, and took ten grains of calomel, and an ounce of purging mixture, every two hours. In the evening he exhibited some shade of returning consciousness. He constantly examined his paralysed right hand, or any other person's, with his left, as though endeavouring to ascertain the cause of its strange appearance to his distorted vision. Tears flowed *from his right eye* when he was addressed on any affecting topic.

On the following afternoon (the 23d), the pupils were very large, perfectly insensible to light, and streaked with films from the deficient winking. His right arm continued useless; the strabismus as on the preceding day. The pulse quick, oppressed, and feeble. He was ordered to be cupped again to eight ounces; a blister was applied to the back of the head, and he took two grains of calomel every fourth hour. There appeared at this time some irritation about the genitals; his hand was frequently applied to them, as if to remove some inconvenience of clothing. The secretion of urine, however, had been suppressed, and although for twenty-four hours, hitherto without priapism. The irritation referred to attracted, therefore, but little attention.

But even at this period of the history, the symptoms pointed out a chronic disease, probably a tumor on the left side of the brain, in such a situation as to irritate the cerebellum, or medulla oblongata; while the following facts, adduced upon diligent investigation, con-

curred to render the case still more hopeless; for we learned that he had undergone a severe illness of some days at Calcutta; that he had been subject for a long while to sudden headaches, and fits of nausea, which obliged him to take a table-spoonful of brandy on the spot; that he had suffered at intervals from double vision, for three or four days previous to the fit, and expressed a firm conviction that he should not recover from his present illness.

On the 24th the secretion of urine was resumed, but he was no better. He had now continual priapism (possibly from the blisters); he appeared to be losing the use of his left arm, and approaching to a state of perfect coma. A drachm of mercurial ointment was ordered to be rubbed in, every fourth hour; the calomel to be continued; and a stimulating enema, containing two drachms of confect. rutæ comp., to be injected forthwith.

On the 25th, for the first time, his breath exhibited the mercurial fœtor; his urine was secreted abundantly, but it was strongly ammoniacal; his bowels had been opened more voluntarily; the pupils were beginning to contract under the influence of light; he spontaneously put out his tongue on seeing me approach him, and shewed symptoms of consciousness when addressed in terms of affection. In the course of the evening he recovered the use not only of his left but also of his right arm, and to some extent the power of speech. During the night he screamed violently, sometimes as if from alarm or phantasma; at others as though suffering pain. He raved continually, as if engaged with his ship at sea, and sung one or two of his sea songs. The feet were now first beginning to inflame, and rising in large blisters, from the sinapisms, although they had been taken off at the end of forty-eight hours from their first application. He seemed to recognize his brothers and sister.

On the 26th, although the strabismus of the right eye was not removed, both pupils were easily affected by light. For about a minute he knew what was said to him, giving rational and distinctly-articulate answers, but then wandering. He had complete double vision; and sometimes saw six or eight objects for one that was actually presented to him. This doubtless arose from the films of mucus which lay across the cornea, and the turgid state of the vessels of the whole conjunctiva, in consequence of his not having winked enough to keep his eyes moist and clear. His urine was intolerably strong of ammonia, so that it had excoriated the pudenda and loins. The mercurial fœtor was now very distinct. The inunction was ordered to be repeated every twelfth hour; and the pulse having assumed the character of irritation (as from mercury), although he was still suffering from effusion of the head, the use of leeches was dispensed with, a blister was applied to the top of the head, and an aperient mixture prescribed.

On the 27th, in the morning, he appeared more quiet; the saliva was dribbling from his mouth. Although the strabismus continued (as it did to the last), his pupils were restored to their natural appearance; his pulse was softer, at 108; he was thoroughly washed, and seemed much refreshed. Towards evening, however, his strength failed considerably, so that he was ordered wine, if he should be at all worse; and at six o'clock the following morning I was sent for, and found him dying. He had all the characteristics of the facies hippocratica. The heat of his body was considerably above the natural standard; there was subsultus of the right side, and the muscles of the throat were beginning to act laboriously. He took a little wine with great difficulty, and we gave up all hope. Still, however, he exhibited signs of consciousness, and shed tears again (as always) from the right eye, upon being addressed affectionately. The labour of respiration increased, and he died about eleven o'clock on the 28th, with the extremities so hot that the sealing-wax on some old parchments placed under the sheet on which he lay, was melted.

Sectio Cadaveris.

July 29th.—The vessels of the meninges cerebri were distended with blood; those of the substance of the brain were hardly more than usually injected. The corpora fimbriata were pale and flaccid; the right one containing a hydatid, the size of a small pea. The ventricles were largely dilated with transparent and colourless serum, so that the corpus callosum gave way on removing the falx, and the liquid poured out.

At the apex of the left corpus pyra-

midale was discovered a lobulated tubercle of homogeneous scirrhous structure, spheroidal in its general form, and four-tenths of an inch in diameter.

Several lines of turgid vessels, with slight appearances of extravasation, were observed within the stomach; the rest of the body was perfectly sound.

Remarks.—There are several points, in the history of this case, of important practical interest; and which, if they could be established as general pathological phenomena, would serve as invaluable rules for our conduct, not only towards patients attacked with apoplexy, but many others. To begin with that which is most easily disposed of, I would mention the application of the sinapisms to the feet on the day of the fit, the 22d of July, and their continued application for forty-eight hours; because they produced, *during that period*, no *sensible* effect. After a second interval of forty-eight hours, it was observed that the feet first became flushed and inflamed, and presently began to blister upon the soles, the heel, and the instep, to an extent which it was painful to witness. I have always observed, in the use of the mustard cataplasm, that its sensible operation on the skin, in unconscious patients, and in such as are suffering, at the time of its application, from *collapse*, is not by any means in direct proportion to the effects which it produces some days after its removal; while, even in patients who can make known their complaints, the impression that will be made by such applications is not to be calculated from the visible and palpable irritation produced by them while applied.

Besides extensive experience, much science is required to estimate, to any certainty, the consequences of an excoriating application, especially to the skin of young persons. The severest is the safest, as most likely to warn the ignorant practitioner of its imminent effects; but I have seen three children lying dead at once, from blisters kept on for twenty-four hours, because, in that time, they had not raised the cuticle; for, when they raised it, which was as long afterwards, the integuments sloughed, and they all three died of the sloughing*.

A common blister, applied to most persons for six hours only, will rise about the twelfth or eighteenth, although it has been removed at the end of the six; for the irritation, although not *sensibly* affected at that time, is really and effectually accomplished; and we may safely adopt it as a rule, that sinapisms, or any other excoriants, are not to be continued on the body according to their effect while thereon, but according to the general experience of our profession with respect to the irritation they afterwards produce, and that it is an error to continue their application after the period in which they may be reasonably expected to have insured the *future* blush, vesicle, or superficial mortification, expected from them; since, in awaiting their first notable operation on the skin, we may perhaps insure a greater degree of mischief than it is in the power of the ablest practitioner to allay or control.

It appears to me, that ignorance of this aphorism, (if I may so call the doctrine I have ventured to promulgate), and ignorance of the facts on which it rests, have led to the many cruel deaths, inflicted even in this metropolis, not long ago, on patients who were *merely suspected*, or *pretended*, to be liable to internal diseases, removeable by external irritation. In the legitimate use of unguentum antimonii tartarizati, for example, we do not rub it in till the pustules appear, but for so many minutes, in expectation of their future development, and in due time they rise; but if we persevere too long in the infriction of that ointment, of St. John Long's nostra, sinapisms, or any similar remedy, the consequence may be fatal, as has been proved by facts equally notorious and deplorable.

In the foregoing case of C. V. R., I have no reason to believe that the sinapisms were kept too long upon his feet: if it were otherwise, I would confess it; though I myself was a party to prescribe. I merely draw attention to the fact, that their *very first* effect on the skin was not until forty, or from that to forty-eight hours, after the removal; and I insist upon the principle which that fact involves,—upon the strength of innumerable cases which I could quote.

The next point to which I would request the reader's attention, is the indications of substantive disease, or, as it turned out, of *a tumor* in the brain.

The 48th number of the Medical Ga..

* After the sloughing took place, those children were under the best possible care.

zette, (August 31st, 1833), states " a loss of appetite, nausea, and vomiting, as symptoms of apoplexia hydrocephalica," (*see* p. 735); and we found that Mr. C. V. R. had these symptoms long before his last illness. To what extent the ventricles of the brain may be overloaded in any patient afflicted with those symptoms, is not material to the present *clinical paper;* but it is most worthy of observation that the patient in question did reject from his stomach pints of liquid, of the colour of pure and bright verdigris, by frequent vomiting, which nothing subdued, for *three weeks*, before he died; his death being the consequence of a tumor in the medulla oblongata.

Added to the sickness, there was stupor, eccentricity of the natural character, and intense thirst; probably all alike indicative of pressure originally commencing at the base of the brain; for we know that any injury or disease in that situation, will at last have the effect of augmenting the heat of the body; and it is not, therefore, improbable that, at an earlier stage, it may be the cause of insatiable drought. In this case, the first indication of unequal pressure within the head, and I may say the first decisive mark of impending serous apoplexy, was in the patient's complaint of *double vision*, on the 19th or 20th of the month, or perhaps earlier; in expressions of apprehension that he should lose his sight—whether of both or only of one eye, I have not been able to ascertain. The strabismus of the right eye occurring occasionally and doubtfully, as observed by his relatives, on the 20th or 21st, is the first symptom of *the left side* of the encephalon being the seat of his disease; and the paralysis of the right arm, on the 22d, the earliest assurance that that affection was a menace upon his life. It was on the 23d that the locality of his disease might be determined with some degree of precision, from the irritation spoken of above, with the paralysis of the right side of the upper extremity and head: and the priapism, toward the conclusion of the case, together with the circumstance of the body being unnaturally hot when he died, left no manner of doubt that the injury was on the left side and at the summit of the medulla spinalis.

It is not a matter of mere curiosity thus to ascertain, before the death of a patient, the seat of his disease, although it be inevitably fatal. In the present case, a lovely and promising youth, whose conduct had entitled him to praise and promotion in his ship, was, plausibly enough, suspected by his relatives of shamming illness. He was treated accordingly; and bitter had been their self-reproach if it could be supposed that they aggravated his sufferings, or hastened his death. Yet such must have been the conclusion of all, if the head had not been opened. Such might have been the permanent reflection of his friends, had I not told them, before the examination of the body (putting my finger on the spot where the tumor was afterwards discovered), " Here it will be found; and you will then know that the treatment we have pursued was adopted by us upon a sure acquaintance with the facts of the case, and if any thing could have cured him it would have done so."

The size of the tumor was *absolutely* small enough; but the situation which it occupied was exceedingly narrow, compared to the vault of the cranium above; the size of the tumor was therefore *comparatively* large, and there is reason to believe, from its history, that, if not so early a death, insanity of a most revolting type would have been the consequence of its continued slow and most gradual, but uncontrolable and malignant, growth;—

" Beatus ter et amplius."

There are several other most important considerations in this case; indeed more so than those already noticed; but from its present extension, they had better be deferred. One question, however, I would propose to the medical profession—viz. as to the degree of consciousness retained by apoplectic and comatose patients after they lose the power to manifest any. For my own part, I am induced, by a multitude of cases, to believe that the consciousness of such sufferers, and generally both their hearing and vision, is exalted, rather than deteriorated, under those circumstances. An attentive eye and heart, and a very sound mind, may be indispensible to detect it; yet what I now state as a guess, may one day be fully proved: and if it be true, how very different should be the conduct of those around an apoplectic death-bed, from what we usually see it; and that not only among inferiors who happen to be present, but as

respects professional gentlemen also. If, by any possibility, an apoplectic patient be conscious (and I can prove that some, who were wholly incapacitated to shew themselves conscious at the time of their apparent insensibility, were notwithstanding really so), how abhorrent to all our best feelings is it that the conversation in the room should be no otherwise than as if the patient were already dead, and as regardless of his extreme passion as though he were a plant!

62, Torrington-Square,
March 26, 1834.

FURTHER REMARKS ON THE CROWING INSPIRATION OF CHILDREN.

To the Editor of the Medical Gazette.

Sir,

I had little expectation, and less desire, that the views which I have advanced of the pathological nature of very many of the cases of the crowing inspiration in infants should be adopted without investigation; and I am pleased, therefore, with the observations of Mr. Roberton, in the last number of the Medical Gazette. It was long before conviction was forced upon my own mind by the reiterated cases, which, since my attention has been directed to the subject, were presented to my notice; and I have no title to expect that the result will be different with others. The prejudices of early education and impresssions are not easily eradicated; and the influence of habitual trains of thought I have found most difficult to combat. These have led to my conceding much more to the agency of those other affections, which are constantly enumerated as causes of this sonorous inspiration of infants, than my own experience would legitimately warrant. Within the last six months I have seen not less than thirteen instances of this malady. In twelve I could trace distinctly the enlargement of the glands which I have noticed; in the thirteenth, in particular positions of the neck, and by pressing the finger deep under the sterno-cleido muscle, in a direction from the trachea outwards, I could occasionally perceive two pisiform enlargements, which led me very strongly to suspect that the same morbid condition existed there. Many other instances had previously fallen under my observation, and had led me to think seriously upon the subject, though I have not recorded them, and am not, therefore, in a condition to avail myself of them. Under these circumstances, I trust, it may be considered that I have not jumped to my conclusions without "*a wider induction of facts*" than your correspondent is disposed to give me credit for. And although I have preferred direct observations upon this disease to guide me in my researches, yet that I have not neglected to fortify my opinions with the collateral aid which analogy would afford to me.

With this general "apology" for my recent papers in the Gazette, I proceed to notice, very briefly, the communication of Mr. Roberton, which, breathing, as it does, a spirit of calm inquiry, and excluding the tone of controversial discussion, deserves a reply; and I shall best consult my own convenience, as well as conciliate the interest of your readers, by confining my observations within very narrow limits.

A very brief analysis of the three cases which Mr. Roberton has related, and which do not, in his estimation, "*tend to support Dr. Ley's view of the nature of the complaint*," has led me to a different conclusion, as to their value in that particular, from that which the intelligent writer is disposed to accord to them. The first I should have no hesitation in quoting, as strongly corroborative of my opinion; the second affords no evidence in opposition; and from the third no satisfactory conclusion can be drawn. In the first, the glands were ascertained, during life, to be enlarged from a cause similar to one which I have put upon record — "an eruption of the scalp." That they appeared less after death, is the natural result of the cessation of the circulation in them, and is common to all tumors, especially those cognizable to the sight and touch; a fact strikingly exemplified by a case which occurred within my observation in the Middlesex Hospital. The disease was medullary tumor of the liver, producing immense enlargement in the epigastric and hypochondriac regions. But after death, such was the subsidence of this tumor, that some could not persuade themselves, until the interior of the body had been

examined, that it was the corpse of the same woman. It is so with the cervical glands. Although of large size during life, after death one is surprised to find them so much reduced in volume. The second case is extremely similar to one which I have related, in which, from the crowing, as there were no enlarged cervical glands, I was disposed to infer that the bronchial glands (which turned out to be the case), were diseased, in combination with a morbid condition of the lungs. Like the instance I have described, it began with hooping-cough, "from the period of getting rid of which he had shewn symptoms of the complaint;" which, moreover, destroyed the child in about the same space of time—twelve months. It is to be regretted that no examination of the body took place, which might have cleared up the difficulty. At all events we want the negative evidence that the thoracic absorbent glands were not diseased. And as this case occurred before the attention of the practitioner was directed to the subject, the cervical glands might have undergone enlargement, and yet have escaped observation; just as, in the first case, would have happened, but from the accident of meeting with my communications to the Gazette. "I now," says Mr. Roberton, "pointed out to Mr. Wilson, that the glandulæ concatenatæ on the right side of the neck were perfectly enlarged. *This I should not have noticed, had I not been recently reading Dr. Ley's papers.*" The third case presents some anomalies, which induce me largely to doubt if it were singly and simply a disease of the kind I have described; or at all events it was complicated with some other affection. The frothing at the mouth, implying an inability to swallow, together with the continuance of respiration, however irregular, and the blackness of the face, (a very unusual concomitant, as far as my experience has enabled me to observe), take this individual instance of crowing out of the ordinary range of such cases, and lead me to think this example more allied to convulsive movement than paralysis; and this spasm affecting the muscles supplied by the glosso-pharyngeal nerve, as well as the parts influenced by the superior laryngeal. Froth is, of course, but saliva retained, and entangling air in its passage to and from the mouth, as we see in true convulsions; and the blackness of the face proves that the glottis was not permanently closed during the fit, although there must have been struggles, and those vehement ones, for the recovery of the breath, just as happens in cases of submersion and of hanging. If, in the former, the individual comes frequently to the surface, and the struggles to recover the breath be violent, the face is bloated and dark; if, on the contrary, the glottis does not again open, the face is pallid; and a similar observation, *mutatis mutandis*, will apply to hanging. These are the observations which suggest themselves to my mind, from a perusal of Mr. Roberton's case. I shall next proceed to consider the remarks with which his paper concludes.

I have already said enough upon the subject of the sufficiency of the facts upon which my inductions are founded; but it is further proposed, in the mild form of an inquiry, whether the relation between the enlarged glands and the crowing is not rather one of mere coincidence than causation. I can only answer this by appealing to the general laws of medical evidence. If a given symptom attend, in twelve out of thirteen cases, a given morbid change,—if the symptom recur during the continuance of the change, and cease with its disappearance,—the most sceptical of medical reasoners will be ready to concede, that the symptom is the consequence of the change which is its cause; and it would be no great stretch of inference or induction, if, when we should meet with the symptom, we should conclude that a similar change exists, although it may be difficult in detection. But, in conformity with the general impressions upon this point, I have much limited my conclusion, leaving to the experience of future observers to enlarge or to restrict it as circumstances may demand. But, in truth, this argument of Mr. Roberton would be conclusive against the existence of *any* cause, whether head-affection, to which I have been disposed constantly to apply such reasoning, or to teething, or to distended bowels, or to faulty secretions from the liver. They all constantly occur without crowing; and the latter occurs unconnected with those states: the absence, moreover, of the symptom is infinitely more inexplicable in those cases, if *per se* they are capable of producing it, than with respect to glandular enlargement. The latter may exist to a

great degree, and yet not occasion pressure upon the mere line occupied by the recurrent nerve, which in children does not exceed the twelfth of an inch in diameter; and, moreover, glands, not constantly in contact with the nerve, may, during the varied contractions of the muscles of the neck in crying, coughing, laughing, or sudden twisting, produce at once contact and compression, when temporary asphyxia will be the result. But if cerebral excitement, or effusion—to confine myself to one illustration of the other causes—be capable of producing crowing, without any intermediate state to which the occurrence may be referred, I am at a loss to explain the fact, that hydrocephalus has gone through its whole course, even to the bursting of the cranium, without producing that symptom.

The only other point to which I have at present leisure to advert, is Mr. Roberton's very candid admission, that "if Dr. Ley can furnish examples of parallel effects upon the nerves, from mere enlargement of glands, in other parts of the body, they will be highly instructive, and tend to establish his views." I readily accept this challenge, and am prepared to shew, not only that such "parallel effects" do occur "from mere enlargement of glands," but that I have not met with a single instance in which the pressure of an absorbent gland upon a healthy nerve produced any other result. One case I have alluded to, in an early part of the paper upon the pathology of this affection, and have founded an argument upon it. It was communicated to Sir Charles Bell, and forms the first of his beautiful series of cases, in the Appendix to his work upon the Nerves, the expensive character of which, in its present form, has, it is to be regretted, much diminished the extent of its circulation, and consequent usefulness. It was an instance of "partial paralysis of the muscles on one side of the face, from the action of mercury on her mouth. The sore mouth inflaming a lymphatic gland, between the mastoid process and the angle of the jaw, compressed a branch of the seventh pair of nerves. The muscles of the face on that side were so completely paralysed, that the cheek was drawn by their antagonists, and the mouth disfigured." This single illustration might suffice, excepting that, as all tumors between the angle of the jaw and the mastoid process do not thus paralyse the nerve, Mr. Roberton may doubt the efficiency of the agent, more especially as he has described one case, in which no such effect was observed, from "an absorbent gland situated behind the angle of the jaw, and enlarged to about the size of a small field-bean." I shall on this account quote one or two other instances of the same kind, from the same work. The case communicated by Dr. Malden, of Worcester (No. xxxii.), was also an example of pressure upon the respiratory nerve of the face, occasioning paralysis, "produced by a hard, fixed, and indolent tumor, lying between the ramus of the lower jaw, and the mastoid process of the temporal bone." From the pathological researches of Dr. Abererombie, Sir Charles Bell has mentioned another instance of "twisting of the mouth, and paralysis of the orbicularis of the left eye," from "a small, hard tumor, under the ear, deeply seated betwixt the angle of the jaw and the mastoid process. It lay directly above the facial branch of the portio dura;" and (what is important) this accurate pathologist "could not discover any deviation from the healthy structure in the nerve itself." The next case, No. xlvii. and xlviii. is an example of "compression on the portio dura" from an enlarged gland, which is tender upon pressure, "just between the mastoid process and the upright portion of the lower jaw."

Such, then, are the effects of lymphatic glands, when enlarged, upon nerves which they compress. I may next mention an instance in which fits of suffocation, ending in fatal asphyxia, were the consequence, in an adult, of enlargement of the thoracic absorbent glands. "I once knew," says Cruikshank*, "an instance of these glands becoming scirrhous, and forming a considerable mass, which, being prevented by the sternum from pushing forwards, pressed backwards upon the trachea, and compressed it in such a manner that its cavity was gradually obliterating. The man was for some time incapable of the least exertion or motion, without running immediate risk of suffocation, *and actually died one morning suddenly, as he was putting on his clothes.*" The glands thus adverted to, were those in the im-

* On the Absorbent Vessels, p. 129.

mediate vicinity of the recurrent on both sides. The symptoms are not those of obstruction, threatening obliteration, of the air tube, as all will admit who are conversant with the pathological history of croup, and other affections of the trachea and larynx, but they are those of pressure upon the recurrent, whatsoever may be the cause of that pressure, which, I doubt not, was also the cause of death. In a case somewhat analogous, related by that indefatigable pathologist, Dr. Hodgkin*, in which a tendency to enlargement of the absorbent glands manifested itself in almost every part of the body, and appears to have been the prominent complaint of the patient, "*death took place very suddenly*," and, it would seem, without any previous warning. Dr. Hodgkin, with his characteristic caution, says nothing of the cause of this man's death; but "the veins of the head *and neck* were turgid, the bronchial tubes contained some thick mucus," and "the glands along the subclavian arteries, and about the roots of the bronchi, were much enlarged." These appearances lead me at least to suspect that the immediate cause of death was asphyxia; and that this was produced by the influence upon the recurrent of the enlarged thoracic absorbent glands; although it must not be omitted, that "the functions of the brain had been somewhat disturbed, and the left eye did not see so well as the right" —symptoms which were afterwards traced to thickness and opacity of the arachnoid, with infiltration of the pia mater.

But compression from other causes upon the recurrent, will also produce fatal asphyxia, by closing the glottis. This is well illustrated by an observation of M. Bourdon, who, after speaking of the effect of the division of the pneumo-gastric nerve between the superior and inferior laryngeal nerves, says, "in this too may be seen precisely in what manner the aneurism of the arch of the aorta acts upon the voice; the left recurrent nerve (which turns round that artery), is pulled and elongated by the aneurism, the voice is first altered, and occasionally at the same time suffocation occurs from it, although the aneurism shall not have burst. No one has yet tried tracheotomy under such circumstances. It has not yet been observed, whether the aneurism of the right subclavian artery produces accidents analogous to that of the aorta upon the voice*." A case, however, by Mr. Lawrence, goes far to supply this deficiency of information to which M. Bourdon alludes. In his elaborate paper upon the subject of bronchotomy, he mentions the case of a girl, under 20, who had great difficulty in drawing air into her chest, amounting to a sense of suffocation; coming on in fits, between which she was free from all complaint, her breathing and pulse being perfectly natural. She died suffocated on the night following her admission into the hospital. Her disease was aneurism of the arteria innominata, behind the first bone of the sternum, which so pressed upon the trachea as *very slightly* to indent it. Mr. Lawrence further observes, "The termination of this case is the more remarkable, inasmuch as, in another patient, an aneurism rising out of the arch of the aorta, and pressing upon the corresponding part of the trachea, so as to produce ulceration of the internal membrane, under which there was a slight appearance of coagulated blood, caused no affection of the breath at all. The discovery of the aneurismal tumor was quite accidental†."

We are left to infer, with respect to the first of these cases, that mechanical pressure upon the trachea was the cause of the death, although this tube was very slightly indented, and free from disease; yet in the instance placed in juxtaposition and contrast with it, the pressure was much greater, and the sum of disease of the trachea was very considerable. This difficulty naturally leads me to seek further for an explanation of the difference, the key to the discovery of which is afforded us by the observation of M. Bourdon, and by the experiments of Le Gallois, Majendie, Brachet, and a crowd of modern physiologists, upon the division of the pneumogastric nerves, or their recurrent branches.

In the first of Mr. Lawrence's cases, the recurrent nerve which passes immediately behind the upper part of the innominata, where its continuation is about to become the right carotid, was

* Medico-Chirurgical Transactions, vol. xvii. p. 176.

* Principes de Physiologie Medicale, 10 partie, page 638.

† Med.-Chir. Trans. vol. vi. page 227.

compressed by the aneurism; in the latter, that nerve escaped. It is well known, that the effects of compression upon a nerve are identical with those of division; and "no one is ignorant, at the present day, that the section of the nerves of the eighth pair paralyses the dilating muscles of the larynx, and that, in consequence of that paralysis, the larynx becomes closed, and presents an insurmountable obstacle to the admission of air*." In an instance, also, in which a surgeon of great eminence seems to have placed accidentally a ligature upon the recurrent, as it winds around the right subclavian, temporary asphyxia was produced; and the appearance was so alarming, that some of the assistants left the room, lest they might witness the death of the patient under the hands of the operator.

I have thus shown, in reply to Mr. Roberton, that the effect of compression by a lymphatic gland upon a healthy nerve is paralysis; and that pressure upon the recurrent, from whatever cause, may occasion asphyxia: and having thus satisfied his mind upon the general principle, I trust I may claim his suffrage in support of my opinions, which are not altogether so exclusive as he appears to assume. At all events, I have accepted his invitation to "furnish parallel effects on the nerves from mere enlargement of absorbent glands in other parts of the body;" and I am not without hope, that, in the language of your correspondent, rather than my own, "they will be instructive, and tend to establish my views."

As to the effect of abscess near the larynx, or masses of suppurating glands in the neck, these may well produce "*not periodic, but continued*," difficult, perhaps "*croupy*" breathing, and, sometimes, sudden death. These morbid conditions, however, are commonly too high to influence the recurrent, and too superficial to reach the superior laryngeal nerve; they, therefore, probably exercise their principal influence, mechanically and directly, upon the external muscles of the larynx. In the only case of sudden death from scarlatina which has fallen within my observation, the cause was obviously the rapid extension of œdematous swelling to the lips of the glottis, interfering with its movements, and obstructing its aperture. The only cases I have been able to meet with, of matter coming in contact with the par vagum or recurrent, have been in the celebrated, because the first, operation upon the common carotid for aneurism, by one of my earliest professional teachers, Sir Astley Cooper; and in an instance observed by M. Gendrin. In the former case, after death, "the aneurismal sac was found inflamed, and around the clot of blood which it contained there was *a considerable quantity of pus. The inflammation extended on the outside of the sac, along the par vagum, nearly to the basis of the skull**;" in the latter, "the nerves of the eighth pair had been *denuded by a deep-seated abscess*†;" in both a convulsive cough—in one, with, in the other, without, hooping—was the result; being similar to that observed in an experiment of Cruveilhier, "who excited a convulsive cough in dogs, in whom he irritated, mechanically, the eighth pair of nerves, to ascertain their action on digestion and respiration;" and to that noticed by Antenrieth, in cases where he found the eighth pair, and the cardiac nerves, inflamed. If nerves have not partaken of surrounding inflammation, I believe, with Bichat, they may be "bathed in pus with impunity;" if the cyst of the abscess be distinct and hard, and compress an *uninflamed* nerve, I believe that paralysis will be the consequence; but if the nerve partake of inflammation, going on in contiguous parts, experience and observation combine to prove, that excitement, characterized by an "exaltation of function," will be the result.

Having thus considered the material points in the very courteous and temperate observations of Mr. Roberton, I may be allowed to conclude with an allusion to the result of a case, detailed in one of my former communications, (that of W. C.), which ended fatally, about a week since, by suffocation. In that case, I stated my impression, from the inability to raise the eyes, that some cause of pressure, continuing from the attack of convulsions, in March 1833, probably had affected the superior recti muscles; and I think, (for I have not the number of the Medical Gazette by me), I expressed my suspicion that it

* Brachet, Recherches experimentales sur les Fonctions du Système Nerveux Ganglionare, p. 147.

* Med. Chir. Trans. v. 1, p. 8.

† Montault in Journ. Hebdomed. de Med. et Chir. tome 2, p. 73.

was not watery effusion, but either some thickening of membrane or glandular disease, towards the latter of which opinions I rather inclined. The examination of the body proved it to be "thickness and opacity of the arachnoid, with infiltration of the pia mater:" but other interesting appearances presented themselves. The sinuses, as the skull, which was beyond the average thickness, was sawed through, discharged more blood than usual; but the brain appeared to me less vascular, and the vessels upon its surface, and in its interior, less turgid than is common. In many parts there was slight opacity, with thickening of the arachnoid; and the pia mater was, in the language of Dr. Hodgkin, "infiltrated," and this in an especial degree, under the decussation of the optic nerves, and in the direction of the motores oculorum. The ventricles contained no fluid; their walls were in contact, and the choroid plexus pallid. The medulla oblongata, and the nerves, as they proceed from the respiratory column, were perfectly healthful. Towards the end of the dissection of the brain, however, a very minute quantity of fluid gathered in the depressions of the occiput, which was probably the disentangled infiltration. This was less in quantity than we found in the pericardium and in the peritoneum, and was probably merely the result of the recent, violent, and reiterated fits of asphyxia and crowing. Several glands were detected over the course of the recurrent; and upon the left side, a little above the clavicle, that nerve was imbedded in a glandular mass. The bronchial glands were not enlarged. The mesenteric glands had undergone very considerable enlargement and induration. This case bears a striking analogy to that mentioned by Dr. Hodgkin. In the one I have related, the cause of the depression of the eye-ball was thickness and opacity of the arachnoid (with infiltration of the pia mater); the cause of death was the closing of the glottis, from paralysis of the recurrent. In the latter interesting example of glandular disease, "the thickness and opacity of the arachnoid, with infiltration of the pia mater," partially paralyzed the optic nerve; but the sudden death, totally unlike that from head affection, arose also probably from a mass of enlarged glands compressing and paralyzing the recurrent.

I have only further to trespass upon your columns, by requesting insertion of a singularly acute conjecture upon the subject of spasmodic croup or acute asthma, in the fifth edition (from which I quote) of "The Principles of Midwifery," by Mr. Burns. I was not aware of the existence of this passage at the time I communicated my "observations upon the crowing inspiration" to the readers of the Medical Gazette. It is as follows:—"The recurrent of the eighth pair seems to be often chiefly affected; and when we call the disease spasmodic, we probably are often wrong, its nature being, in many instances, rather a temporary paralytic state of that nerve, or, at least, a condition unfitting it for its function; and the *modus operandi* of emetics may be to excite the nerve." Mr. Burns also subjoins the following foot-note:—"I retain the name of spasmodic croup, both because it is generally received, and as it is probable that spasm may, in certain cases, be the cause. There, however, is often a mistake made by considering the contraction of one set of muscles, produced by torpor or paralysis of the antagonists, as spasm; and it is this kind of contraction which often takes place in croup, and produces doubtless the same feeling and effects as spasm. Inaction of both sets of muscles about the glottis would have the same effect. Where the recurrent is cut, the rima closes, and the animal dies." The only observations I have to offer upon this quotation, are, that there is some obscurity about the statement of the effects of "inaction of both sets of muscles," the result of which, as conclusively established by an experiment of Magendie, who divided the four laryngeal nerves, is, the glottis remaining immoveably open; that, though conjectural, it has great ingenuity and interest; and that as far as the speculative opinion of one who deserves so highly of the profession can avail, it materially fortifies my argument. I may also mention, that Antenrieth is represented to have arrived at the conclusion, that in "spasmodic croup" "death was sometimes the consequence of the mere palsy of the parts affected."—I am, sir,

Your obedient servant,

Hugh Ley, M.D.

Halfmoon-street,
March 31, 1834.

PHYSIOLOGY OF THE VOICE.

EXPLANATION OF MR. RICHMOND'S MUSICAL PERFORMANCE.

To the Editor of the Medical Gazette.

SIR,

A VERY beautiful exhibition of a novel mode of producing musical sounds by the voice alone, was given by Mr. Richmond, on Tuesday evening, at Dr. Elliotson's, where I happened to be present. As the performance is likely to attract attention, it may be amusing to your readers to know the principle upon which it is executed.

The effects produced resemble those of the Jew's harp, when skilfully played: that is to say, two sounds are heard—a drone, or bass sound, accompanied by a succession of higher notes, resembling those of a musical snuff-box.

The first thing evident is, that the succession of higher notes is not produced in the larynx. Whenever laryngeal notes are uttered, a rise in the pitch is attended with the elevation of the larynx in the throat, as it may be ascertained by placing the finger upon the *pomum Adami*. But Mr. Richmond allowed me to keep my finger on his throat, while he warbled an air; and I found, that as long as the drone continued unchanged, the larynx did not rise or fall with the variation of the higher notes.

Again, the bass note appeared to be laryngeal; for in the same experiment, I found that when its pitch was raised, the larynx was elevated.

How, then, and where, are the higher notes produced? They are elicited out of, or rather produced upon, the bass sound, in the same manner as the higher notes of the Jew's harp. In each of the two cases the two sounds necessarily go together. Mr. Richmond is unable to produce the higher sounds without the drone, with which his performance begins.

We are indebted to Mr. Wheatstone for explaining the production of the sounds of the Jew's harp. The principle is this:—It is possible to find for every sound produced by a vibrating tongue, a chamber, or rather a volume of air, which will reciprocate a multiple of that sound. A little box, for instance, open at both ends, may be made, of such a shape and size, that when at one opening a Jew's harp is applied, and its tongue made to vibrate, the cavity will produce another and a higher sound in addition to the first, of which it is a multiple. But the mouth and fauces form a chamber of variable dimensions, or may successively assume different figures and capacities. And a person with a good ear, and organs naturally flexible, may, with a little practice, find out what shape of the fauces is necessary to reciprocate one multiple of the bass sound, what another; and gradually acquiring facility of execution, may be able, with the Jew's harp vibrating between the teeth, to present to it chambers which will reciprocate different multiples of the original sound.

Many gentlemen who were present at Dr. Elliotson's conjectured that the higher sounds were produced as in whistling, and the lower accompanying sound by the larynx; but Mr. Wheatstone, who was likewise present, explained, that the mechanism which Mr. Richmond employs in his natural organs is analogous to the mechanism which produces the tones of the Jew's harp. The only difference in the two cases is in the relative position of the vibrating tongue, and of the reciprocating cavity. In the performance on the Jew's harp, the cavity is situated behind the vibrating tongue; in Mr. Richmond's performance, the reciprocating cavity is placed before the vibrating tongues, or the vocal chords, as they are termed, of the larynx.

About three years ago I saw, at the Egyptian Hall, in company with Mr. Wheatstone, an exhibition which differed entirely from that above described, but was as surprising. The performer uttered at the same time two series of notes; neither of them was a drone, or continued bass sound; each, on the contrary, was a part in a concerted piece. The performer gave two different series of notes synchronously; or seemed, so flexible were his organs, to imitate at once the harp and clarionet. It was evident, on observing him, that the two sounds were formed in different organs; one series came from the larynx; the other came from the lips, which were pressed together; and such tension given to their edges that the air forced between them, threw them into sonorous vibration.

The observation last described gave me the clearest idea I had yet obtained

of the mode in which sound is produced in the larynx. Applying the parallel, I supposed that the vocal chords are like the lips, when pressed together, and drawn into a state of sufficient tension. I had afterwards the means of making an observation which proved the correctness of this supposition. The opportunity to which I allude was furnished by a case, which is described in a former number of the Gazette. A patient, who was under my care in the Middlesex Hospital, in the attempt to commit suicide had made an opening into the upper part of the larynx, which allowed the position of the vocal chords to be seen. I ascertained that at the time he uttered sounds, the vocal chords were in close apposition.

I am led, in part, to advert to this case on the present occasion, as the facts with which I have connected it have led me to see, more distinctly than before, the mode in which we are enabled *voluntarily to close the larynx.* In my Treatise on Physiology, I have stated, as a deduction from the corresponding structure in birds, that the human larynx is probably closed by the pressure of its sides against each other. I had, however, great difficulty in conceiving how simple pressure of membranous edges against each other would prevent the passage of air between them, when impelled by the whole force of the expiratory muscles,—a difficulty increased by my having observed, that when my patient, above referred to, voluntarily closed his larynx, the seeming approximation of the sides of the larynx was not greater than during simple vocalization.

It was the consideration that, by simple pressure of the lips together we can wholly prevent the escape of air from the mouth—and again, that we can alternate this perfect closure of the lips with that degree of tension and resistance at which air passes between the lips, and throws their edges into musical vibration—which led me to see that, the mechanism of the larynx being so parallel, the same agency, lateral pressure alone, produced by traction of membranous surfaces, when in apposition, ought to produce a like effect. In the larynx the mechanism of tension corresponds with that of the lips; while the breadth of the surfaces, which are held in contact, is even greater.—I remain, sir,

Your obedient servant,

HERBERT MAYO.

19, George-Street, Hanover-Square,
April 2, 1834.

MEDICAL GAZETTE.

Saturday, April 5, 1834.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

PRESENT ASPECT OF THE MEDICAL REFORM QUESTION.

IT is almost proverbial how slowly the mass of the public, like individuals, are moved, when they do not immediately comprehend how their interests are involved in their moving; their *vis inertiæ*, indeed, is fully proportioned to the quantity of matter to be set in motion. The fact has been illustrated in numerous instances of great concernment; and we were therefore not to be surprised at the reluctance with which the question of medical reform was entertained when first introduced to the public notice. But it is something to get matters astir; and astir undoubtedly the said question now is at last: the public attention is strongly attracted by it; and it forms a topic in the current conversation of the day.

The points which seem thus far to have made an impression on the public mind are few—but they are not without their importance. People in general now begin to be acquainted with the following facts:—1, That there *are* medical corporations; that physicians, surgeons, and general practitioners, proceed from different, distinct, and independent sources; that though they are all vulgarly called "doctors," yet that there is some difference in point of education, and other circumstances. 2, That there

is something amiss—"something rotten"—in the state of those corporations, which causes medical men to rise up and cry aloud for reform. And, 3, That, in the spirit of the age, reform is about to be accorded to the medico-political as it has been to the general political constitution.

So far so good: it is much that these fundamental facts have obtained a certain publicity; but it would be better still if they could be made to receive that due degree of attention which their interest and importance deserve. And there is a primary and essential fact which we fear has as yet little chance of obtaining any share of notice—namely, that the business of medical reform is the business of the community; that the public benefit is, or ought to be, the great end in view; and that, whatever be the issue of the question, the public will be the ultimate gainers or losers.

The popular impression on this head, we have reason to believe, is, that the reform which is destined for the profession is altogether designed for the accommodation and advantage of medical men. Short-sighted reasoners in the profession itself would seem to be of that opinion also; and we cannot, therefore, so much blame the community at large. But this is a most serious mistake, and one which is calculated to obstruct very materially the progress of medical reform. It is totally overlooked that, whatever be the private views of parties combining to dispense medical education to young aspirants, their combination must not be considered merely as a commercial or trading speculation, the object of which is to bring certain produce into the market—produce which may be bought or rejected, according to the apparent goodness or badness of its quality. Many people, no doubt, think that there is no more in an arrangement of this sort than the solution of a problem in political economy—to determine how a sufficient *supply of doctors* may be produced for the necessities of public consumption, and that it is easy to expedite the question by a reference to statistical returns;—and assuredly so it is, if the quality of the medical supply were of no more importance to the public than the quantity; if it were no matter what sort of young practitioners were *let loose* on the public, so as there were enough of them.

But it is not with intellectual produce as it is with that of trade and mercantile industry; if the latter fail in one quarter, or be of bad quality (of which most people can judge), or if it be exorbitant in its price, the evil may be abundantly remedied by the rivalry of opposition, or the working of free trade; and those who bring into the market the best articles will have the best customers. Not so, however, where the wares are of an intellectual nature; the mass of the buyers and consumers are incompetent to judge of their merits. Let the quality of the article be what it may, the great majority of the purchasers are unqualified to gauge it, while purchase they must—they have no alternative.

The results, we repeat, of whatever system of medical polity shall be adopted, must ultimately fall on the public; and this is that principle, that essential and primary fact, which we just now said must, if it were properly appreciated, constitute the life and soul of the reform question, and act as a prime mover in exciting the public to an efficient interference in the matter. It would supply a master key—a royal road—for attaining that which otherwise will only be slowly arrived at. Above all, by the light of such a principle, the mist would be dispelled which at present greatly prevents the selfish and presumptuous drift of certain projectors from being thoroughly detected. For example, when once it is understood that the medico-political arrangements

adopted with respect to medical education, deeply concern the community at large, how grossly impudent must the proposal appear, of the proprietors, who own a particular school of recent foundation, and, in some respects, of very inferior claims, to get into their own hands the exclusive privilege of supplying the country with practitioners! We know not whether we should impute it more to the good fortune or the good sense of the public, that this barefaced attempt at the establishment of a new monopoly has been hitherto resisted; but we trust that the attention which has now begun to be bestowed on the question generally, will serve to place the matter in its proper light.

Of late, we are aware that the wants of the Dissenters have been pleaded for the furtherance of the monopoly to which we allude: and some of the Dissenters themselves, we are sorry to see, have suffered themselves to be induced to press for its adoption. But they surely have many respectable doors to knock at in their difficulties, without being driven to such an alternative. If the Cambridge petition, just now presented to parliament, does not augur a speedily-approaching liberalization of both the great universities, at least, with a little patience, the dissenting body will be sure of obtaining all they can reasonably require—so far as medical degrees are concerned—by awaiting the creation of the intended National Metropolitan Board, which will be competent and fully sanctioned to bestow the highest honours in medicine.

We are tempted to speak in this pretty confident tone of the creation of such a Board (whether connected with the existing institutions or not is yet to be seen), the more especially as we see so many able advocates of the measure starting up on all sides of us. A year and a half has not elapsed since it was first broached: we take credit to ourselves for having originally suggested it to the profession*; and we know of no measure which, in an equal space of time, has secured so many firm supporters. Many who are constitutionally and habitually cautious, and slow to adopt specious projects, have fully admitted the unquestionable value of this; and many who were always violently opposed to the very semblance of innovation—and, as it would seem, indissolubly attached to the old system—have embraced this project with ardour. We do not reckon in either category the author of a clever pamphlet, entitled, "Suggestions respecting the intended Plan of Medical Reform†." He has been known for some time as a medical reformer of a very liberal caste; and his plans have been, on former occasions, as well as on this, characterized at once for their boldness and comprehensiveness. Mr. Green, however, is no leveller: on the contrary, he maintains the necessity for grades to an extent which even we are inclined to consider unreasonable; to an extent, indeed, which rather borders, we think, on self-contradiction, or, at least, on a contradiction in terms. He suggests four grades or degrees for those who would aspire to the higher ranks in their profession:—

"The *four grades* might be entitled—1. Inceptor-Graduate in Mathematical and Physical Science.—2. Graduate in Mathematical and Physical Science.—3. Inceptor-Graduate in Physiology.—4. Graduate in Physiology.

"That three of those grades, or designations, should be employed to mark the proficiency required in three stages or periods of medical education. 1. That the degree of Inceptor-Graduate" (by the way, does not this title involve an inconsistency, as if we were to say *the degree of Undergraduate?*), "in ma-

* Med. Gaz. v. xi. p. 60, and subsequently *passim*.

† By Jos. H. Green, F.R.S., Professor in King's College, &c.

thematical and physical science, should imply one adequate preliminary education. 2. The degree of Inceptor-Graduate, in physiology, should make the completion of a course of three years of professional study and proficiency in the *knowledges* required. And, 3, that the degree of Graduate in Physiology should be the guarantee of a fully-completed medical education."

These several grades, we must confess, we do not hold to be at all called for; at the same time that we are by no means disposed to maintain that there should be no ranks of honour open to the medical man after passing his first examination as a practitioner. There are, indisputably, certain ranks to which the professional man of high attainments may with advantage be promoted, when he has already distinguished himself in the ordinary practical branches of his profession; but those ranks ought to be founded on their utility—on their practical efficiency, rather than on mere theoretical distinctions connected with the pursuit of science. The metropolitan medical board itself (the *synod* of Mr. Green) might be set forth as an object to satisfy the ambition of the distinguished medical man; the professorships and lectureships also, under the new system, might tend to the same purpose; and the formation of a council of health, including an order of practitioners entrusted with the business of state medicine, might at once present encouragement to merit, and secure important services to the community.

But while we object to the formal establishment of grades—such as those suggested by Mr. Green—we must by no means be mistaken as countenancing the existence of but a single order of practitioners. To this chimerical scheme we have all along been consistently opposed; and, for all we can see or hear put forth in its favour, must still continue so. Nor indeed is our repugnance much less to an idea which seems to have caught the fancy of an influential but non-professional writer*—namely, that all medical men ought to be qualified for public practice, by passing one and the same examination. Let the medical man, says the party to whom we allude, before he is "let loose on the community," pass a certain ordeal; and after that, let him follow what line of practice he pleases, and seek distinction according as his abilities enable him. Now this is evidently the advice of one unacquainted with the history of medicine. The writer seems to think, that because medical education ought to be the same for all students up to a certain point, at that point they might undergo an examination; and, entering at once on practice, they should be equally ready for every thing medical and surgical that fell in their way—becoming physicians, surgeons, or general practitioners, just as they pleased. Nothing can be more dangerous than such doctrine, for it leads to nothing better than licensed empiricism. It is true that there must be fixed some amount of education, on the attainment of which a practitioner must be held qualified for general purposes; but to lose sight of all subsequent distinctions arising from the cultivation of particular branches of medical knowledge and practice, is equivalent to maintaining that Astley Cooper and Baillie might have been just as eminent and illustrious in reputation, if they had not confined themselves to their respective departments, but dealt indiscriminately in pharmacy, surgery, and medicine. The fact is, that in every well-regulated medical system there must be admitted grades, involving subdivision of labour; without which there would be among practitioners a sort of general acquaintance with all branches, but a perfection in none.

How highly necessary it is for practi-

* The Editor of "Times."

cal efficiency, that there should be in most occupations division and subdivision of labour, might be shewn not only by referring to the arrangements in the church and at the bar, but in the general profession of literature. We could appeal to the learned editor of the newspaper, which maintains that the passing one general examination should suffice to qualify practitioners for being "let loose" on the community, in all the different departments of medicine, whether his assistants, sub-editors, readers and all, are qualified to conduct the business of every department of his journal efficiently; and whether he does not feel the propriety of confining one to one department, another to another, and so forth,—each according to his attainments and approved qualifications, though each and all, up to a certain extent, have had the same education? If this position be true in literary matters, how much more important is it, that in a profession so widely extensive, and taking in such a multitude of objects, as medicine, there should be such an adjudication of proper persons to proper places as would render the whole the most efficient and conducive possible to the interests of the community!

We are far from having any objection to all passing the same examination in the first instance; but we hold that it ought to be open to every one who does so, to go up, after a certain time, for a higher testimonial, either in physic or surgery, as suits his views—it being understood that he then drops pharmacy, and practises as a physician or a surgeon exclusively. We do not despair of these truths reaching the cognizance of the public in due time; in fact, a little experience and reflection cannot fail to lead to their general admission.

In the list which we gave last week of those who had been examined by the Parliamentary Committee, the name of Dr. Clendinning was accidentally omitted.

WESTMINSTER MEDICAL SOCIETY.

THE FARCE.

The five-act piece which we so faithfully described a fortnight ago, excited an "extraordinary sensation" (that is the technical phrase) among the One Faculty members of the Westminster Medical Society; so much, indeed, that at their last meeting it was actually called for and formally read for the edification of all present. It was intended, of course, that it should have been received in solemn silence, and with every mark of indignation; instead of which, it gave rise to general merriment. We expected nothing less than that this rather unusual proceeding was to be followed up by a motion for putting an extinguisher on the "Medical Gazette;" or at the least for summoning the editors, publishers, printers, and devils and all, to appear at the bar of the House; nevertheless, after a good deal of fretting and fuming, one gentleman, more prudent than the rest, suggested, that if they did not take care, the five-act tragedy, which had excited them so much, would probably be followed up by an account of the *farce* they were then enacting;—a hint which answered the purpose of laying the unquiet spirits, and so the discussion dropped without leading to any result; a regular specimen of the *parturiunt montes*, &c. The quotation is rather musty.

COLLEGE OF PHYSICIANS.

The usual monthly evening meeting took place on Monday last, and was very numerously attended. A paper from the pen of Dr. Williams, containing some interesting facts connected with the treatment of Nodes, was read; but we regret that we are accidentally prevented from being able to give it till next week.

CLINICAL OBSERVATIONS ON TRAUMATIC EMPHYSEMA.

By Baron Dupuytren.

From the "Leçons Orales," published periodically, under the Baron's inspection.

Emphysema from Fractures of the Ribs, and from Laceration of the Lungs and Pleura.

On the 9th of last December, it so happened that two individuals affected with

traumatic emphysema, in different degrees of severity, made their way to this hospital. It was, I think, a beneficial circumstance, gentlemen, for your instruction; for you thus have had before you two examples of similar disease arising from different causes, and necessarily presenting you with symptoms and appearances of a varied character. One of these patients is a water-carrier, aged 68, who was struck by the shaft of a wood cart, and overturned: the wheel passed over the anterior of his chest, on the left side. The cart, though not loaded at the time, was weighty enough to cause all the mischief which I shall describe. Cough is present, and great oppression, and the pulse is frequent and full. The closest examination of the right side of the chest did not enable me to detect any lesion; but on the left there is a sharp pain towards the pit of the stomach, and that quarter is peculiarly sensible to the touch. When the hand is applied there, we perceive a crackling sensation, like that of the ends of fractured ribs, while the sides of the chest are in respiratory motion. The ear too can distinguish, besides, a noise resembling that of the fall of a multitude of drops of water following each other in rapid descent. There is likewise a considerable swelling on the same side, but without change in the colour of the skin, or any mark of inflammation—a swelling which may be displaced, and driven from one spot to another, by chasing it, in a manner, with the pressure of the hand. At the same time a well-marked crepitation is perceived, similar to that which is observed in animals blown to deprive them of their skin. These are the unquestionable symptoms both of fracture of the ribs and of emphysema produced by the infiltration of atmospheric air into the subcutaneous cellular tissue. Yet there appears to be no danger of the patient's life; the emphysema is not serious; it does not extend beyond the left side of the chest; and there is no reason to suppose that it has seized the internal organs. Experience teaches us, that in such circumscribed cases the infiltration does not exceed a few cubic inches of air, the absorption of which is easily and speedily effected. But the case is very different when the air has not only invaded the whole of the cellular tissue of the surface, but that of the viscera of the chest and belly—when there is not only infiltration, but effusion of air, into the great serous cavities; and such is the grievous condition of the other patient, of whom we shall presently speak more at large. In the case before us we must consider the concomitant lesions, which have directly led to the development of the disorder. It must have happened, beyond a doubt, that the fragments of the broken ribs having been pushed against the pulmonary organs, have torn the pleura, and probably the air-cells more, or less; whence have ensued the organic injury of the lung, and the eruption of air towards the walls of the thorax. And the small quantity of air which has strayed from its proper quarter in the present instance, gives us reason to presume that the laceration is not very considerable.

Let us now explain the mechanism of this infiltration. When, in consequence of old attacks of pleurisy or pneumonia, there remain organized adhesions between the pleuræ, and there is formed in this way a continuity of tissue between the surface of the lung and the wall of the chest, emphysema under such circumstances may be easily understood to happen: the air passes directly from the interior of the lung into the texture of the newly-organized substance, wins its way gradually, and at last reaches the fractured wall entering the subcutaneous cellular tissue. When there are no adhesions, the inspired air escapes partly by the opening on the surface of the lung, and spreads itself into the surrounding tissues and the pleural cavity; thence it proceeds, being constrained by the respiratory motions of the chest, into the cellular tissue of all the internal and external organs; so that if the air effused be considerable, it pervades not only the walls of the chest and abdomen, the upper and lower extremities, with the scrotum, neck, and head, but the pleuræ themselves, the two mediastina, the pericardium, and even the cellular tissue which unites the different organic elements of which the lungs are composed.

But to return to our patient. The man was bled upon entering the hospital; he was bled again on the following morning; compresses saturated with resolvent liquid were applied to the disordered part; and the whole trunk was enveloped in an ample bandage. This last precaution was adopted in order to suspend the respiratory action of the external muscles, and to oblige the individual to breathe by the diaphragm only—that the union of the ribs might not be interfered with, and that the spread of the emphysema might as much as possible be obviated. That the diaphragm is perfectly competent to perform this duty is plain, when, from injury done to the upper part of the spinal cord, all the external respiratory muscles have been struck with paralysis, and yet respiration proceeds.

The other patient, an older man than him of whom we have just been speaking, and of a much stronger constitution, having got into a scuffle, was thrown by his adversary. The latter, not content with his superiority, kicked his fallen antagonist

repeatedly in the chest; several of the ribs were fractured, and the fragments being violently driven inwards, inflicted severe wounds on the lung. An enormous emphysema ensued, which speedily seized the shoulder, whence it spread over the whole of the chest, anteriorly and posteriorly; thence to the neck, and finally to the abdominal region and the testicles. This man, besides, laboured under an old asthma—a very unfavourable circumstance in such a case. Now, an emphysema of this kind is always a very serious malady: the infiltration makes fearful progress,—the atmospheric air is stowed to such an extent in the organs, that breathing becomes almost impossible. I have seen several patients die from this cause—the suffocation alone; and we may well imagine how cruelly the affection must be complicated, when an asthma is present. Patients in this predicament sink rapidly; and accordingly we could only despair of this old man's life. You saw his severe suffering,—his vain attempts to utter a few syllables in reply to our questions. Upon every inspiration a new column of air was added to the accumulation. The expectoration was bloody,—shewing that there was a deep injury of the lungs; the pulse was small, concentrated, and convulsive. We could do nothing for him. To bandage him, as we did the other patient, would only have added to his tortures. Incisions were proposed, and we made a certain number of them on different parts of the body, rather, however, in compliance with the suggestions made to us, than with the hope of obtaining any useful result; and so they proved, for reasons which it is needless to explain. Were it an emphysema, however, of no great extent, and circumscribed, incisions of the parts chiefly engaged might be productive of benefit, by opening a passage for the egress of the collected air: but in such a case as the present the insufficiency of such a resource cannot but be too apparent.

Well, the patient died; and, upon examining the body, all the appearances that were anticipated were observed. The anterior and posterior mediastina contained a great quantity of air, as did also the whole extent of the interlobular tissue. Three ribs on the right side were broken, and the corresponding portion of the lung presented a large and deep laceration.

Our first patient, on the other hand, went on improving. In the course of five days, he no longer felt any pain in the affected side when he coughed; the expectoration was not at all bloody; the infiltration had nearly disappeared; the pulse was good; the respiration little impeded; and his appetite began to return: in a word, he is in the way of complete recovery.

In both patients we see how important a part of their malady the emphysema constituted: it was the most manifest symptom, and was carried to such an extent in one, that, independently of the serious lesions, of which it was the result, it formed of itself a grievous disease. In some cases, it only makes its appearance among the secondary symptoms, and is not readily recognized; not that it is of so much importance to recognize it, in order to oppose its progress, but being recognized, it enables us clearly to appreciate the concomitant disorders.

Emphysema, with complicated injury of the chest.—Death.

A man, aged 41, of a strong constitution, was jammed up against a wall by the pole of a carriage. When taken to the Hôtel Dieu, it was observed that his breathing was exceedingly short and laborious: the sternum was transversely fractured at the junction of the two upper thirds with the lower, and the upper fragment was deeply sunk into the mediastinum. On the right side of the chest, the fourth, fifth, and sixth ribs, at about four or five fingers' breadth from the sternum, were much depressed, and a crepitus was heard, which evidently indicated their fracture, and perhaps that of their cartilages. A little below the seat of these fractures there was an ecchymosis, about the size of a five franc-piece. The features of the patient were marked with profound anxiety: his answers were short and impeded; his pulse rapid, and almost imperceptible; his skin cold. By feeling in front of the fractures, a crepitation was perceived, resembling that produced by air traversing the cells of the subcutaneous tissue: but, what was most remarkable, every time inspiration took place, the tumor increased considerably, and extended from the lower part of the sternum to the seat of the fracture; it diminished, on the contrary, when expiration took place. The least pressure gave rise to the emphysematous crepitation, and was sufficient to reduce the tumor. In the two inferior thirds of the right cavity of the chest the stethoscope detected a very distinct *gargouillement*. The patient was bled; compresses were applied to the seat of the emphysema, and a bandage was put round the trunk. At first every thing seemed to go on well under this treatment,—the pain was greatly alleviated; but on the fourth day, all of a sudden, the oppression was redoubled; the cheeks assumed a hectic blush; the pulse became small, hard, and quick. Another bleeding was employed. Ten days thus passed over, with

alternations of better and worse, as to the suffocation; but, by the end of that time, no trace of the emphysema remained: a large and very black ecchymosis, however, extended from the base of the chest to the upper and exterior part of the thigh. The chest became more and more embarrassed, and on the twelfth day the man died. On examining the body, old and very strong adhesions were found between the pleuræ; the upper fragment of the sternum was fixed to the pericardium; the right ventricle of the heart was torn by it, to the extent of two-thirds of its thickness. A great quantity of bloody serum, and of pure black blood, was found in the pleural cavity of the right side. The fourth, fifth, and sixth ribs were fractured, and their cartilages detached from the sternum: between these there was a lacerated opening, large enough to suffer a finger to go through it.

Traumatic emphysema of the eye-lids.

The introduction of air into the subcutaneous or intermuscular tissue is not peculiar to penetrating wounds of the chest; it may take place in any of the parts connected with the respiratory system. Emphysema of the eye-lids is by no means rare: it is mentioned by several authors, and we have had more than one case of it in the Hôtel Dieu.

CASE I.—*From supposed Fracture of the Os Unguis.*

An excavator was struck on the forepart of the head, neck, and chest, by a quantity of earth that fell upon him. When he disengaged himself, he only complained of a slight pain at the root of the nose, to which he paid but little attention, and went on with his work. About a quarter of an hour after, on blowing his nose, a considerable tumefaction of the eye-lids immediately ensued: the whole eye was covered up by it. Was this erysipelas? The skin, indeed, was bright and tense, as in that complaint; but there was neither that redness nor heat which characterize it. The eye-lids retained their natural colour and ordinary temperature. Was it œdema? Œdema is never formed so rapidly; and, on the other hand, that *empâtement* of the tissues is not observable in it which is peculiar to serous infiltration. Was it an effusion of blood in consequence of some violent contusion from the blow he received? But this would have been recognized by the presence of numerous violet-coloured ecchymoses, and by a brownish hue, more or less deep, of the eye-lids. On the whole, we could not but suspect that it was an infiltration of air; in fact, upon touching the parts, emphysematous crepitation was perceived, not at a single point, but throughout their whole extent. In such cases, whenever incisions are made in the skin, a certain quantity of air makes its escape. On the bodies of individuals who have died either from the severity of the emphysema itself or the injury of the other organs involved, the presence of air has been demonstrated, just as crepitation has been noticed during life. Having had to treat an individual in whom this phenomenon was very manifest, we were induced to make some incisions on the part engaged, upon which a considerable quantity of air issued forth. In short, there can be no doubt of the fact: the only question is, in the present case, how to account for the infiltration. Now this, I think, must have happened from the blow received on the face having ruptured the os unguis of the ethmoid, through which rupture the air made its way from the nasal fossæ into the interior of the eye-lids. It was remarkable that the emphysema did not occur immediately after the accident, but only after the lapse of some time, and subsequent to the patient's effort in blowing his nose. The reason of this is explained without much difficulty. The fracture of the bone did not immediately lead to the disruption of the soft parts, and the latter accordingly still excluded the air; but as soon as the patient forced a column of air against them in his effort, they gave way, and the communication was thus established between the nasal fossæ and the eye-lids. We were desirous of ascertaining whether any drops of blood were shed by the nose; for in all the other cases of the kind that I have met with this was an invariable symptom. But it does not appear that it occurred or was noticed in the present instance.

The treatment consisted in general bleeding and the topical application of compresses, steeped in resolvent solution. The patient was recommended to use no effort in attempting to blow his nose, to avoid coughing as much as possible, and to do nothing that would tend to open afresh the unnatural passage of the air. On the third day the crepitation had much diminished; on the fourth it was almost insensible; and on the fifth the eye-lids were nearly in their natural condition. The following case is one that bears a strong similarity to that just related.

CASE II.—*From supposed Rupture of the Pituitary Membrane.*

A young man, who received a violent blow from a plank striking him on the nose, felt at first only the pain which might be expected; but in the course of a few hours, upon blowing his nose, he perceived, as it were, a line of fire passing

from the side of the nostril to the angle of the eye, and thence spreading itself into the two lids of the left eye. Such a swelling immediately ensued, that the eye was quite closed up, and the passage of the luminous rays intercepted. The patient was received in the Hôtel Dieu. The lids were found much distended and resisting, but indolent, and without any change in the colour of the skin. Emphysematous crepitation was very distinct. The same treatment as in the preceding case brought about a cure in four or five days. There is every reason to believe that the malady in this instance resulted from a rupture of the pituitary gland opposite the union of the lateral cartilage of the nose, which had probably become somewhat disunited from the bone.

Case III.—*Emphysema of the Temples from Fracture of the Frontal Sinus.*

A man received a fall on the fore part of his forehead. Some time after, a tumor of considerable bulk made its appearance in the temporal region. The nature of this tumor seemed unaccountable to some persons who saw it; but when it was slightly compressed, it might be forced to pass to the front of the forehead, and then to disappear. It was owing to the passage of the air into the surrounding cellular tissue, and that in consequence of fracture of the frontal sinus, which created an opening under the skin.

We might readily multiply examples of traumatic emphysema resulting from penetrating wounds of the chest, or from a communication of some other kind, forcibly formed with the air passages. But those already cited, with the remarks we have made, may probably be sufficient to give you precise notions of this curious malady, and of its diagnosis, and the treatment it requires. With respect, however, to the infiltration which sometimes takes place in some of the highest points of the respiratory system, a few words may be added. Beside the crepitation, called emphysematous, and the negative signs already noticed, it may be still farther ascertained, when there is doubt about the nature of the disorder, by getting the patient to blow his nose, when a manifest augmentation of the tumor of the eye-lids, or other parts affected, will be perceivable. If the patient be in such a state as to be unconscious of what is required of him, by compressing his nostrils an instantaneous increase of the tumor will be observed, owing, no doubt, to the column of expired air, which, not finding an egress by the natural way, is carried into the one newly established by the accidental injury. In every case of emphysema of the eye-lids, the swelling takes place with rapidity—a circumstance easily accounted for by the great looseness of the cellular tissue of which they are composed.

HOPITAL DE LA PITIÉ, PARIS.

Clinique of M. Louis.

Poisoning with Sulphuric Acid—Death after two months' suffering—Postmortem appearances.

A carman, aged 47, of a strong constitution, gave himself up to excessive drinking on the first day of the year (1834): he drank not less than five or six bottles of wine, and got home in a state of complete intoxication. On the following day he renewed his dissipation, and was returning towards home, when he spied a phial on the table of a limonadiere. It contained vitriol, with which it is not unusual, in the streets of Paris, to acidulate the beverage of the lovers of lemonade. The man seized the phial, thinking it contained liqueur: he drank it off, and was quickly attacked with dreadful pains in his throat and stomach. Milk was abundantly administered to him, and copious vomiting ensued; but this treatment did not much avail: he was growing worse when taken to the hospital, fourteen hours after the accident.

The symptoms noted at his entrance were—sharp pains in the throat, along the track of the œsophagus, and in the pit of the stomach; bloody sputa, numerous eschars on the lining of the mouth, and an inability even to move the lips without great difficulty; the pulse small, regular—100; the skin moderately warm, covered occasionally with cold clammy sweats. Ordered magnesia and water, to neutralize any remains of the poison; thirty-six leeches to the abdomen. After two days the pulse was no longer accelerated. The pains, however, continued, and the mucous membrane of the mouth was still inflamed. The fauces could not be examined, in consequence of the difficulty of opening the mouth. The sputa extremely abundant; deglutition greatly impeded; and the patient can only introduce into his stomach some spoonsful of coffee and gum-water.

For two months these symptoms continued. The patient had no headache all the time, nor acceleration of pulse. His thirst was considerable, and he spat a great deal. The bowels were habitually confined; and there were occasionally

pains in the loins and back, which were increased by pressure. During the last three days of the patient's life there was much sinking, the spitting decreased, and black liquid matter was passed from the bowels. It was thought that the colour of the evacuation was owing to the presence of blood; but chemical analysis did not detect a particle of blood in it.

The patient died on the 2d of March.

Examination of the body.—The ulcerations of the mouth are cicatrized; the surface of the pharynx wrinkled, uneven, with many ulcerated spots upon it. The œsophagus adheres closely to the parts with which it is in contact, the cellular tissue between them being indurated and thickened. The mucous membrane of the œsophagus entirely destroyed; the passage itself containing three principal ulcerations and a perforation. The first of these ulcerations is situated at the upper part; towards the middle third the perforation is found, together with a mass of brownish liquid substance, contained in a pouch formed between the lung and the œsophagus. The other ulcerations are near the cardiac orifice. In some spots the muscular coat is wholly destroyed, in others it is remarkably thickened: so that it can readily be conceived how difficult it must have been to swallow.

The changes observed in the stomach are not less deep and numerous than those just examined in the œsophagus. The organ itself is of a moderate size, but altered in shape. On cutting into it, in the direction of the greater curvature, and throwing it open, it looks like a kidney divided from the exterior. The upper part of the mucous membrane is of a bright colour, the lower of a bluish tint. Along the lesser curvature there are several ulcerations, irregularly rounded: in some places the mucous membrane is completely gelatinized, in others its consistence is scarcely altered. In the lower part there is found a sheath, of a bistre colour, floating within the stomach; one of its sides is a polished surface, the other presents true villi. There is every reason to believe that this sheath is the mucous lining of the œsophagus, which was found absent altogether in the passage.

At what period did this membrane become disengaged? How is it that the digestive powers did not act upon it? It is difficult to give an opinion; and accordingly M. Louis has his doubts respecting the nature of this anormal substance found within the stomach. Nothing remarkable was noticed about the other abdominal viscera.

The patient, during the whole time he was in the hospital, exhibited no symptoms of febrile excitement, if we except that during the first two days his pulse was about 100, and this, though the disturbance of the system was very great.

M. Louis laid great stress on this circumstance, and shewed how very different was the reaction arising from internal inflammation, from what it is when the cause is external. Thus, if a large blister be applied to the chest, in the treatment of pneumonia, the febrile excitement often will not be augmented; but let a little erysipelas seize a small portion of the face, we shall commonly find the pulse raised to above 120, and kept there for some days. There is more in maladies than meets the eye; and we must go farther than the morbid appearances which the scalpel discloses if we want to find out the cause of the symptoms which different diseases present *.

Clinique of M. Clement.

Pregnancy of one of the Fallopian Tubes—Latent Hæmorrhage—Death—Morbid Appearances.

The following case is remarkable for the rapidity with which it proved fatal, the patient dying in a few hours in consequence of a hæmorrhage produced by rupture of the left fallopian tube—yet most of the phenomena appearing to result from other causes.

Elizabeth Guerin, a sempstress, aged 30, of a strong habit, residing in Paris only for a few months, was taken into the hospital on the 19th February, at half-past eleven at night, and was dead before morning. Soon after her arrival her symptoms were noted as follows:—Shiverings; pain and tension of the belly; the least movement, or pressure of the abdomen, causing her to utter piercing cries; nausea; hiccup; anxiety; painful, hurried respiration; cold sweats; fainting; general paleness; expression haggard and fixed; and the pulse small, sharp, and quick. Subsequently, the limbs growing cold, and the intellectual faculties failing, life soon became extinct.

During this rapid succession of symptoms, the patient was unable to take any thing, except a few spoonfuls of sweetened gum-water. It was understood that she had been complaining only about three hours before she came to the hospital; and had previously been in excellent health.

Examination 21 hours after death.—Upon opening the abdominal cavity, more than three litres of clear bright blood escaped.

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* Gazette des Hôpitaux.

In the pelvis there was an enormous black clot, completely filling the eavity, and extending into the iliac fossæ. No doubt could be entertained of the immediate cause of death; but it was desirable to seek the vessel which had been ruptured. A careful examination was accordingly instituted; and the result was, that the left uterine tube was found to have been distended by a tumor, and to have given way within about an inch and a half of the uterus.

The tumor was the size of a pigeon's egg, somewhat irregular in shape, of a reddish brown colour, and it had on its most prominent part a laceration of about two lines in breadth. The opening was enlarged by delicate disseetion, and at the depth of about three lines there was discovered a transparent poueh, eontaining a liquid nearly eolourless, and very distinctly floating in it a fœtus of about the sixth or seventh week of gestation. The head could be clearly distinguished, and the curved trunk, and the tubercles of the members. The uterus itself was nearly twice its natural size; its walls were thickened, softened, and reddish. Its cavity contained a tolerably dense tissue, resembling in colour and other respeets portions of torn placenta. This was the tissue that Chaussier so well deseribed as being invariably formed in fallopian pregnancies, and which he eonsidered as a true decidua or epichorion.

The ovaries were well developed. Nothing remarkable was observed about the other viscera, either of the chest or abdomen.—*Gaz. des Hôpitaux.*

M. DUPUYTREN'S RETURN.

The eminent surgeon of the Hôtel Dieu has returned to Paris, perfectly recovered, it is said, from his tour in Italy. On the 25th ult. he made his appearance in the Académie de Médecine, when he was received with congratulations by the assembly. On his retiring, M. Louyer-Villermay moved that two of the members should be deputed to transmit the *félicitations* of the Académie to the illustrious Baron, which was accordingly done.

LITERARY INTELLIGENCE.

The Work about to appear, by Mr. James Augustus St. John, on the Present Condition of Egypt, will contain an Account of the Madhouse of Cairo, the Medical School, Hospital, and Dissecting-Rooms.

A Translation of the last Edition of M. Billard's Treatise upon the Diseases of Children. By J. R. W. Vase, M.D. with Notes and Illustrations.

WEEKLY ACCOUNT of BURIALS,

From Bills of Mortality, *April* 1, 1834.

Age and Debility	24	Hooping-Cough	3
Apoplexy	4	Inflammation	23
Asthma	6	Bowels & Stomach	2
Cancer	1	Brain	1
Childbirth	3	Lungs and Pleura	5
Consumption	40	Jaundice	1
Convulsions	14	Liver, diseased	5
Croup	2	Measles	5
Dentition or Teething	2	Mortification	1
Dropsy	6	Paralysis	1
Dropsy on the Brain	9	Small-Pox	2
Dropsy on the Chest	1	Sore Throat and Quinsey	2
Fever	3	Thrush	1
Fever, Scarlet	2	Venereal	1
Fever, Typhus	2	Unknown Causes	2
Gout	1		
Hæmorrhage	1	Stillborn	14
Heart, diseased	1		

Decrease of Burials, as compared with the preceding week . . . 158

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

Mar. 1834.	Thermometer.		Barometer.	
Thursday . 20	from 33	to 46	30·39	to 30 38
Friday . . 21	24	47	30 35	30·24
Saturday . 22	39	49	30 16	30 04
Sunday . . 23	37	51	29 90	29·70
Monday . . 24	39	49	29·68	29·73
Tuesday . . 25	31	44	29 79	29·85
Wednesday 26	24	45	29 99	29·90

Wind variable, N.W. prevailing.

Generally cloudy; rain at times. A few flakes of snow in the afternoon of the 25th.

Rain fallen, ·1 of an inch.

Thursday . 27	from 33	to 57	29·80	to 29·71
Friday. . . 28	38	49	29·63	29·58
Saturday . 29	39	53	29 56	29·66
Sunday . . 30	31	49	29·74	29·86
Monday . . 31	30	48	29·93	29·99
April.				
Tuesday . 1	33	49	29·98	29·91
Wednesday 2	40	53	30 01	30·09

Prevailing wind, S.W.

Except the 29th and three following days, cloudy, with rain at times.

Rain fallen, ·175 of an inch.

Charles Henry Adams.

NOTICES.

We perceive that Dr. M. Hall has opened a correspondence with Mr. Wakley, in an attack upon this journal. Of all the correspondents we ever had, we will do the worthy doctor the justice to say, that he has been the most troublesome. We sincerely congratulate our contemporary on his acquisition.

Mr. Weiss's letter cannot be inserted, exeept as an advertisement on the wrapper.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, APRIL 12, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

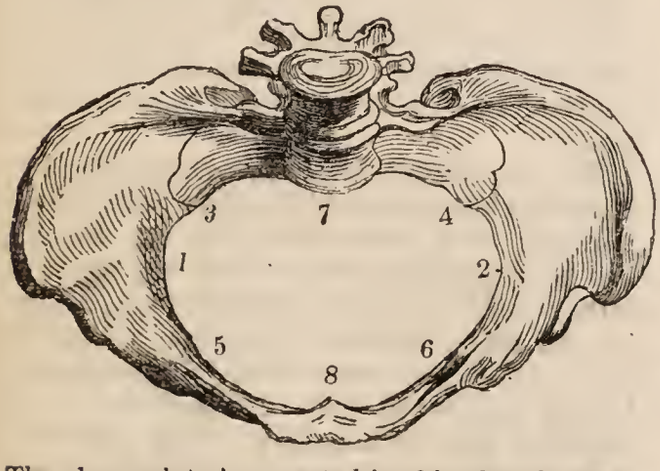
By Francis H. Ramsbotham, M.D.

Lecture XXVII.

LABOUR.

IRREGULARITIES OF HEAD PRESENTATION.

Although according to the arrangement which we have chosen, all varieties of head presentation are considered natural, still, as some are of very infrequent occurrence, they may be regarded as *irregularities;* and under that term I shall proceed to describe them. Having, then, fully considered the mechanism of parturition under the most usual positions of the head, when the face turns into the hollow of the sacrum, previously to its expulsion; and having learned that this partial rotation occurs in all the four first divisions; it naturally follows that we should next give our attention to the other four cases of vertex presentation.



The above plate is repeated in this place because the arrangement regarding the positions of the head followed in these lectures differs considerably from Baudelocque's, which has generally been adopted; and to obviate the inconvenience of referring to another volume. The figures indicate the direction of the face as regards the pelvis; the vertex being the presenting part.

It is of great importance that we should learn to distinguish the different parts of the head one from another early in practice; since, when irregularly situated, by a very slight degree of exertion, and the employment of very simple means, we may often place it in the most favourable of all positions, and convert what would have been a tedious and difficult labour into one of the most ordinary character.

VERTEX PRESENTATION, WITH THE FACE BEHIND EITHER GROIN.

It must be evident to you all, when I introduce this fœtal cranium into the pelvis which I hold in my hand, in the fifth and sixth positions, according to my arrangement—that is, with the face situated behind either of the groins—that the head is by no means so well adapted to the cavity as when the face is directed to the side of the pelvis, or looks diagonally backwards; and this want of accommodation it is which generally induces a somewhat lingering labour under the irregularity before us; and which often obliges us to have recourse to instrumental aid.

But although a tedious case may be anticipated under this malposition;—although the sufferings may be greater, and the time of duration more protracted than is usual;—the mere irregularity of situation is not of itself sufficient to warrant us in terminating the case by artificial means. We are not to interfere instrumentally because the face is placed anteriorly; but we must wait till some symptoms appear which call imperatively for relief and assistance. It matters not whether the face is looking backwards or forwards,—to one side or the other,—provided such symptoms arise as indicate danger, it is our duty not to allow them to become

aggravated, but to deliver the patient by those means which are least likely to do injury. Most writers agree—and this observation accords entirely with my experience—that the fifth and sixth positions of the head, as just described, are of rare occurrence; but Professor Naegelè, of Heidelberg, in his essay on the Mechanism of Parturition, (a translation of which was four or five years since put forth by Dr. Rigby), states that this sixth variety of vertex-situation is by far the most frequent of head presentations, after that under which the face looks originally to the right sacro-iliac synchondrosis.

When the head enters the pelvic brim with the face either to the right or left groin, it passes down into the cavity in the same direction, until the occiput comes to press upon the lower edge of the sacro-sciatic ligaments, and the forehead on the inner surface of the ischium. Since, however, here also the long diameter of the head is opposed to one of the short diameters of the outlet, it cannot pass out without a change being effected; a turn is consequently made exactly the reverse of what occurs when the face is directed backwards—namely, with the occiput into the hollow of the sacrum, and the forehead under the arch of the pubes. To effect delivery, however, in this manner, it is requisite that the proportions of the pelvis should be large in reference to the size of the head; and we cannot, therefore, be surprised at the difficulty we observe in its transmission. On the expulsion of the head, the shoulders are directed through the outlet obliquely, and the body passes with the abdomen appearing under the pubes, instead of backwards, as in the more ordinary cases.

This is the course we may expect the head to follow; but from many cases which I have watched narrowly, I am persuaded that not unfrequently, instead of this quarter turn of the half pelvis being taken, so that the occiput shall be thrown into the hollow of the sacrum, a three quarter turn is effected; the face gradually occupies the sacral cavity, and makes its exit, sweeping the perineum, while the occiput peeps up under the arch of the pubes. Baudelocque mentions this change as "sometimes, but unfortunately for the woman too seldom, happening." But Naegelè assures us, he is "*thoroughly convinced*" that in the cases which I have considered the fifth and sixth varieties, the occiput is not usually turned into the hollow of the sacrum, but escapes under the arch of the pubes; and that "this change in position requires no peculiarly favourable circumstances; but that these species of labours can be completed by the natural powers under the most usual proportions, in the same time, with the same expense of strength, and without greater difficulty, than when the head takes the most common position." He states, also, that out of ninety-six cases in which the face presented towards the left groin, (which he observed with particular care, and described in his note-book), in three instances only the head cleared the passages with the face directed anteriorly; and in all these three cases there were some peculiarities in the structure of the head, to which he seems to attribute the forward inclination of the face. I have already stated, that this is contrary to the commonly-received opinions, and does not coincide with my observation, as far as I have been able to judge. The remark, however, is highly worthy of attention, as emanating from an observant and practical man.

Mode of detection.—It is not very probable that this malposition will be distinguished before the membranes break; because, as the vertex presents, the posterior fontanelle will first offer itself to the finger, and it will be difficult to detect the course which the different sutures take thus early in the labour. Besides which, I have already advised you, after you have positively satisfied yourselves that the head is the presenting part, not to endeavour to gain further information respecting the mode of position while the membranous cyst remains entire; partly because of the difficulty of doing so, but principally because of the danger of inadvertently evacuating the liquor amnii prematurely. After the second stage, however, has commenced, when the expulsive pains are well established, we shall probably find that the head does not descend with the same ease and regularity that we observed in the patient's previous labours; and on making as accurate an examination as we can, to ascertain the cause of the delay, we shall detect the posterior fontanelle at the back part of the pelvis, against one or other of the sacro-iliac junctions, and we shall be able to trace the sagittal suture running upwards and forwards, to terminate in the large diamond-shaped open space—the anterior fontanelle—situated behind the opposite groin.

Being assured that the head occupies this situation, I would strongly recommend you not to interfere early in the labour, but to wait in the hope and expectation, either that it will be expelled naturally, in the manner most frequently observed, — or that the face will be gradually turned backwards into the hollow of the sacrum, and eventually make its exit, sweeping the perineum. Presuming, however, that after a number of tolerably strong expulsive pains, no advance takes

place in the situation of the head, it will then be proper to introduce the three first fingers and the thumb of the right or left hand, and to give the face an inclination to the right or left ilium, according as its original direction was to the right or left groin: and this attempt must be made in the absence of uterine contraction, and before the head has become locked in the pelvic cavity; for if it be delayed until a state of impaction has occurred, the malposition cannot be remedied by the power of the hand alone, and instruments will most likely be required, in order to finish the delivery. In making this change in the position of the head, it is not right to turn the face at once into the hollow of the sacrum, even if it could be accomplished; because the probability is, that the child's body, being held tight within the contracted uterus, will not follow the sweep which the head describes; and we incur great danger of injuring the neck. All that we are required to do, is to incline the face to one of the ilia, and leave the rest of the process to nature.

Face directed to the promontory of sacrum, or the symphysis pubis.—It is very rarely that the head offers itself, at the very commencement of labour, above the brim of the pelvis, with the face directed either against the promontory of the sacrum or the symphysis pubis; so rarely, indeed, that some practitioners of great respectability have denied the possibility of its occurrence. From my own observation, however, I am perfectly satisfied that such a presentation occasionally does take place.

Under this position the head is placed with its longest diameter in the direction of the shortest diameter of the pelvic brim; and if the head and the pelvis be of average dimensions, it is impossible for the head to occupy the cavity, unless a change in situation either occur spontaneously, or be effected artificially, as you see perfectly exemplified in the machine before you. No power that could be employed would force the cranium through the superior strait, while so placed; whereas we have already had an opportunity of observing how easily the same head enters into the cavity, when the face is turned in the least degree to either side.

The vertex, then, is observed to lie quite above the brim of the pelvis, almost out of the reach of the finger as introduced in a common examination; nevertheless, it will most likely be detected even before the membranes rupture, if two fingers of the left hand be carried high up into the vagina. But although, from the difficulty in feeling the presenting part, suspicion may be excited that the position is irregular, the peculiar nature of that irregularity will probably not be determined until after the membranous cyst has given way. On the second stage of labour, however, having commenced,—following the general directions before laid down,—it is right that an accurate examination of the head, and of its bearings in relation to the pelvis, be made, and we shall find the sagittal suture running from before directly backwards, and not laterally or diagonally. We may then be assured that the face is looking either towards the promontory of the sacrum or symphysis pubis, and our indication will be afforded by the situation of the anterior fontanelle. In relation as this fontanelle is directed backwards or forwards, so will the face be situated.

I have little doubt that this malposition is in many instances rectified by nature herself; that—the force of the uterine contractions being resisted, by the approximation of the pelvic bones in their conjugate diameter not affording due and proportionate space for the descent of the head thus situated—the mechanical impediment offered induces a turn, with the face to one or other side, on the same principles as regulate the turn which is observed to occur in all natural cases, just previously to the escape of the head externally. I see no more difficulty in believing that such a change of position is likely to occur at the upper than at the lower pelvic aperture.

Being satisfied, then, of the situation of the head after the membranes have broken, having watched the effect of two or three pains, and observing that it evinces no disposition to accommodate itself to the dimensions of the pelvic brim, it is proper,—lest the woman become worn out by inefficient struggles, and lest the cranium become wedged in this unfortunate position,—to follow nature's dictates, and incline the face laterally. This alteration in situation it would not be difficult to effect, by grasping the head between the three first fingers and thumb introduced into the vagina, provided the os uteri were well dilated, the vagina and perineum sufficiently relaxed, and the head remained above the brim, perfectly moveable, free, and unimpacted. On this slight alteration being made, the head will enter the pelvis, all the difficulty will be over, and the case will be reduced to one of the most ordinary character. If, however, we cannot accomplish this object, let us then be guided by the general rule, to which there is no exception—that of waiting till either the lapse of time, or symptoms of danger, require instrumental interference.

BROW PRESENTATION.

Other parts of the head besides the vertex may present. The anterior fon-

tanelle, or brow, may be the depending part; and under this presentation the face may offer itself at the pelvic brim, looking to one ilium or the other—obliquely backwards to either sacro-iliac synchondrosis—obliquely forwards to either of the groins—directly forwards to the symphysis pubis—or directly backwards towards the promontory of the sacrum; in the same manner, indeed, as though the vertex presented.

Under either of these malpositions the head is still less adapted to the passage than when the vertex presents with the face forwards, since much greater space is required for its transmission. You may observe, by the oval apertures in this piece of thick leather, cut for the purpose of illustrating this position, that the same cranium, when the brow or face is directed first, requires a space of nearly an inch more in the longest diameter, than when the vertex is protruded.

Brow presentation; the face looking diagonally backwards.—It is not often that the anterior fontanelle is found to be the presenting part, while the face looks either directly to one ilium or the other, or diagonally backwards, to either sacro-iliac symphysis; because, under these situations particularly—as well indeed as under every other variety of instance in which the brow offers itself,—there is a natural inclination to convert the case into a perfect face presentation; and this is owing to the fibres of the fundus uteri being strongly exerted upon the fœtal body; under which action the shoulders are pressed downwards, the chin is gradually separated more and more from the chest, and the head is expelled in the manner presently to be mentioned.

It is very probable that the presentation of the anterior fontanelle may be detected before the membranes rupture, because of the large space it offers to the finger: but even should this information be obtained thus early, little can be done towards rectifying the position before the membranes break; because, under any attempts we might use, we should almost unavoidably destroy the integrity of the membranous cyst, which it is of such essential importance to preserve whole. It is necessary that the case should be watched carefully and narrowly; and that, on the rupture of the bag, an accurate examination should be instituted, with the view of determining whether the face lie forward, backward, or laterally. In the case now under consideration, the sagittal suture, which becomes our guide, is traced from the anterior fontanelle running obliquely forwards and upwards to one groin or the other. There can then exist no doubt, first, that the anterior fontanelle presents; and, secondly, that the face is placed backwards in relation to the pelvis.

Inasmuch as a considerably greater space is required under the presentation of the brow than when the vertex depends; inasmuch as there exists such a disposition to convert the case into a face presentation; and inasmuch as the labour is usually protracted, and attended with a proportionably increased degree of pain; it would naturally follow that we should endeavour to place the head in a more favourable position, by throwing the chin more upon the chest, and causing the vertex to descend; provided this could be accomplished without incurring danger, without any aggravation of suffering, and without the formidable appearance of preparing for an operation. This object can frequently be gained, if the position be detected soon after the rupture of the membranes, and before the head has perfectly engaged in the pelvic cavity, by a very simple and easy method: it only requires that steady pressure should be made upon the brow during the urgency of pain, so that the forehead may be arrested at that spot, and the powers of the uterus expended upon the back part of the head. It is then usually observed that the head is bent forwards on the neck, as on a hinge; the vertex comes down, the brow remains stationary; and thus the case may be made one of the most simple, natural, and easy. We can only succeed, however, in this endeavour during the paroxysm of pain: we are not to expect that we shall be able to push the anterior fontanelle up above the brim; our only intention should be to prevent it passing down in the same proportion as the other parts of the head descend. There can be little question that it is desirable an attempt should be made to produce this slight, though very important change; for if we allow the labour to proceed naturally, it is more than probable, that, instead of the vertex descending, the face will come down, and we shall then have a case requiring even more room than the brow: so that it is to prevent the impaction of the head in that situation, and to prevent the brow being changed to a face presentation, that I strongly recommend you to use this counter pressure,—with caution, tenderness, and judgment,—in all cases where the anterior fontanelle originally offered itself, the face being directed laterally or backwards.

Brow presentation, with the face forwards.—The anterior fontanelle may present with the face looking forwards to one or other groin; and this position is even more unfavourable than either of those previously described; because not only is

that part of the head presenting, which in itself requires a considerably increased space, but it is placed in a most awkward situation as regards the pelvis. There is the double disadvantage of a brow presentation, and the face being directed forward. There can be but little difficulty in detecting this position, at any rate, after, or even previously to, the rupture of the membranes: the anterior fontanelle is easily discriminated, and the sagittal suture can be traced running obliquely backwards and upwards, until it terminates at the superior angle of the occipital bone, in the direction of one of the sacro-iliac symphyses.

The same remarks just made respecting the propriety and necessity of giving a new inclination to the different parts of the head, apply with equal truth, and even more force, to the variety we are now contemplating. There is the same chance of the case being converted into a face presentation,—the same likelihood of a protracted termination; and we possess almost an equally easy and effectual method of rectifying the unfortunate position. Counter pressure, during the time of pain, will here also avail us much; not, however, directed on the centre of the brow, but on one side, just above the temple. We may often succeed in preventing the chin passing downwards, in making the vertex the most depending part, and in throwing the face a little backwards. If we can cause the head to move in the slightest degree, so as to direct the forehead opposite the iliac fossa, we shall find that nature will eventually turn it with the face into the hollow of the sacrum. Should these attempts, however, not prove successful, the head may be embraced between three fingers and a thumb, and a rotatory inclination given to it: the proper change can thus usually be effected, unless indeed some time has elapsed since the membranes broke.

Brow presentation, the face being directed towards the promontory of the sacrum or symphysis pubis.—This position of the head is even more unusual than the same direction of the face, the vertex presenting; there will be equal or even more difficulty in its passage through the brim; the same means must be taken to detect its situation, and the same attempts used to place it in a more favourable one.

FACE PRESENTATION.

I am inclined to think that most of the face presentations which we meet with in practice were originally brow presentations, and have been changed by the action of the uterus in the way I have already specified: however, there is no question that the face sometimes offers itself even at the very onset of labour. Various are the positions in which a fœtus, when the face presents, may be placed; but this is the most usual. The crown of the head is directed to one ilium; the chest towards the other, and a shoulder towards the sacrum and pubes respectively. As the case progresses, the face descends down into the pelvis, until the summit of the head impinges on one ischium. The direction of the head is then totally altered; the face appears under the arch of the pubes, and the occiput sweeps the perineum. This change is, I believe, always observed to take place; at least I never knew an instance of face presentation, under which the head was expelled with the face sweeping the cavity of the sacrum. This case requires even more room than a brow presentation; and in a first child it is generally attended with great difficulty and distress: but if the parts be well relaxed—if the pelvis be good, and the pains strong—as a general principle, face presentations will be terminated with little or no assistance.

Mode of detection.—This case it is not difficult to detect, even before the membranes break; or at least it is easy to determine that no part of the cranium, properly so called, presents; for the face is readily distinguished from the harder parts of the head. On making an examination, an irregular soft body meets the finger, offering itself at the brim of the pelvis; which, unless we are careful in our diagnosis, we may possibly mistake for other parts of the child. The face has not unfrequently been confounded with the breech—of which I have known more than one instance; the cheeks have been taken for the nates, and the mouth for the anus. The prominence and regularity of the features will, of course, be our discriminating marks. Thus we have the nose about the centre; the two eyes, which are not difficult to detect, above; the chin below, and the mouth differing from the anus in shape, size, and in possessing lips;—the gums and the tongue can often be felt, and then doubt can exist no longer. Besides these sufficiently striking features, the breech possesses certain distinctive marks of its own, to be hereafter particularly noted. If we are satisfied indeed with simply placing our finger against the puffy cheek, we are very likely to fall into error; but we are not to form an opinion by one part alone—we must take all points and circumstances together.

Under a face presentation, then, must any means be adopted to place the child in a more favourable position?—So difficult, and almost impossible, was the transmission of the head under this presentation (as well as some other of these irregu-

larities) at one time thought, that it was recommended that the hand should be introduced into the uterus—that the feet should be laid hold of—that the position of the child should be completely changed by the operation of turning—and that it should be made a footling case. This operation, performed under the most favourable circumstances, is always attended with great pain, and frequently with great danger—danger both to the mother and the child;—to the mother, from the chance of injury which her structures (particularly the uterus) are exposed to—to the child, in consequence of the pressure which the funis umbilicalis must more or less experience, when the head is passing through the pelvic cavity. All these circumstances, then, being taken into consideration, the practice of changing the position of the child under a face presentation, by *turning*, is now exploded; and we rather leave the case entirely to nature, than subject the woman and the child to such dangers.

But suppose, on watching the case, we find no advantage gained; no change in the situation of the head — no advance from hour to hour—what then is to be done? We must here also act upon the same unerring principles before laid down: wait till symptoms require our interference, and then use that instrument which seems most applicable to the emergency. For it is impossible, by any counter-pressure, to make a beneficial change in the situation of the head under a face presentation: we cannot cause the head to turn upon the neck, so as to approximate the chin to the chest, by pressure applied by the finger; nor can we, indeed, succeed in producing the same alteration by the introduction of the hand over the vertex, the adaptation of the points of the fingers to the occiput, and the application of gentle traction; as some have recommended. The vectis, then — provided any instrument be required—will be found the most appropriate. Face and ear presentations, indeed, appear to me the only cases in which the vectis possesses any superiority over the forceps.

The features of a child born under a face presentation are generally much swollen, turgid, and livid. We must be prepared, therefore, to expect some disfigurement; which, however, will generally disappear in a day or two.

EAR PRESENTATION.

Ear presentations are by far the most rare of any of these irregular positions of the head. I only recollect to have seen one case in which the side of the head presented, so that the ear offered itself in the centre, directly over the brim of the pelvis. Either side of the head may present; the face may look to one side or the other, or to the pubes or the sacrum, as I now shew you.

As illustrative of the mechanism of ear presentations, we will take a case in which the face is looking backwards; in which the summit of the head is directed to the right ilium, and the left shoulder impinges on the left ilium; and in which the ear meets the finger, immediately on being passed up to the pelvic brim. In this case the head is usually propelled into the pelvis in proportion as the trunk of the child advances, until it comes to press low down upon the outlet; but in consequence of the head, in this position, being doubled sideways on the shoulder, the space required for its exit thus is more than the pelvic aperture affords, and before it can escape it must take a fresh direction: a change in situation, therefore, is effected;—not, indeed, a semi-rotatory turn, such as the head describes under the presentation of the vertex, but the summit of the head passes downwards, moving on the joints of the neck as on a hinge; and (as exemplified on the machine) the case is, by degrees, converted into one of the most favourable kind for the exit of the child; the face being thrown into the hollow of the sacrum, and the occiput turning up under the arch of the pubes.

Mode of detection.—There can be little difficulty in detecting an ear, or in determining how the head lies, when we feel it. There is no part of the fœtal body we are likely to confound with the ear. We can feel the different parts of the ear itself, and the bony head surrounding it. We can feel the flap and the sessile part; we know that behind the flap is situated the occiput, and before the sessile part, the face; and these points will immediately lead us to distinguish the true position of the head.

Having, then, detected the ear, and ascertained the situation of the head, must we proceed to deliver by turning, as some have recommended; must we endeavour to bring down the vertex; or leave the case to nature? I can only repeat the observations I have just offered for your guidance under face presentations. Turning is not to be thought of. I do not think any good can be effected by counter-pressure; I cannot see what advantage can be gained by the introduction of the hand, and it is certainly not necessary to interfere instrumentally, merely because the ear presents. The common principle must here direct us; we must wait patiently, in the hope that nature will effect her object, and should the labour not be advancing satisfactorily, we may, after the lapse of some hours, perhaps, introduce the vectis, and endeavour to bring down the vertex, in the way I shall hereafter shew when I come to speak of the value of that instrument.

ON THE LAWS AND TREATMENT OF SYPHILIS.

By Robert Williams, M.D.
Senior Physician to St. Thomas's Hospital, &c.*

Every morbid poison uniformly produces a disease, generically the same; but those that act on two or more tissues are liable, in different seasons and in different localities, to undergo certain modifications, which occasion slight or even important variations in their specific phenomena. The constitution also of the patient—its strength, or other peculiarity, greatly influence their actions. The poison of syphilis belongs to this class, and its specific phenomena are liable, consequently, to many variations. It is intended, in the present paper, to notice only some of its most usual laws, and only some of their most prominent specific deviations.

The venereal virus is usually introduced into the system either by a mucous membrane or by an abraded surface of the dermoid tissue; and having been absorbed, it lies latent a period of time varying from a few hours to three or four weeks, or more. This period of incubation being passed, it generally, but not constantly, limits its effects to producing a specific inflammation of the part by which it was introduced. This inflammation terminates in ulceration, and the pus secreted by the sore is capable of communicating the disease to another person. These primary sores being healed, the disease is, in many persons, cured, and the poison eradicated. But in a considerable number of cases the poison has not exhausted itself; it is only disarmed of part of its virulence, and remains circulating in the system, modified, indeed, but still possessing a power of producing, at varying, and sometimes very distant, periods of time, many secondary actions or affections; but whose products, unlike those of the primary affection, are no longer contagious. At the end, then, of a few days, or a few weeks, or a few years, after the primary affections have terminated, (for there are cases now in St. Thomas's Hospital, in which four years have elapsed between the healing of the primary and the occurrence of the secondary affections,) the poison falls, but in no very certain order, on some or all of the following parts—namely, the skin, the throat, the iris and conjunctiva, the bones or periosteum of the nose; the head, the hand, or on the long bones of the extremities; and also on the clavicle. It attacks likewise the cartilages of the sternum, the fascia, the ligaments, and synovial membranes of the joints.

All these multitudinous affections, however, do not necessarily occur in the same individual; for there are many instances in which the poison, having induced one or more of them, becomes exhausted before it has produced the whole series. Neither do these affections, even when they do occur seriatim, always occur in the order that has been mentioned, although that is the most common one; but on many occasions, according to every possible inversion of this order: for we often see the affections of the bones taking precedence of those of the throat; those of the throat, that of the iris; that of the iris, those of the skin, and so on. But in this respect the poison of syphilis does not vary from its general law more than many other poisons from theirs: for in scarlet fever, for instance, the affection of the throat not unfrequently precedes that of the skin, contrary to the usual and customary orders. In some cases also of scarlet fever, the affection of the throat is altogether wanting, the poison having exhausted itself in the primary action on the skin; affording thus an instance of that partial action which is common to many poisons, and which has been affirmed to be a law of syphilis.

It is a law also of poisons that act on more membranes than one, that their specific actions on those membranes may be either simultaneous or consecutive. Thus, in fever, the typhoid poison acts on the mucous membranes of the intestinal canal, on the serous membranes of the brain, and also on the membranes of the lungs. Now it may produce all those affections contemporaneously, or it may induce them severally and consecutively, or, as has been stated, may produce them partially. So it is with syphilis: for although that poison may attack each tissue seriatim, yet it more commonly induces a simultaneous affection of more than one—for example, it may attack simultaneously two tissues, as those of the throat and skin (a combination very commonly seen together), or three tis-

* Read before the College of Physicians, March 31, 1834.

sues, as those of the throat, skin, and osseous system; or even four tissues, as the skin, the bones of the head, the throat, and the ligamentous tissues; and these may be all affected together, as in a case now in St. Thomas's Hospital. Four tissues, however, are the greatest number that I recollect to have seen diseased at the same time. But in whatever order these actions are set up, whether simultaneously or consecutively, or partially, long periods of time (many months, perhaps many years) may elapse between each specific attack; so that a considerable portion of life may pass away before the effects of this poison are thoroughly exhausted. The longest period of time that has elapsed, under my own observation, between the successive attacks of the poison, occurred in the case of a gentleman, who, shortly after the battle of Waterloo, laboured under primary sores, which yielded in a few days to the mildest treatment. Two years ago, however, he was attacked with psoriasis of the leg, and considerable enlargement of the lower third of the tibiæ. These affections had every character of venereal affections, and after resisting for a long time mercury and sarsaparilla, at length only yielded to the treatment which will be hereafter described. In this case, fifteen years must have elapsed between the primary absorption of the poison and the occurrence of the secondary symptoms; and it is quite certain that no other primary affection occurred in this case during this long interval. Such are the more general laws of syphilis.

In the treatment of syphilis an opinion has prevailed, for more than three centuries, that this disease never terminated except by the death or mutilation of the patient. But it has been proved within the last twenty years, and by the most irrefragable evidence, that the disease, whatever be its form or complexity, will terminate spontaneously. It is admitted, however, that when the disease is allowed to run its course uninfluenced by medicine, that it is much prolonged, and that the secondary symptoms are of much more frequent occurrence. It is necessary also, to ensure a favourable result, that the patient should submit to a painfully long confinement to his bed, and not only that he should abstain from every indulgence, but must submit to an exceedingly low or spoon diet. A spontaneous cure, therefore, is a termination little in unison with our habits of active life, and some remedial mode of treatment is not merely desirable but necessary. In the cure, then, of syphilis, mercury in greater or less doses, and introduced into the system either locally or by the mouth, or by inunction, is unquestionably, according to our present experience, the great, and indeed only specific remedy by which we are enabled to control and subdue the primary action of this baneful poison.

The problem of the cure of the secondary symptoms is not of such easy solution. It has been affirmed that these symptoms are aggravated wherever mercury has been used for the cure of the primary sores: of this, however, I entertain the strongest doubts; for it is agreed that secondary symptoms much more frequently occur when the primary symptoms have been allowed to terminate spontaneously than when mercury has been used, and yet it seldom happens, when there is so strong a tendency to disease, that such disease, when excited, is mild. It is a fact also, that before mercury was used for the cure of syphilis, it took its name from the more hideous of the forms of its secondary symptoms. In Lisbon also, where mercury is little used for the primary sores, every military surgeon admits that more mutilated faces are to be seen than in any other town of the same size in Europe. But whatever may be the fact, it is certain that the cases of secondary symptoms which present themselves at the London hospitals are of great severity, and shew little tendency to a spontaneous cure. On the contrary, the long and extreme sufferings of the patients, and their worn and emaciated frames, demand prompt and immediate relief. In the cure, then, of the secondary symptoms, mercury and sarsaparilla are the only remedies that at present maintain any character.

The medicinal properties of the latter substance, however, are so little determined—the cases in which it is useful so little agreed on—that Mr. Hunter, and also many most eminent physicians and surgeons, have abandoned it as altogether inefficient; and it is hardly an exaggeration to state that the majority of the profession, in the present day, rely on mercury as the only remedial agent in all forms of the disease.

If we look, however, to the laws which govern other poisons, we find that

their different specific actions are only subdued by many and even opposite remedies; and consequently we might expect that although mercury might be eminently useful in some stages of syphilis, still, that in combating so multiform a disease, many different modes of treatment would be necessary. In paludal disease, for example, as long as the action of the poison is limited to inducing intermittent fever, unaccompanied by any local disease, quinine is a certain and specific remedy. But no sooner are the secondary affections of that poison established, as inflammation of the liver or spleen, than calomel is the specific medicine; quinine being either unavailing or injurious. An equally remarkable change of treatment is required should the poison of scarlet fever fall upon the larynx or peritoneum, or on the synovial membrane of the joints. It will be plain, therefore, that mercury, the great specific in the primary affections of syphilis, is of doubtful efficacy in its secondary affections; and every practical physician knows how often this powerful remedy totally fails in relieving many of the most distressing forms of the disease. As the treatment, therefore, of the secondary symptoms of syphilis has not as yet received any sufficient investigation—as its laws have not, as yet, been sufficiently determined—the following results of an extensive hospital practice in the treatment of syphilitic affections of the bones, of rupia, and of foul and deep ulcers of the throat, are submitted as affording some data towards the solution of this most perplexed and difficult problem of medical science.

The venereal affections of the osseous system consist either in the formation of periosteal nodes or in inflammation of the substance of the bones themselves; and each of these affections has two varieties.

The periosteal node is formed by the deposition of a series of layers of imperfectly-formed bony matter, deposited on the external surface of the healthy bone, and which may be removed by maceration. The periosteum also is thickened in these cases, and occasionally ulcerated. This description of node, like all newly-formed parts, has a tendency to run into disease, and readily suppurates. The second description of periosteal node is caused by the secretion of a gelatinous matter, either by the periosteum or among the ligaments or fascia. It is of the consistency of thickened gum-water, though in a few cases it is, in parts, much harder; and hence it has acquired the appellation of the gummy node. This node has a considerable tendency to ulcerate; and when the tumor is opened, extensive ulceration generally succeeds, followed by exfoliation of the bone, and consequently long and severe disease.

When the poison attacks the bone itself, the solid portion of the osseous cylinder inflames, and becomes greatly thickened, and also exceedingly more dense. Interiorly, the cancellous structure is encroached upon and partially obliterated; the medullary matter also is of greater consistency than in health; while exteriorly it presents a hard, solid, prominent tumor. The periosteum also, as in the former cases, is thickened. If the disease preceeds, an ulcer forms in the very heart of the inflamed portion of the bone, and exfoliation ultimately takes place. The second form of the osteal node is when the periosteum becomes greatly thickened and the bone inflames, and the disease terminates in caries.

The hard periosteal node is the most frequent form of the disease, and many cases will yield to mercury; still there is a large number in which mercury produces no such successful results, and the patient is doomed to many years of excessive suffering, or is only relieved during the period that his mouth is sore*. New experiments were therefore necessary, to determine a more efficient treatment of this form of disease.

Isaac Chilton was admitted into St. Thomas's Hospital, 17th January, 1831, with large periosteal nodes on each tibia; his sufferings were most severe, and his health was much impaired. The first medicines that were prescribed him were mercurial, and he was salivated; but his pains returned as soon as his mouth healed. A decoction of smilax aspera was next tried, but without success. On the 7th April, therefore, the decoction of sarsaparilla was substituted, and which was taken till the 2d of June, a period of nearly eight weeks; but still there was no alleviation of his suffer-

* Two cases have been recently cured in St. Thomas's, both females, in which the one had been reduced to a three pennyworth of opium daily, and in the other the back bone was denuded.

ings; on the contrary, the first phalanges of the second and third fingers of one hand became thickened, and in a short time the inflammation terminated in necrosis, so that one finger was obliged to be amputated; and this untoward event occurred in spite of the application of leeches, and a variety of local treatment, as also of minute doses of the pil. hydrargyri and of colchicum, and even of nux vomica, which was tried when the other medicines failed in producing any good effect.

The parts having healed, new experiments became necessary to save the other finger, which seemed to be rapidly running into a similar state of necrosis. On the 2d July, therefore, or not until nearly six months after his admission into the hospital, five grains of potassæ hydriodatis, out of camphor mixture, were ordered three times a day; and this quantity was, during some part of the treatment, increased to grs. x. The results were most happy; his pains were relieved, the nodes subsided, and his finger was saved. This patient subsequently laboured under iritis, ulcerated throat, affection of the nose, and also of the skin, as also of the ligaments and synovial membrane of the joints, and was consequently under my care or observation for at least twelve months afterwards; but had no return of the periostitis.

The treatment of this case had scarcely terminated, when the coachman of a member of parliament was admitted into St. Thomas's Hospital with an exactly similar state of the tibiæ, and of the phalanges of the fingers. This man had been for many months under a treatment by mercury and sarsaparilla, and was so reduced by constant suffering, that his family thought his death inevitable. Eight grains of the hydriodate of potash were directed to be taken by this person three times a day; and in two months he was discharged cured.

Instructed by these cases, I have invariably for the last three years used the hydriodate of potash in the hard periosteal node, and with uniform success. The average dose of this medicine is eight grains; carried beyond that quantity it purges; and even limited to that quantity, it requires some management to obviate nausea. It is not till the end of from five to ten days that its mitigating effects are felt; but at the end of that time the pains are relieved, the node begins to subside, and in the majority of cases it disappears altogether. In a few instances, however, it is permanent; but still I apprehend it will be found to be much less frequently so than when treated by mercury.

The hard periosteal node, like all newly-formed parts, runs readily into disease, and often suppurates. It is a remarkable law of some poisons, that when the inflammation they excite terminates in abscess, the remedy which properly and timely administered would have prevented such an untoward event, now loses all power over the disease, or only aggravates it,—as is the case with mercury in hepatitis. I have seen no case of hard periosteal node, treated by the hydriodate of potash, run into suppuration; but it frequently does so when it is treated by mercury; and in such cases that medicine ceases to produce any salutary effect, and sarsaparilla now appears to be the great remedial agent provided by nature for the cure of this form of the disease.

Joseph Keaff was admitted into St. Thomas's Hospital, 28th Dec. 1826, with large hard periosteal nodes on the tibia of both legs; his face bore strong marks of the agony which he suffered; he was much emaciated, and his whole appearance haggard. It was attempted to relieve this man by leeches and blisters to the nodes, and by the warm bath, as also by opium, but without success. The blue pill was now given, but still without relief; and this mercurial treatment was afterwards pushed to the point of salivating him, by rubbing the linimentum hydrargyri liberally on both legs. This treatment mitigated his pains; but on his gums healing they returned in their former force. The nodes now passed from the hard stage to that of suppurative inflammation; and the sharp edge of a deep ulcer, filled with fluid, could now be felt on each tibia. The mercurial treatment having so egregiously failed, was of necessity abandoned; and the patient's strength was attempted to be supported by the inf. gent. comp., and afterwards by quinine, of which five grains were given 6tis horis. Under this treatment, however, his tongue became white and greatly coated, febrile symptoms appeared, and his life seemed in much danger. This patient had now been under a varied, but unsuccessful treatment, nearly

six months, when, on the 5th of June, 1827, the decoction of sarsaparilla was prescribed for him. Under this medicine his pains subsided, the ulcers healed, and he was discharged, cured, on the 2d August following—a space of little more than two months from his point of greatest depression, and the time of the commencement of the treatment by sarsaparilla.

I have of necessity had a great number of cases of ulcerated periosteal nodes under my care since the year 1827, and they have all, without exception, been healed by the exhibition of sarsaparilla; but, what is extraordinary, I have not seen a single case in which ulceration and exfoliation of the bones of the nose has appeared to be in the least controlled by any quantity or any form of sarsaparilla; neither have I seen sarsaparilla of any demonstrable use in any other form of syphilis; and of a truth, after a long series of experiments to determine its sanatory properties in all diseases, I have been driven to the conclusion, that its only valuable medicinal effects are those of healing ulcerated periosteal nodes.

The gelatiniform, or gummy node, is a disease of much less frequent occurrence than the hard suppurating node; and I have much less experience in cases of this description than the former. From their tendency to ulceration and exfoliation, I have known them terminate fatally, when treated either by mercury or sarsaparilla, or both. There is a case, however, now in St. Thomas's Hospital, of this kind; and the patient, a woman, has been treated with the hydriodate of potash. This medicine has been taken for more than six weeks. The progress of the cure is slow; but the tumors have much decreased, and the pains are greatly relieved; so that a rational expectation may be entertained that this intractable form of disease will ultimately yield to this new remedy.

Venereal caries of the bones is by no means of the frequent occurrence which the specimens in our museums would lead us to believe to have been the case in former days. It will be seen, that, in two cases that have been narrated, the hydriodate of potash was successful in controlling this disease. Venereal inflammation of the substance of the long bones, ending in deep-seated ulceration, is also of rare occurrence. From the difficulty of determining the exact state of the diseased parts, it is probable much time must elapse before the most efficient treatment of this form of disease can be satisfactorily determined. There is a case, however, now in St. Thomas's Hospital, of this description, and in which the hydriodate of potash has failed to produce the good effects that might have been expected from its use; but it is probable, in this case, that it was not exhibited till after ulceration of the substance of the bone had commenced. Extensive exfoliation of the tibia has taken place, and mercury and sarsaparilla, or sarsaparilla alone, has afforded the greatest relief, and it is probable the patient will shortly do well.

When the bones of the nose are affected, I do not remember to have seen any case in which either mercury or sarsaparilla has either interrupted the course of the disease, or produced a decidedly good effect. In a very few instances I have seen this disease subside while the hydriodate of potash was being exhibited for some other part that was affected: but I feel much difficulty in affirming, that the hydriodate of potash has any decided sanatory effect in these affections. I am much more in the habit of relying on, and therefore of directing, black-wash to be injected two or three times a-day into the nasal fossæ; and though it may be difficult to assign the principle, a cure of the disease is by this local treatment very generally accomplished.

When the syphilitic poison falls upon the skin, it induces many proteiform diseases: many of these require that mercury should be liberally introduced into the system; a smaller number, as the tubercular eruption, I have some reason to believe will yield to the hydriodate of potash; but my experiments to determine this are not sufficiently extensive: but there is one which is best treated locally with mercury, namely, rupia.

Rupia usually attacks the feeble in constitution, and the emaciated in frame; and I have seen cases that have died when mercury has been given by the mouth, even in very moderate doses. Warned by this result, I usually adopt a local mode of treatment; first removing the scab by a poultice, and then directing the ulcers to be dressed with the unguent. hydr. nitr.

oxyd.; and I have seen no case of rupia that has not done well under this mode of treatment.

Should the syphilitic poison affect the tonsils, soft palate, and pharynx, I have seen many patients die under the most judicious general treatment; and the following case is by no means a solitary instance of how violently it sometimes perseveres in its course, until it has destroyed the patient.

F. Johanson, a German, was admitted 4th February, 1830, with rupial sores, diarrhœa, and venereal sore throat: the rupial sores were readily healed by the unguenti hydr. nitr. oxyd., and the diarrhœa was stopped by opium; the ulceration of the throat, however, remained either stationary, or continued to spread. To remedy this most threatening evil, the following medicines were in succession tried:—the decoction of sarsaparilla, with the extract and dilute nitric acid; the decoction of smilax aspera; the pil. hydrarg. in small doses, but continued till the mouth was affected; small doses of the hydriodate of potash*; quinine; the infusions of calumba and of canella, as also many metallic substances; as the sulphate of iron, the liquor arsenicalis, and also the oxide of platinum, in 1 gr. doses, three times a-day. This medicine acts emetically, when given in doses of 2 grs. 6tis horis; and as two cases of chronic laryngitis, not depending on syphilis or on tuberculated lung, had recovered under the use of this substance, after all hope of curing them either by mercury or ordinary antiphlogistic remedies had failed, a hope was entertained that some benefit might result in this case from its use. It is perhaps unnecessary to add, that this patient received a liberal supply of wine and porter, and, indeed, of every indulgence which his state of health required, and which the London hospitals so bountifully afford; as also that many species of local application were tried, as the linimentum æruginis, mercurial fumigation, gargles of the chloride of soda, and of the nitric and muriatic acids. But, nevertheless, in spite of all our endeavours, this patient died the 3d of April, 1831, having lingered a period of fourteen months.

This case was followed by a quick succession of four or five similar cases admitted into the hospital; and among the rest was Chilton, whose life seemed once more compromised by deep and spreading ulcers of the throat. But having observed the different results of general and local treatment in rupia, I determined on a similar experiment in the treatment of these extensive and too frequently fatal ulcerations of the throat. I accordingly forbore in these cases giving any mercury by the mouth, but directed the ulcerated parts, both of the tonsils and pharynx, to be smeared, by means of a camel's-hair pencil, with the ung. hydr. nitr., or, when that failed, with the ung. hydr. nit. oxydi. All the patients so treated recovered, although many of these cases were as severe forms of the disease as I have ever witnessed*. It is deserving of remark, also, that those cases recovered the most rapidly where the gums resisted the action of the mercury altogether. In a few instances it was impossible to prevent the mouth being affected, when an extension of the ulceration immediately followed; shewing that the amelioration was occasioned by the local stimulus, and not from the constitutional affection. I have treated a considerable number of cases of this description since that period, and they have all terminated in the most favourable manner.

The syphilitic poison not unfrequently falls upon the iris and conjunctiva, producing iritis. In this form of the disease, mercury is again necessary, and to the fullest extent of affecting the system. The rapid manner in which iritis succeeds to other syphilitic affections which have been cured by affecting the constitution, is most remarkable; for I have seen cases in which it could hardly be said that the gums were healed from a course of mercury, for the cure of an affection of the skin, when iritis has appeared, and that formidable disease has only yielded by again inducing the severest ptyalism.

When the poison falls on the ligaments or synovial membranes, the disease is in all cases of obstinate duration;

* I exceedingly regret not to be able to confirm the beneficial results attributed by Mr. Mayo to the hydriodate of potash in the cure of venereal sore throat; for in my own practice I have seen no case of venereal sore throat cured by this medicine; on the contrary, I have frequently seen venereal sore throat break out and continue to spread under its use.

* Mercurial gargles will not produce the same effect, even though the liquor hydr. oxymuriat. be used undiluted, and the patients, with all their inconveniences, prefer the ointment.

and I am at a loss to determine whether small doses of mercury, or the hydriodate of potash, with occasional blisters, is the most to be commended. The patients, however, have sometimes asked for the hydriodate of potash in preference.

Having thus briefly and imperfectly touched upon some of the more remarkable phenomena of this complicated disease, it only remains to be stated, in conclusion, that it seems to be established, that the poison of syphilis taints the blood; a fact which is proved by the occasional birth or abortion of children covered with venereal affections; and that mingled with the blood, it may circulate for many years after the primary sores have been healed, producing a long series of secondary symptoms, always disgusting, frequently most painful, and sometimes fatal. It has been imagined that these secondary results are not a consequence of the syphilitic poison, but are caused by mercury improperly and injudiciously administered. This is, however, unquestionably an error; for secondary actions are not peculiar to syphilis, that they should be thus mistaken, but are common to many other poisons, as the paludal, the typhoid, and the variolous. It seems absurd, then, to attribute these affections to mercury, since that medicine, when exhibited for the cure of other diseases, is followed by no similar phenomena.

In the cure of this disease, it should be remembered that it is a law of some poisons that they act with an intensity proportioned to the weakness of the constitution. Syphilis is one of this order; and consequently the rule must be to treat each particular affection as it arises at the least possible expense to the health of the patient, be that treatment by mercury or otherwise. It is a law, also, of syphilis, as well as of poisons in general, that its virus cannot be controlled or neutralized, or rendered inert, as long as it lies latent: medicine cannot prevent its actions, although when set up it may cure them. It follows, therefore, that the continued use of mercury after the primary or other affection has terminated, is contrary to all the rules deduced from the known laws of poisons. Such an exhibition may, by debilitating the constitution, render it more subject to secondary diseases, but it cannot prevent them. In these respects, then, the laws of the treatment of syphilis, as deduced from theory, are in accordance with the practical results of the most eminent men of our times.

It perhaps would be an unprofitable task to enter upon the question of the *modus operandi* of the medicines which have been recommended; but it may be right to state, that when the hydriodate of potash is exhibited, iodine is found in considerable abundance in the urine—clearly proving that this remedy does circulate with the blood; and thus perhaps the bane and antidote may be brought into actual contact.

As the experiments that have been narrated have been frequently repeated, and always with the same beneficial results, and as they promise to be a considerable improvement on the present mode of treating secondary symptoms, it has been thought that they were of sufficient importance to be submitted this evening for the opinion of the profession at large.

ON PERFORATION OF STRICTURES OF THE URETHRA.

With Cases.

By R. A. Stafford,

Surgeon to the Mary-le-bone Infirmary, and formerly House Surgeon to St. Bartholomew's Hospital.

To the Editor of the Medical Gazette.

Sir,

In sending you for publication a series of cases of impermeable stricture of the urethra, treated by perforation, it gratifies me to be able to state, that my opinion of this treatment remains the same as when I first recommended it to the notice of the profession, in a work* published by me a few years since. I have not yet had a single case, under my own care, where it has failed of success; nor has any instance occurred in which mischief has arisen from it. In no case has a false passage been made, nor have extravasation of urine, hæmorrhage, or any other bad symptom, ensued in consequence of its adoption. The advantage which the perforation of this description of

* "A Series of Observations on Strictures of the Urethra; with an Account of a New Method of Treatment, successfully adopted in cases of the most obstinate and aggravated form of that Disease. Illustrated by Cases."

stricture possesses over any other method usually employed, will be seen by the relation of the cases; and I am convinced, should any prejudices exist against it, they will, ere long, by all who wish well to science and their fellow-creatures, be entirely overcome.

I shall only further add, that my sole object in offering to you these cases for publication, is to enable the members of the profession to judge for themselves of the expediency, or otherwise, of adopting this mode of treatment: I must, however, admit, that a strong conviction, on my own part, of its superiority, in the worst cases of stricture, has made me the more anxious to direct attention to it.—I have the honour to be, sir,

Your very obedient servant,

R. A. Stafford,

28, Old Burlington Street,
April 2, 1834.

Case I. — *Impermeable Stricture of from twenty to thirty years standing —Failure of Bougies and Caustic— Cure by the Lancetted Stilette.*

George Stevens, æt. 56, was admitted into the St. Marylebone Infirmary, January 14, 1831, with an impermeable stricture, situated at the bulb. He had been afflicted with this disease, (which originated from gonorrhœa), from between twenty to thirty years, and latterly has suffered so much from it, that he thinks the irritation of the urinary organs brought on a paralytic seizure of the left side, which he has now laboured under for more than a year. For some years past, he has made water only drop by drop, and the bladder can never entirely empty itself. The urine is of an alkaline character, and mucus is deposited at the bottom of the vessel in which it is contained. Bougies, caustic, and other means, from time to time, have been employed, but never with any relief. About a week after his admission, I made an attempt to perforate the stricture, and got through half an inch of it. The part was kept open by passing a bougie every other day. He suffered no pain nor constitutional symptoms that required more than fomenting the perineum, and taking an occasional aperient. In a week from the last operation, I again employed the lancetted stilette, and at once succeeded in perforating the obstructed part in the canal. A small gum elastic catheter was immediately passed into the bladder, and left there. In two days this was changed to one twice its size. The catheter now produced considerable irritation, so that it was left off altogether. Bougies and steel dilators were consequently passed every second or third day, until the strictured part was as large as the rest of the canal. On March 15, he left the hospital, and could make water in a full-sized stream.

Remarks.—It is not improbable that the irritation produced by the stricture brought on the paralytic affection. The sympathy between the urinary organs and the brain is very remarkable. Hence we frequently see diseases of the kidney, bladder, and urethra, terminate in urinary coma and serous apoplexy. This case could not have been treated successfully by any other plan. It had resisted the common modes—bougies and the application of caustic. Division from the perineum, independently of its uncertainty, would most probably, from the severity of the operation and the state of the patient, have caused death. The milder, and perhaps the most natural method, was the one pursued—perforation of the stricture within the canal of the urethra.

Case II.—*Impermeable Stricture of thirty years standing, in a man of 72 years of age, cured by the Lancetted Stilette or Urethral Perforator.*

Mr. R., æt. 72, came from the country in May, 1831, to consult me concerning the state of his urethra, which had been the source of considerable annoyance to him for at least 30 years; and latterly the malady had so much increased, that he was in constant irritation and pain. He could only pass his urine guttatim, and was constantly obliged to rise during the night to attempt to make water, but he could never empty the bladder. He was in a miserable condition, being much worn and enfeebled; and, as he expressed himself, all he wished was to spend his latter days in a tolerable degree of comfort. On examining the urethra, I found a stricture immediately behind the bulb, which was impermeable to a bougie. The lancetted stilette, or urethral perforator, was passed to the part, and the stricture was at once perforated. A small catheter was introduced into the bladder, and its size was increased every two days. In a week from the operation they were left off. Steel dilators were now passed every second or third day, until a No. 12 could be introduced with ease. In a month or five weeks he went into the country again, perfectly well; the pain and irritation had ceased; he was enabled to sleep during the whole night without being under the necessity of rising, and he could make

water in a full-sized stream. The auxiliary treatment was by aperients, frequently soothing the urethra by fomentations; and when inflammatory symptoms arose, applying leeches to the perineum.

Remarks.—This case shews, that even at a very advanced period of life this treatment may be employed without danger. Considering the enfeebled state of the patient, and the duration of the disease, the immediate relief obtained is remarkable.

Case III.—*Two Strictures perforated by the Lancetted Stilette; one three inches from the orifice, and the other at the entrance into the membranous portion of the Urethra.*

The history of the early part of this case may perhaps be best described in the patient's own words.

"Previously to the beginning of the year 1818 I had several claps and some trifling stricture, which I have strong reason to believe were eradicated, in the early part of this year, by the use of bougies, &c. I remained well till October 1823, when I again got a clap, which was renewed in the early part of 1824, and from which I think the commencement of the stricture may perhaps be dated, as I recollect having considerable difficulty in making water in August and September, 1825. I applied to a medical gentleman at that time, from whose treatment I received considerable relief. But I believe this difficulty in voiding my water did not wholly arise from stricture, but in a considerable degree from the state of the bladder, probably occasioned by stricture, as there was a great deal of sediment or mucus in my water, and which entirely separated from it after standing some time. Although I did not make water in a sufficient stream, yet I did not perceive myself getting much worse until the beginning of 1829, when I think I began to get gradually worse, and still kept neglecting myself, with the exception of several times (during that year and the beginning of 1830), taking something prescribed by a medical gentleman to relieve the bladder, not being aware that stricture existed to the extent which turned out to be the case. For several months previously to putting myself under your care, in October 1831, I had considerable difficulty in holding my water; in fact, I could not at all times prevent its voluntarily dribbling away. I must also observe, that for several years (say four or five), I have had cause to make water oftener than a person free from disease, and voided it in a smaller stream, and in less quantities, than ought to be the case. This propensity has also increased within the last eighteen months or two years, until it has passed only by drops.—R. B."

When I first saw this gentleman, I found him to be in a highly nervous state. He was much in the situation he has described—passing his urine guttatim, having frequent rigors, a discharge, and being almost always in pain. A stricture, impassable by a bougie, was present, three inches from the orifice, and its indurated structure could be felt externally by taking the part between the finger and thumb. After having soothed the urethra by bleeding, fomenting, and administering anodynes and aperients, the straight urethral perforator was passed to the part; and by three incisions with the lancet, the stricture was divided. But little pain was felt, and only a few drops of blood followed. A small catheter was passed, with the view of leaving it in the bladder; its progress, however, was arrested at the entrance into the membranous portion. In consequence of this second stricture, steel dilators only were passed every other day through the first, until it was enlarged to the natural size of the urethra.

The patient having business in the country, left town for two months. He felt himself much relieved from the first operation; and on his return the other stricture was perforated. A catheter was left in the bladder for a week, and followed up by passing steel dilators. He went into the country, having lost all his distressing symptoms, and could void his urine in a full stream. He was desired to pass a catheter once every week for the present, and to do it as often as required afterwards.

Remarks.—The presence of two strictures in the urethra at so great a distance from one another, would have made the operation of their division from the perineum unadvisable; for as soon as one had been operated upon, another would have been found four inches from it; and thus it would have been necessary to have laid open at least four or five inches of the urethra. The time it would have taken to have repaired so much injury done to the canal would, of course, have been considerable, besides the uncertainty of its ultimate accomplishment. The perforation of the strictures was attended with little pain or loss of blood; but little confinement was required, and the cure was complete.

CASE IV.—*Stricture of twenty-five years standing, where the Caustic and Potassa Fusa had failed, perforated successfully.*

May, 1830.—Mr. W., æt. 47. In his youth, when he was about 20, this gentleman had contracted a gonorrhœa, for the cure of which he had used strong injections. The discharge ceased, and he thought no more of the disease until two years afterwards, when he found that the stream of urine became smaller and smaller, until it was about the size of a packthread. He consulted his surgeon, who employed bougies, and dilated the stricture. Being a good deal in the sporting world, he neglected himself for some years, and lived pretty freely. The stricture gradually returned, and got worse. The part at length entirely closed up, and he could void his urine only drop by drop. He now applied to his surgeon again, who employed the caustic and potassa fusa. Neither of these had any effect in destroying the stricture. He went on in this miserable state for a few years, when his health began to suffer. In May, 1830, he applied to me. He was in a highly nervous condition, and was very timid. I found a stricture situated only two inches from the orifice: it was indurated, and quite impassable by a bougie. He would not consent to my operating with a larger instrument than a small No. 4. I perforated the stricture, which was about half an inch in length, with this instrument, and then passed a gum-elastic catheter into the bladder. He could not bear the catheter to remain longer than a few hours; it was therefore withdrawn. Bougies were now passed every day, and the sizes increased as the patient could bear them. In five days a No. 6 catheter could be passed; but on that day he received a communication that his wife was dangerously ill in the country. He immediately left London, and I did not hear from him until a year afterwards, when I received the following account:—He says, "You no doubt have ere this condemned me for neglect or indifference from not writing you. I was so unfortunate as to take a severe cold, either before I left town or on my journey down, as to lay me up with a severe attack of rheumatic fever three days after I got to Sheffield; I have been confined to my bed and room ever since. I am now much better; and as the weather gets warmer, no doubt I shall soon be better. With regard to the other complaint, the stricture, I am happy to say I have not retrograded, which is a wonder, considering the helpless state I have been in, not being able to introduce the catheter regularly. I can now pass a sound (No. 7), a size larger than the silver catheter you lent me."

REMARKS.—There is but little doubt that had this gentleman remained under my care, he would have got well. His being obliged to leave town just at the time he did was very unfortunate: it was still more unfortunate that he got an attack of the rheumatic fever. It was gratifying to find, however, when a bougie had not been passed for a whole year after the operation, that a No. 7 sound could be introduced into the bladder.

CASE V.—*Stricture that had resisted Bougies and all Urethral Instruments, perforated and cured.*

Geo. Pace, æt. 20, was sent to me by my friend Mr. Evans, surgeon, of Hampstead, Dec. 16, 1831. He had a stricture, brought on from gonorrhœa, in the membranous portion of the urethra, which had resisted the passage of the smallest bougies, and every description of urethral instrument, for four successive months. His sufferings were very great; the urine only passed drop by drop; he had considerable pain in the loins; and when he attempted to void the contents of his bladder, the straining was so great that his eyes immediately became blood-shot. He had sleepless nights, tenesmus, and a constant desire to make water, without being able to effect it. The lancetted stilette, or urethral perforator, was passed down to the stricture, and two incisions made. A tea-spoonful of blood followed the incisions. He was desired to go home and keep quiet; to take an aperient; to foment the perineum; to apply leeches, if the pain was great; and to have a bougie passed on the alternate days. This he did, and returned to me in a week. Two more incisions were made, the stricture was perforated, and a No. 10 elastic gum catheter passed into the bladder. He went home immediately in a coach, and, under the superintendence of Mr. Evans, wore the catheter for a day or two, when it caused so much irritation that he was obliged to leave it off. Bougies were ordered every other day, and in a fortnight from this period, a No. 12 steel dilator could be passed into the bladder with the greatest ease; and there was so little impediment in the urethra, that it would have been impossible to have told where the stricture had been present.

REMARKS.—In this case it would have been necessary to have divided the stric-

ture from the perineum, had not this treatment been adopted. Besides the pain and uncertainty of such an operation, the time it takes to restore the urethra by this method is considerable, not taking into account the imperfect manner in which it is usually performed. The superiority of the treatment by perforation cannot admit of a doubt; it is accomplished without much pain; with but little loss of blood; the urine flows through the natural channel; the time taken is comparatively short; and the cure is complete.

[To be continued.]

OBSERVATIONS

ON THE

THEORY OF RESPIRATION.

By Dr. Stevens.

To the Editor of the Medical Gazette.

Sir,

I have been induced to send you the following observations, in consequence of a paper written by Dr. Gregory, and Mr. Irvine, of Edinburgh, and lately republished in the Medical Gazette. If I have passed in silence the objections which have been made by certain individuals, it was partly because it must have been obvious to all that the said objections were made from personal motives, and written at a time when the writers were under the influence of excited feelings, which rendered them altogether incapable of investigating truth. The paper from Edinburgh is of a very different description. It is written by a gentleman of scientific acquirements: their object is pure, their objections are plausible, and therefore entitled to the most attentive consideration.

Until very lately, it was generally believed that the addition of oxygen was the essential agent in producing a change of colour in the blood from venous to arterial; but it is now, if I mistake not, almost universally admitted that there is another agent in the circulating current which is much more essential than even oxygen for this purpose.

The air which is obtained from pure water contains about 32 per cent. of oxygen. When we immerse a small clot of arterial blood* in a large volume of this oxygenated fluid, the colour changes from arterial to venous, exactly in proportion as the salt of the blood oozes out of the clot; and if the water be repeatedly changed, the clot, after a short period, becomes quite dark. Hence it is evident, that black is the natural colour of the hematosine. When we remove the black clot from the distilled water, it does not become red on exposure to the air; in fact, it remains black, even in an atmosphere of pure oxygen: but when we immerse this black clot in a clear saline fluid, the colour changes instantly from black to arterial, even when the water has been previously deprived of its air by the action of the air-pump. From these and other facts, it is very obvious that it is not oxygen, but the natural saline matter of the blood, which is the essential agent in the change of colour from venous to arterial. It is more easy, however, to prove the inaccuracy of an existing hypothesis than to bring forward a new theory, however correct it may be, against which critical ingenuity cannot devise some plausible objections.

It is admitted by Dr. Gregory and Mr. Irvine, that a strong saline solution changes the colour of the blood from venous to arterial, without the contact of oxygen, or indeed of any gas whatever; but on the same authority it is stated, that the serum, which is a dilute saline fluid, or water of the same saline strength as the serum, produces *no effect whatever, without the presence of atmospheric air*. This, if it be one, is the only plausible objection that has yet been brought against the new theory of respiration; but I trust to be able to prove that there is a fallacy in the experiments referred to, which renders the conclusion which has been drawn from them of no value.

It has been proved by numerous experiments, that it is not the iron, but the saline matter of the blood, which is the cause of its red colour, even in the venous circulation. We have seen, also, that when the blood is deprived of its saline ingredients, it becomes perfectly black; and, with the exception of these, there is not one of the other ingredients

* The blood which was used in my own experiments had been arterialized by exposure to the air. That which was used by Dr. Turner was taken from the femoral artery of a dog, (*See* Turner's Chemistry, 4th edit. p. 903.)

of the blood which is capable of striking a red colour with the dark hematosine. We have reason to believe that there is not one particle of free oxygen in the venous blood; yet even the venous blood has a dark *red* colour, which would not be the case if the salts in the serum had no effect whatever, except when in contact with atmospheric air.

It may not be superfluous to observe, that the exact quantity of salt in the blood has not yet been ascertained with sufficient accuracy. When the solid ingredients are exposed for hours to a red heat, it is probable that a part of the salt is also driven off with the animal matter; but, even by this rude process, at least ten grains of dried alkaline salts are obtained from 1000 grains of blood; or, in other words, each ounce of blood contains about five grains of salts; and it appears from the following experiments, that this quantity is sufficient, without oxygen, to account for the red colour of the venous, or the scarlet tint of the arterial blood.

About two ounces of serum were exposed to the action of an air-pump, until as much of the air was removed as it was possible to obtain from so dense a fluid. A black clot which had been soaked for hours in distilled water, was immersed in the serum; it changed in a short period from black to a bright red colour, although the pump was again almost immediately used to expel any small portion of air that might have entered with the clot.

It is very difficult to remove the whole of the serum from the clot, and equally so to remove the whole of the air from the dense serum. The clot which was used in the above experiment was quite dark; it is possible, however, that it still contained a minute portion of salt; but to avoid this objection, a portion of blood was allowed to remain undisturbed for twenty-four hours. The serum was then carefully poured off; a number of small pieces were cut from the centre of the crassamentum, and soaked in distilled water, which was occasionally renewed, until the water, which was poured off, lost all trace of saline matter. The clots were again washed in distilled water; they were afterwards exposed for some time on bibulous paper, and then used in the following experiments.

Ten grains of dried muriate of soda were dissolved in 1000 parts of distilled water. This was boiled for a few minutes, and allowed to cool in a close vessel: it was afterwasds exposed to the action of the air-pump, and only a few globules made their escape. A clot was then introduced into this exhausted fluid; the pump was again immediately used; but notwithstanding this, the clot changed almost instantly from black to bright red. The clot was allowed to remain upwards of an hour in the exhausted receiver; it was then removed and placed beside one of the clots which had been taken from the distilled water. This was quite dark, but the former had a bright arterial colour, even at the moment when it was removed from the weak saline fluid. If the brightness increased at all on exposure to the air, the increase of colour was scarcely perceptible.

In the next experiment a solution containing ten grains of bicarbonate of soda, was dissolved in 1000 parts of distilled water. The result was exactly the same as with an equal portion of muriate of soda; it was also similar when a solution was used containing six grains of the muriate, two grains of the bicarbonate of soda, and two grains of the muriate of potass.

A solution containing eight grains of the muriate of soda produced a decided change of colour. The tint was not so strong as in the former experiments, but still when the clot was removed from the water, it was nearly as florid as arterial blood. I may add, that an increase of redness was produced, even when a solution was used which contained only five grains of the muriate of soda. These experiments were made in the laboratory of Mr. Squire, of Oxford-Street, by that gentleman and myself; and those who will repeat them, will scarcely admit that "a weak saline solution has no effect whatever, except when in contact with atmospheric air."

The following experiment was made during the early part of last year, in Copenhagen, by Professor Forchammer and myself:—

About four ounces of venous blood were conducted from a vein in the arm, through a tube, into an inverted glass filled with mercury. When the blood rose to the top, and had displaced an equal volume of the heavier fluid, the blood appeared through the glass to be almost black. We then introduced about one ounce of a solution of salt.

As soon as this came into contact with the lower portion of the colouring matter, there was a decided increase of redness; but still it was far from being equal to the arterial colour. From this we may infer, that so long as carbonic acid, which is the cause of the darkness of the venous blood, remains in immediate contact with the colouring matter, it is difficult, even with an increased quantity of salt, to counteract entirely its darkening effect. On the other hand, when a clot has been immersed in distilled water, the colour is black merely from the loss of its saline matter. In this case a very weak saline fluid is sufficient to produce the arterial colour, even when the vessel which contains the clot is immersed in an atmosphere of pure carbonic acid *.

Carbonic acid, hydrogen, and nitrogen, darken the colour even of arterial blood; and so long as these agents remain in direct contact with the hematosine of venous blood, a stronger saline fluid is required for the purpose of changing the colour from venous to arterial. It is not fair, however, without making a due allowance for the difference, to draw a comparison betwixt what takes place out of the body, in an air tight glass over mercury, from which the blackening gaseous agent cannot escape, and the changes which occur in the respiratory organs, where the blood is freely exposed to the action of the air. Had Dr. Gregory and Mr. Irvine attended to this difference, they would have drawn a different conclusion from the result of their experiments.

When the impure blood is circulating in the venous system, the carbonic acid, the cause of the darkness, is in immediate contact with the colouring matter, consequently the natural quantity of salt in the blood is not more than sufficient to produce a faint redness; but when this dark blood arrives in the lungs, and the acid is removed, the same quantity of salt appears, from the above experiments, to be quite sufficient for the purpose of producing the arterial colour.

In the paper referred to, it is stated that the colour of the blood became arterial when the clots were exposed to the action of the air. This, however, is not a positive proof that the change was produced by the addition of oxygen; for this may have been owing entirely to the sudden removal of the darkening gas *.

From the facts which have been stated, it must be obvious to all, that the experiments are not conclusive which were said to render a modification of the new theory of respiration "somewhat necessary." A modification may be necessary, but certainly we are not led to this by the result of any of the objections or experiments hitherto published.

In my late work on the blood, if I have dwelt too much on the reddening which is produced by the removal of the acid, it was partly because this circumstance had been previously entirely neglected. If, on the other hand, I attributed too little to the change of colour supposed to be produced by the addition of oxygen, it was partly because this had hitherto been considered as the sole cause of the arterial colour.

I may have erred in attaching too much importance to improvements which I then believed, and still believe, to be entirely my own; but by referring to my work on the blood, you will find that I had some doubt on this subject, even at the period when that work was published. I have stated in the tenth page,

* Carbonic acid, which is a necessary constituent of venous blood, is said by my friend Dr. Clanny, to be the mainspring of life; but if so, why is it so destructive when allowed to enter the arterial circulation? When the colour is dark merely from the diminution of its saline ingredients, as in the last stage of the yellow fever, cholera, &c. such blood may pass into the arterial system with comparative impunity, and maintain life for twenty-four hours. When blood, however, which contains carbonic acid enters the arterial system, even though it be less dark, it acts as a poison, and causes almost instant death. It has been ascertained by Dr. Edwards, that the young of certain animals do not begin to generate animal heat for several days after they are littered. During this period it is probable that there is very little, if any, carbonic acid in the venous circulation; consequently this blood can circulate in the arterial system with such comparative impunity, that these animals may be repeatedly immersed under water, even for half an hour at a time, and still live. As soon, however, as they begin to evolve their own heat; or, in other words, as soon as carbonic acid is contained in the venous blood, this power of living under water is entirely lost.

* A late writer, O'Shaughnessy, has denied that carbonic acid blackens the blood; but the fallacy of this can easily be proved by a very simple experiment. When we almost saturate distilled water with common salt, and mix this with recently-drawn venous blood, the colour changes almost instantly from venous to bright arterial: when we pass a stream of pure carbonic acid through this, it suddenly becomes nearly black: when this is exposed for a short time to the air, it again recovers its scarlet colour, and the experiment may be repeated as often as we please.

"That the cause of the scarlet colour exists in the blood independent of oxygen; or, at all events, *oxygen of itself* cannot produce either the red or arterial appearance;" and this appears to come near to the truth, for as a reddening agent, oxygen of itself is perfectly inert.

We have seen that even pure oxygen can no more produce redness in blood that has been deprived of its saline matter, than it can give an arterial colour to common ink. Hence we may infer, that the essential cause of the arterial colour exists in the blood; and were it not that the serum is a saline fluid, there would not be any change of colour in the pulmonary organs. The addition of oxygen *may* have some effect in the change of colour from venous to arterial, but so far from being the sole, it is a mere secondary agent. I have proved that the quantity of salt contained in the blood *can* produce the arterial colour, without any assistance from the atmospheric air; but still it is possible, when the acid is removed in the pulmonary organs, that the oxygen which enters may produce some change in the hematosine which renders it more easily reddened; or what is more probable, a part of the oxygen which enters may combine with, and produce some change in, the saline ingredients, which may enable them to act upon the colouring matter with more force: but at the same time that I suggest the possibility of this, still I must repeat that *oxygen of itself is perfectly inert*; and if it does produce any effect, it can only do so *when the colouring matter is in immediate contact with a saline fluid.*

It is admitted by all, that carbonic acid is evolved from the lungs, and also that oxygen is absorbed. Lavoisier was the first, however, who affirmed that carbon was the cause of the dark colour of the venous blood. According to his hypothesis, when this impure or carbonized blood arrives in the lungs, the carbon combines with the oxygen of the air, and forms carbonic acid, which is then expelled in the process of expiration. Such is the hypothesis which, until very lately, was almost universally received; but the result of some recent experiments renders this view of the changes which occur in the lungs altogether untenable.

When Dr. Edwards forced some small animals to breathe in an atmosphere of hydrogen, a quantity of carbonic acid was evolved, in some instances equal in bulk to the size of the animals. These experiments were looked upon by many as almost decisive of the existence of that acid in the venous blood; but others consider them of no value, for they allege that there may have been oxygen sufficient in the pulmonary cells to account for the formation of the acid; consequently they are not willing to admit that it existed in the venous current. To avoid this objection, the following experiment was performed during the early part of last year, in the Polytechnic School of Copenhagen, by Professor Forchammer and myself.

A few ounces of venous blood were drawn from the arm, through a tube, into a glass vessel filled with pure hydrogen; the carbonic acid was expelled from the blood by a gentle heat, and though the volume of hydrogen was large in proportion to the small quantity of blood, still the gas which passed over was sufficiently impregnated with carbonic acid to give a milky appearance to lime-water.

In another experiment, the blood (after having been well agitated with hydrogen) was allowed to remain in the glass for half an hour. The hydrogen was then displaced by pouring a quantity of pure mercury into the vessel, and the result was the same as in the last experiment.

Since my return to England, I find that a similar experiment has been performed by Mr. Hoffman, of Margate; which is thus described in the twenty-sixth number of the Medical Gazette for last year:—"Blood taken from a vein of the arm was received in a phial of pure hydrogen gas; great care being taken to prevent access of atmospheric air. After agitating this blood, in contact with hydrogen gas, the gas was found to render lime-water turbid, and not to be inflammable; shewing the presence of a considerable quantity of carbonic acid gas, which the hydrogen had attracted from the venous blood."

In the following experiment, which was made lately, I was assisted by a gentleman who is one of the best practical chemists in London, and consequently well calculated to judge of its accuracy.

A double-necked pint bottle was filled with pure hydrogen, and the tubes which were connected with each orifice

were also filled with the same gas. The orifice of the ascending bent tube was placed on the skin near to the bend of the arm, and, when the vein was opened, the orifice of the tube was slid along the skin until it covered the incision which had been made with the lancet. The blood was conducted through the tube into the bottle. In proportion as it entered, a part of the hydrogen passed through the descending tube, the orifice of which was immersed in distilled water. When the bottle had received about five ounces of blood, both the orifices were completely closed, and the blood was agitated with the hydrogen for about five minutes. Before examining the air, the bottle was allowed to rest for about half an hour, so as to give the hydrogen sufficient time to attract the carbonic acid. The orifice of the descending tube was then immersed, and uncorked, in barytic water. This, in connexion with the bottle containing the blood, was then placed in the receiver of an air-pump. In proportion as the air was removed by means of the pump, the gas which was over the blood passed through the descending tube into the barytic water; and in this experiment the hydrogen was so strongly impregnated with carbonic acid, that even the first few bubbles instantly caused a dense cloud in the water. From the above result it is very obvious that the venous blood is saturated, even in the extremities, with "ready-made" carbonic acid; consequently we can no longer believe that this gas is formed in the pulmonary organs.

The oxygen which appears to exist in the arterial blood, chiefly in a free state, is evidently derived from the external air; and the carbon is probably produced by changes which occur in the capillary system. These agents are essential to life—particularly in warm-blooded animals; but the union of carbon with oxygen, a process which was lately supposed to purify the blood, is in reality the cause of its impurity. There is, in the living body, an incessant combination of carbon with oxygen, and consequently a constant evolution of animal heat*, together with the necessary formation of carbonic acid. This change, however, is evidently effected, not in the lungs, but in the intermediate structure which exists all over the system between the arterial and venous circulations. It is in this texture that the blood ceases to be arterial and becomes venous, partly from the loss of its oxygen, and partly from the addition of carbonic acid. When this impure blood arrives in the lungs, it is changed from venous to arterial, partly by the sudden removal of the carbonic acid, and partly by the instantaneous addition of pure air*.

Lavoisier appears also to have been the first who believed it possible that the union of carbon and oxygen might occur in the great circulation; but he afterwards abandoned this for the other hypothesis. La Grange gave a decided preference to the theory which supposes the acid to be formed in the great circulation; but both La Grange and his followers erred in supposing that this union commenced in the left auricle of the heart, and continued until the blood arrived in the right ventricle. Dr. Edwards has made experiments to prove, that the oxygen which enters the arterial blood combines with the carbon in the great circulation; but at the time his late work was published, he appears not to have made up his mind with respect to the manner, or the exact spot where this combination takes place. He says, "With respect to the oxygen which is to contribute to the formation of the carbonic acid contained in the mass of the blood, one of two things must happen: it enters into combination either suddenly or slowly. In the latter case there will be oxygen to excess, circulating in the mass of the blood. This pure oxygen will therefore be subject to exhalation, which will take place in the organs adapted for giving passage to it, as happens in fishes, in the air bladders of which animals oxygen is found." From this, as well as other parts of his work, it is evident that Dr. Edwards at

* The quantity of animal heat that is evolved, appears to depend partly on the degree of difference in quality betwixt the arterial and venous blood, and partly on the rapidity with which this blood circulates through the extreme texture. In the fœtus there is very little, if any, difference of quality betwixt the arterial and venous blood; and we have reason to believe that the fœtus does not possess the power of evolving heat until it begins to breathe: until this period, the blood seems to be used almost entirely for the formation of the solids.

* Dr. Bostock, in his valuable work on Physiology, states, "That we have no proof of the existence of any gas in the blood." When we subject, however, either the arterial or venous blood to the action of an air-pump, we have sufficient evidence that both these fluids contain a very large portion of air.

that period had not come to any decision on this subject. The carbonic acid, however, is not contained in the mass of blood: it exists only in the venous circulation; for the blood even in the most minute arteries of the extremities is as highly arterial as it was when it left the left side of the heart. On the other hand, the blood is highly venous, even in the most minute veins. From this it is obvious that the carbon does not combine with the oxygen either in the arterial or the venous circulation; and for the same reason it is equally clear that the animal heat is evolved, and the carbonic acid is formed *only in the intermediate structure all over the body where the arterial circulation ends and the venous begins.*

It is well known that the impure blood and the atmospheric air do not come into immediate contact with each other in the lungs. To what, then, are we to attribute the removal of the heavy carbonic acid from the blood? Spallanzani believed that the acid was exhaled from the lungs; and Dr. Edwards has adopted the same opinion. It is very obvious, however, that if this process were left to mere exhalation, the carbonic acid would remain in the blood and cause death.

We may give to the phenomenon any name that we please; but there is now no question of the fact, that oxygen possesses the power of attracting or lifting up carbonic acid even through the medium of a dense membrane *. When the impure blood arrives in the lungs, it is this power which enables the oxygen to remove the heavier gas, and to diffuse it almost instantly in the general atmosphere.

The absorption of oxygen is still considered by many as the first change in the process of respiration. It is very evident, however, that a fluid containing carbonic acid cannot absorb oxygen until a part at least of the acid be removed. When a tumbler containing carbonic acid, with a moist membrane firmly tied over it, is placed in an atmosphere of pure oxygen, the acid is removed faster than the oxygen can enter, and the membrane at first becomes concave. From this alone we may infer, that the removal of the acid is the primary change in the respiratory process; in proportion as this is effected, the space which was occupied by the acid is instantly replaced by at least an equal volume of pure air. This purified fluid follows the current. The existence of oxygen in arterial blood has been ascertained by experiment: but experiments are not necessary to prove this; for if there be one word of truth in the great discovery of Harvey, the blood which has received the oxygen must pass on to the arterial circulation. There may be other minor changes; but if the new theory be correct, the essential difference betwixt the arterial and the venous blood is, that *the former contains pure air and the latter carbonic acid.*

From the facts which have been stated in this paper, in addition to those which are contained in my work on the blood, the following conclusions may be drawn:—

1st, That the animal heat is evolved, and the carbonic acid is formed, not in the general round of the great circulation, but in the capillary system all over the body, where the arterial circulation ends and the venous begins.

2d, That carbonic acid is the chief cause of the dark colour of the venous blood.

3d, That when this impure blood arrives in the lungs, the removal of a part at least of the acid is the first change that occurs in the process of respiration.

4th, That the acid is removed by means of a power of attraction which oxygen possesses for this gas.

5th, That the salt in the blood is the essential agent in the change of colour from venous to arterial *.

I am, sir,
Your obedient servant,
Wm. Stevens, M.D.

Albany-Street, Regent's-Park,
March 27, 1834.

* See my late work on the Healthy and Diseased Properties of the Blood, p. 71.

* I know but one respectable writer who still resists this conclusion. Dr. Paris, in the last edition of his Pharmacologia, states (see p. 135), "That the new views regarding the conversion of venous into arterial blood cannot be supported but at the expense of every received opinion. We are called upon to level with the dust the edifice which has been raised by the joint labours of our most distinguished philosophers," &c. &c. This defence, however, may be set up in favour of every established error. Such are the arguments that have been used, and with equal propriety, against almost every real improvement that has been made in the profession. It was this sort of logic which retarded for a time the admission of the discovery of the circulation of the blood. But though such objections passed current in the time of Charles the First, they will not be received now by any who are capable of reasoning for themselves. The stately "edifice" which was lately looked

CASE OF

FRACTURE OF THE PELVIS, &c.

WITH CONCUSSION OF THE BRAIN; SUCCESSFULLY TREATED.

To the Editor of the Medical Gazette.

Sir,

If the following should be deemed worthy of insertion in the Gazette, you will oblige me by letting it appear in an early number. The case is narrated, not from any thing novel in the mode of treatment, but because it serves to illustrate the extraordinary power which the miners in our neighbourhood possess of surviving the shock, and overcoming the severe injuries, consequent upon accidents, to which, from the nature of their employment, they are so peculiarly and constantly exposed. As a passing remark, it may be stated, that the accidents we are daily called upon to witness among the colliers are in general followed by considerably less constitutional irritation than those of a similar description usually met with in the metropolitan hospitals, occurring to the persons of coal-heavers and porter-brewers; but this probably may be said to originate in the vast difference which exists between them as regards their habits and method of living.

There is one feature, however, in the accompanying case, which renders it highly interesting; and that is, the slight degree of visceral derangement which supervened upon the fractured condition of the pelvis. As the bladder and intestines, from their relative situation within the parietes of its bony structure, are so liable to be lacerated when the local mischief is extensive, it cannot be wondered at, that recovery, under such circumstances, should be of so comparatively rare occurrence; but, in the succeeding history, it will be seen that the viscera must have entirely escaped, as no important symptoms appeared in the progress of the case but what might be clearly attributed to the severe shock which the brain and nervous system generally sustained.

With every wish for the continued prosperity of your journal, permit me to subscribe myself,

Your very obedient servant,
Charles Thornhill.

Darlaston, Staffordshire,
March 17th, 1834.

On the morning of the 6th of November, between twelve and one o'clock, I was requested to visit Thomas Foster, ætatis 17, who had met with a severe accident in one of the neighbouring collieries. The poor youth had been remaining to work over time, in order to procure a sufficient supply of money to buy him a new jacket, and whilst engaged at the bottom of the pit-shaft, in preparing the skip for its ascent, his clothes became entangled with the chain-hook, which, after dragging him to the height of twenty-five yards, suddenly let go its hold, and precipitated him to the bottom. He was immediately brought up out of the pit, though in an apparently lifeless state, and conveyed home; but manifesting signs of vitality whilst on the way, the men who accompanied obliged him to drink a glass of brandy, for the purpose, as they affirmed, "of enlivening his spirits." Having entered the apartment, I perceived him lying upon the bed in a state of *partial* insensibility. His pulse was feeble and intermitting, and his respiration laborious: his extremities were cold, and the surface of the body generally was deprived of its natural temperature. On being roused, he answered in an incoherent manner to

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upon with so much veneration, is evidently built upon a sandy foundation, and destined to fall. If I mistake not, *there are other received opinions which will soon follow*, much to the benefit of science, as well as of humanity.

Dr Paris is generally allowed to be a man of first-rate talents, and I have no wish to detract from his merits; but I must say that he suffers his prejudices, in favour of the opinions of "distinguished philosophers," to go rather too far, when he stigmatizes, as "an air-built castle," a theory which is supported not only by the most incontrovertible facts, but also by the approbation of many of the very first physiologists of the present day. Even Dr. Gregory and Mr. Irvine admit that the salt of the blood is essential to the change of colour, from venous to arterial; and so will Dr. Paris, when he leaves the decision of this question to *the test of experiment*, in place of referring it to the *opinions* of distinguished philosophers.

I have been censured, also, for the remarks which I have made with respect to scurvy; but if I had scarcely seen this disease previously to the publication of my work on the blood, I have since visited that part of the world where scurvy is perhaps more prevalent than in any other. After having seen this disease on a large scale, so far from being induced to make any retraction, I am now in possession of the most unequivocal evidence to establish the accuracy of the opinions which I have formerly advanced upon this subject; and the fear of offending "distinguished philosophers" will not prevent me from laying this evidence before the public.

any question that was proposed. Though he appeared unconscious of pain, when moved from one part of the bed to the other, yet he occasionally manifested uneasiness whilst I was examining and dressing his wounds. The face was much tumefied, the left cheek partially lacerated, and an extensive wound was seen in the muscles of the left forearm. From evident shortening of the limb, and eversion of the foot, a fracture was also discovered, on inspection, to exist in the left thigh; but since, from the lateness of the hour, there was difficulty in obtaining bandages, and as there seemed but little probability that he would survive the shock which the constitution had sustained, I contented myself merely with drawing together by suture the lacerated parts, dressing them in the usual manner, and leaving him for the remainder of the night.

At day-break I had the pleasure of meeting two or three professional friends, who had been invited to see the case. The patient was still labouring under the effects arising from concussion of the brain. The pulse was irregular; and although increased in frequency, was deficient in power; yet warmth had been universally diffused over the whole body. After making a minute and careful examination, there was found to be an oblique fracture of the lower jaw, on the left side, near to the symphysis; a fracture of the head of the humerus on the same side; and a double fracture of the thigh; the upper fracture extending through the cervix femoris and into the trochanter major, and the lower running transversely through the shaft of the bone, about one-third above the condyles. Added to these, there was fractured pelvis, to which our attention was primarily directed, in consequence of observing considerable contusion and ecchymosis about the perinæum and the integuments thereto adjoining. The fracture, as far as we could ascertain, ran directly through the crista of the pubes, continuing its course towards the left acetabulum, and terminating in an oblique direction through the upper portion of the ramus ischii. When the limb was rotated, a distinct crepitus could both be heard and felt.

Having effected reduction, the fractured thigh and jaw were dressed in the ordinary way, and the pelvis was encircled with a strong linen bandage; but as there was considerable tumefaction about the shoulder, this latter was placed upon a pillow, and directed to be constantly wetted with a diluted solution of the plumbi acetas. The following mixture also was prescribed.

℞. Spiritus Ammoniæ Aromat. ℥iss.; Confect. Aromat. ʒij.; Misturæ Camphoræ ℥iv.; Aquæ Puræ ℥ij. M.; Capiat cochlearia tria magna 3tia vel 4ta quâque horâ.

Three P.M.—The patient has evinced symptoms of uneasiness since he was dressed; sensibility, however, has not been restored, though he has recovered sufficiently to swallow a few tablespoonsful of gruel. Whenever he is addressed, he speaks in a confused manner.

Eight P.M.—Continues to lie in a comatose state, and when spoken to, is roused with great difficulty. There has been an involuntary discharge of urine twice or three times during the afternoon. Pulse quick and irritable; and the skin is inclined to be heated.

7th.—The night has been passed more comfortably than was expected; nevertheless, it has been characterized by frequent tossings to and fro about the bed. The bandages have slipped from the head, so that their reapplication has been rendered necessary. Pulse rapid and vibrating; and the abdomen is rather tense and tumid. He has made water during the night, but as yet there has been no digestion.

℞. Infusi Sennæ ℥iiiss.; Magnesiæ Sulphatis ℥j.; Misturæ Camphoræ ℥ij.; Tinct. Sennæ ℥ss. M. Sumat partem quartam sextâ quâque horâ donec alvus benè soluta fuerit.

Six P.M.—Although he was drawn pretty high up in bed at the last visit, yet has he, from constant jactitation, gradually slidden down to the old position. There has been at times much low muttering delirium; but during the periods of consciousness he has frequently inquired for something to drink. Since the morning he has taken a pint of gruel, in small quantities frequently repeated.

8th.—Hitherto there has been no evacuation from the bowels, but there continues to be a plentiful discharge of the urinary secretion. His mother tells me, that in the night he for the first time gave indications of restored sensibility and consciousness, and began to interro-

gate her as to the cause of the accident. He had not the slightest recollection of its occurrence, or how he was occupied previous to its taking place. The tongue is furred and dry; the pulse is quick; and the abdomen feels hard and tumid.

℞ Hydrar. Submur. gr. iv.; Pulv. Jalapæ, gr. xij. M. ft. Pulvis statim sumendus.

8 P.M.—The patient has experienced much relief from having had two copious dejections. There has been a slight return of pain in the head, accompanied with occasional aberrations; but upon the whole he is better, and may be said to bid fair for an ultimate recovery.

9th.—He has been frequently heard to ramble during the night, and has not had much settled sleep. Complains of severe pain in the shoulder and hip, the former of which appears a good deal tumefied. There is also much pain and uneasiness about the bony structure of the pelvis. The wounds in the face and arm, when dressed, looked kind, but were suppurating,—no part having healed by the first intention. This latter circumstance, however, was no more than could be expected from his restless state, and the constitutional irritation under which he laboured. Pulse frequent; tongue loaded with fur; urine high coloured; and a troublesome cough, may now be ranked among the list of symptoms.

Mist. Salin.

14th.—Having had several hours of sound sleep, he expresses himself as greatly refreshed. He says his cough is better, and his shoulder pretty free from pain; there is still some degree of swelling, attended with considerable heat of surface.

15th.—The case continues to go on favourably, and no unpleasant symptom has occurred to retard the curative process. Swelling of the shoulder has now entirely subsided, and splints and bandages have been substituted for the lotion. The ligatures have come away from the wounds in the face and arm, which are healing as fast as possible.

23d.—Perfect union having been pretty nigh accomplished, the four-tailed bandage was removed from the jaw, at the request of the patient. There is no overlapping of the bone, but a slight projection just along the alveolar processes; and this latter is only perceptible by minutely examining the interior of the mouth. The wound of the cheek has healed up.

Dec. 29th.—To-day he has been carried down stairs for the first time since his accident, which has contributed in no little way to his joy and comfort. He has the entire use of his arm and shoulder-joint, and can perform their various motions with perfect freedom. The thigh appears about an inch shorter than that of the sound side, but he can raise it without assistance. He experiences some difficulty in sitting up, from the weakness still felt about the pelvic apparatus. The pelvis is at present supported by the circular bandage.

Jan. 29th, 1834.—He has managed to walk up to my surgery with the aid of his crutches; but he complains of weakness in the pelvis, and particularly about the acetabulum of the injured side, though he states that he feels his strength to be increasing rapidly.

March 12th.—To-day he has walked to the surgery without his crutches, having substituted for them a stick, for the purpose of keeping himself upright.

BLOODLETTING BY THE *POLYTOME*.

To the Editor of the Medical Gazette.

Sir,

Having in some observations on local bloodletting, published in the Dublin Medical Journal, described an instrument which I proposed to call the Polytome, and which had for its object to make superficial incisions for the purpose of drawing blood, I have been much gratified by learning that a Mr. Thompson, of Whitehaven, had previously devised and used a similar instrument; and that the result of his experience was such, as to induce him to prefer it to the common box scarificator. As, however, it seems fated that doctors must differ, I have to notice a communication from Mr. Knox, in a late No. of the Lancet, in which I find the following objections:—

1. That the operation is slow and painful. 2. That if done quickly, there is but little control over the length and depth of the incisions. 3. That if employed on a restive patient, it will be

extremely difficult to use. 4. That in inexperienced hands it would prove mischievous and disfiguring.

1. The slowness and painfulness. A cutting instrument is not either quick or slow, except as it is rendered so by the operator. Does Mr. Knox mean to assert, that an incision can be made in a shorter time by the action of a steel spring, than by the hand of the operator? If so, he must be prepared to defend the use of spring bleeding phlemes in preference to lancets, in the operation of venesection; and if it be an object to diminish pain, that can only be effected by dividing as little of the integuments as possible, which desirable object is attained by the polytome making linear and not circular incisions, and also by the cutting edges being kept in the best order, which is rarely done in the box scarificator, owing to the trouble of taking it asunder. The shock of several blades penetrating the skin to a considerable depth, impelled by a spring, is both more abhorrent to the imagination, and more painful to the feeling, than the light touches of lancets moved by the hand can ever be.

2. The want of control over the length of the incisions cannot be objected to the polytome more than to any other cutting instrument used in surgery. The length depends on the operator alone; and, as has been well observed by Mr. Thompson, the person is incapable of following the profession who cannot make a short or a long incision as the case may require. The depth can be regulated by holding the instrument under the fingers at a certain angle; and this circumstance deterred me from recommending the addition of a guard for that purpose, as was at first suggested. Mr. Thompson, of Whitehaven, however, I find, strongly recommends the guard; and some polytomes have lately been made by Mr. Thompson, cutler, in this city, which leave nothing to be desired. They are furnished with guards, which are raised or depressed by means of a screw and spring, so that the depth to which the lancets are to penetrate can be regulated in the most exact manner. Those instruments are also easily taken asunder, and may be kept clean without trouble.

3. In case of restiveness on the part of the patient, the polytome clearly has the advantage, inasmuch as it requires the use of only one hand, and leaves the other free to restrain the patient; while the box scarificator requires both hands, and also requires a considerable pressure to prevent it from being raised up when the phlemes are in the act of penetrating the skin. In the case of children, or the insane, the polytome can also be concealed in the hand even at the moment of using it.

The fourth and last objection—that in inexperienced hands it would prove mischievous and disfiguring—applies with more force to the box scarificator, inasmuch as its incisions must necessarily be deep; while those of the polytome are always superficial. With respect to both instruments, as well as to cupping in general, the value of experience cannot be estimated too highly. Those who have witnessed Baron Larrey making superficial incisions with only one blade, for the purpose of drawing blood by means of cupping glasses, can readily appreciate the celerity and certainty with which the appropriate incisions can be made by the polytome. The improvement in the use of the instrument attainable by experience, instead of being an objection, ought to lead to a more patient trial of it, and prevent it being condemned in a hasty manner.

With these observations I beg to recommend it to the candid judgment of your readers.—I am, sir,

Your obedient servant,

JONATHAN OSBORNE, M.D.

63, Harcourt-Street, Dublin,
April 4, 1834.

ROMAN CLINIQUE,

DURING THE WINTER OF 1833-4.

BY DAVID BADHAM, M.D.
Radcliffe Travell. Fellow, &c. &c.

To the Editor of the Medical Gazette.

SIR,

A FEW observations on the hospitals at Rome may not be unacceptable to your readers; and a short account of the mode of practice adopted here, made by a person on the spot, may not even be without its use to the over-bold in the use of the lancet. Sto. Spirito is the principal hospital at Rome: some idea of its capacity, in the only sense in which that capacity is interesting to the physician, will be better conveyed to the reader by a glance at the following

Table, than by a more lengthened description of the length of its wings or wards. I say nothing at present of the Foundling Hospital or Lunatic Asylum, which are connected however with the building. The report is for last year.

On 1st of January, 1833.	Rimasti.	Entrati.	Total.	Left Cured.	Died.	Rimasti at end of the past Year 1833.
	Old Cases.	New Cases.				
Ward of "Febbricitanti"	396	9437	9833	8944	781	208
Surgical ward	35	416	451	346	69	36
Ward for the "Operati"	5	13	18	14	2	2
Phthisical ward.....	11	107	118	19	92	7
Scorbutic ward	3	25	28	22	3	3
Clinical ward.....	10	280	290	266	15	9
Chronic case ward....	28	196	224	46	157	21

This hospital does not receive women. The fact of the chronic cases being so few, does not arise from any scarcity of such cases, but because the great hospital St. Jacomo in the Corso, called the hospital of the incurables, is appropriated to this purpose. Dr. Tagliabò is at present clinical physician, as they change every year: the number of pupils who go round with him is generally about fifty; I believe there are eighty whose names are registered. The way of taking down cases is very good: a printed paper is put into the hands of the clerk, and this paper is divided into columns, each column being headed by some word which suggests the questions in the order which they ought to be put. *Eta — Costituzione — Girone di vita—Dimora—Morbi precedenti—Epoca della malattia—Causè manifesti—Fenomeni de' corsi—Rimedi usati—Denomenàzione della malattia—Malattie regnanti—Qualita di stagione.* Such are the questions. A last column is left open for any clinical observation that may occur from day to day during the progress of the disease; and on the back of these papers an account is given of the autopsy, if the patient die. Dr. Tagliabò is a sensible and very learned physician,—a little too much addicted, perhaps, to the ancients; but he preaches very good sermons from the scriptures of Hippocrates and Celsus. The complaints have been inflammatory, almost without exception; so that the clinical ward has been filled with them: the organs primarily affected, almost without exception, have been the respiratory ones; though metastasis to the intestines, or the head, have been not very unfrequent. What a field is here for studying accurately the various affections of the chest!—yet, will the reader believe, that here (where its employment would be so useful) the stethoscope is wholly unknown, and percussion never, by any accident, had recourse to? Need I say, then, that accuracy of diagnosis is not ever attempted; and that pneumonia, pleurisy, and bronchitis, are all jumbled together in the word "*polmone.*"

It is but a few days since I heard a clinical lecture on hydropericardium, and the difficulty of distinguishing it from hydrothorax! Taking a case at present in the Clinique, he proceeded to show, and truly, the insufficiency of any of the signs laid down by Testa, Krysig, Corvisart, &c., but said not a word about percussion or auscultation, though it must have struck any one who had given the subject a moment's reflection, that even the first of these means, practised in different positions of the body, must, in nine cases out of ten, settle the question at once. There has been a strong tendency towards suppuration in the acute affections of the lungs this year: a small vibrating pulse, with remission of fever towards morning, has not unfrequently continued even beyond the fourteenth day; but this has not always followed the aphorism of Hipporates, and ended in empyema or abscess. Dr. T. remarks, that, as a general rule, if the cough be dry beyond the fourth or fifth day, the case generally terminates fatally. He views the

character of the expectoration as the best evidence of the state of parts; and whenever it begins to get thick, concocted, and opaque, he pronounces his patients safe, however the pulse or other symptoms may be; sometimes a pleuro pneumonia of the lung has produced suppuration in a distant part of the body. We have had three cases of pneumonia in which the brain has suffered; one terminated fatally: copious epistaxis has been critical with three, nature kindly performing what the doctor was afraid to perform, and snatching the patients from the jaws of death: a diarrhœa has cured another, contrary to the aphorism,—"ὕπο πλευριτιδος, η υπο περιπνευμονιης εχομενω, διαρροιη επιγιγνομενη, κακον;"—but Hippocrates means a violent, not a gentle fluxion, of the bowels. Aretæus says, bilious stools to those who have bilious sputa, are good, (*vide* Pneumonia in Aret.) Bilious here means the rusty-coloured expectoration of pneumonia, I suppose. I have made a general remark, that in all the cases where blood has been abstracted, (and much has been abstracted within these four months, the cases being all inflammatory) the buffy coat has been rarely present, and only in one instance of considerable thickness and firmness;—query, may not the Italian mode of living, their vegetable diet, excesses, inactivity, and the want of hardiness of their frame, be productive of a blood in which the serum is greater in proportion to the red globules and fibrine, than with us, who live different lives? Though wholly averse to the present mode of venesection in Italy, yet it cannot be doubted, I think, that if this were practised upon even their most plethoric patients, as it is with us, an alarming depression of the powers of life would succeed, if indeed the vital spark were not quite extinguished. Nothing can be more nervous than the Italian doctor, unless it be the patients whom they have to treat. I have known the pulse of a convalescent man rise in frequency, and come thumping against your finger, as Dr. T., "*magnâ comitante catervâ*," entered the ward, and fall in no time when he had passed on to the next bed.

I was surprised when first I began my attendance at the hospital, to hear a great many affections called rheumatic, which appeared to me to have nothing in common with that complaint. I have gradually collected, in conversations with Dr. T., that by rheumatism the Italian doctors understand a first stage of inflammation—an erythema, as it were, of any part or texture of the body; so that they have a gastric rheumatism, a rheumatism of the mucous membrane of the lungs, of the peritoneal coat, and all the other coats of the intestines, as well as of the muscles and joints. Pleurisy, &c. are therefore only removed from the head of rheumatic affections when violent; it has then passed the rheumatic stage of inflammation.

We have just had an interesting case of Italian rheumatism. A man came into the hospital with a violent pain in his head: an encephalitis was apprehended; he was bled, and treated *à l'antiphlogistique*. Suddenly the pain left his head, and attacked his chest, and all the symptoms of a pleurisy set in, in their turn succeeded by peritoneal inflammation; then came strangury and inflammation of the bowels; and lastly a general dropsy supervened, and carried the patient off,—all in eleven days. All the serous membranes were found to contain fluid; the pericardium was greatly distended with a reddish lymph; the pleuræ were seats of considerable effusion; and serum poured out in large quantities on opening into the cavity of the peritoneum. The encephalon presented nothing remarkable; nor was there any trace of any other morbid action than the effusions.

Almost all the patients of the clinique had had intermittent fever within the last two years. Dr. T. had bled a patient in the seventeenth day of a pleurisy, and thought it necessary to justify his practice by an appeal to the ancients. Without going the length of Galen, who says you are to bleed whenever there is a violent pain (strange, that Galen, who knew so much about the nerves, should not have restricted his injunction!) Dr. T. thinks bleeding advisable whenever there is a "*plethora ad sanguinem*,"—including under this head not only all cases where there is too much blood, but where the consistence of the blood is too thick. His rule as to quantity is to take just so much as to remove the impediment to the free circulation; and all beyond this he deprecates as bad and unnecessary. Large bleedings are to be utterly abhorred by all rational practitioners. I never knew him take more than ten ounces, and this is thought a great bleeding; from seven to eight is the average.

Dr. T. thinks the small hard charac-

ter of the pulse in certain cases of deep-seated inflammation, to be the result of the over-stimulating quality of the blood, which causes the artery to contract rigidly; but then it is obvious to all why, if this were the *rationale* of the phenomenon, it does not occur constantly? The pulse of the patients remains generally quick for some time after the inflammatory disease has left them. There have been many instances of this; and it gave rise to a remark from Dr. T., which I do not remember to have seen explicitly enunciated before, "that in acute diseases generally, it was usual, and quite to be expected, that the arterial, muscular, and nervous systems, should recover their functions by no means synchronously." This appears to me to be a sensible remark, and of considerable importance to the young practitioner, who may think a disease merely diminished, and insidiously lurking to break out again, when in fact it is gone. The diminution of the symptoms of a disease, and the disappearance of its most prominent or important symptoms, are worthy of attention. The different systems of the body sympathize, no doubt, with each other; but the sympathy is general, not intimate or necessary. Physiology shews how parts may live, independent (for a certain time) of each other, or of the whole. I have lately performed some experiments on the Grillus, which go to prove the individuality, so to speak, of the life of different parts. Smith even mentions an insect that copulated three times after its head was off (Wordsworth.) In certain diseases (which is still more to the point), we find paralysis of the muscular structure of parts, while sensation continues; and, *vice versâ*, the function of the nerves may be stopped, while the power of motion remains,—all such instances establishing a greater degree of independence than is supposed. Disease, then, seems to quit the body gradually, unequally, and, as it were, *à plusieurs reprises*.

After ten days suffering from an intense headache, a patient whose urine had been turbid voided some clear water, and recovered. Dr. T. told me, that wherever he had remarked a thick urine which left a sediment, in acute and painful affections of the head, he had found it critical, or of good augury; while, on the other hand, if in such affections the urine became limpid and clear, the prognosis was always grave. *Οκοσοισιν ουρα διαφανεα λευκα, πονηρα, μαλιστα δε εν φρηνετικοισι επιφαινεται.*—*Hippocrates.*

A young man of healthy constitution, and 29 ætat., was seized on the 27th January with rigors, followed by a cough and pain, and all the symptoms of pneumonia. He had had intermittent fever last year. Before entering the hospital he had been bled to ʒxij. by some daring practitioner. He had some headache; the pain in chest was fixed to lower part of left side; his skin was dry, but not very hot; respiration only slightly embarrassed; urgent thirst. He was ordered decoct. pectorale (which is made of liquoriee, mallows, &c.), and was bled three times on the first day, ʒviij. each time. The blood was slightly buffed. Notwithstanding these venesections, he could not sleep that night for the pertinacity of his cough. On the second day the pain had extended considerably; the pulse had acquired a vibrating feel; the sputa scanty, and thin in consistence. A little gum was added to his decoction, and he was bled twice in the course of that day (ʒviij. each time); the blood buffed and cupped. At twelve he broke out into a general sweat, which had no effect in staying the disease. Third day better; sputa getting thicker; pain confined to upper part of left lung; venesectio ad ʒviij.; vesicatoria ij. to thighs. Evening twice bled again ad ʒviij; blood still buffed; an enema was administered; a little syrup. altheæ and ɱxx. of aq. laur. cerass. in a ʒvj. mixture. Fourth day much better; gr. iv. of Kermes' mineral were added to the ʒvj. mixture; twelve leeches to upper part of left lung; no bleeding this day. On the fifth day a remarkable change for the better occurred; there was a remission of the fever; the pain had disappeared, or was scarcely sensible; the breathing was natural; the sputa were dense and matured; he went on with his barley-water, and two blisters were placed again between the thighs. On the morning of the sixth day all was going on well; but in the evening he complained of a pain in the upper part of right thigh, aggravated upon coughing. Twelve leeches were ordered to anus, and he went on drinking his barley-water, with aq. laur. cerass. ℈j. to ℔iijss. Seventh day same state of body, and same treatment; towards evening cough

again increased, pulse became more frequent and hard, and there was a very painful sensation on coughing at the upper part of the right thigh. He took a saline draught. On the eighth his situation was alarming; his pulse was hard, very quick; his countenance sunk; his lips pallid; respiration small and frequent; great restlessness; no pain in chest, but a very distressing pain in the thigh, shooting from the groin and right iliac region, increased by the least motion. Urine had a white precipitate; four grains of Kermes' mineral were given in barley-water; twelve leeches again applied to anus. In the night many stools were passed, and he sweated profusely. On the ninth his countenance had undergone a remarkable change for the worse; expectoration has ceased; right hypochondrium is tumid; twelve more leeches. In the night profuse sweat, diarrhœa, and tenesmus. Tenth, evidently sinking; says he has less pain in the thigh, and more in the chest; œdematous extremities. At about six in the evening pain of thigh ceased; anxiety of countenance gradually gave way to the pallor of death, which took place about six hours after the sudden cessation of pain.

Autopsy.—On opening the head, the brain and its membranes were found healthy; the vessels of the pia mater were injected. The thorax being exposed, shewed the lungs adhering to the pleuræ costalis and diaphragmatica; the left lung was wholly hepatized, and the bronchi were filled with purulent matter, which exuded on pressure. The pericardium contained a considerable quantity of an aqueous fluid. In the abdomen the omentum was found destroyed for one-third of its extent; there was a considerable effusion into the peritoneal cavity; the small intestines had patches of arborescent inflammation upon them (*vide* Billard Memb. Muq.) Both liver and spleen were enlarged; the former was of a light red colour internally; and externally the spleen was covered by a thick false membrane; and there were two unnatural productions, or secreted bodies, which were not tubercles—one of the size of a nut, the other rather larger than an almond. The inguinal glands of the right side were indurated, and cut almost like cartilage. Lastly, an abscess occupied a great portion of the right side of abdomen; it extended from as high as the convex surface of the diaphragm, near the vertebral column, and changing or supplanting the place of the psoas and internal iliac muscles, it descended down to Poupart's ligament, under the ring, through which it had begun already to make its way, when the patient died.

This case suggests many interesting observations; I have only space to make one. If this man had been treated after our way, might he not have recovered? In cases of acute inflammation, I should as soon expect to hear of an Italian doctor's success, as that a fortress might be stormed by pop-guns, or a conflagration put out by a squirt; but very acute cases I have not seen treated; and I am bound to express not only my high opinion of Dr. T.'s good sense, but to say that I think his success is much on an average with what it would be in England.—I am, sir,

Your obedient servant,

D. Badham.

Rome, March 17, 1834.

CASE OF POISONING BY ARSENIOUS ACID;

With Dissection and Remarks.

By John K. Booth, M.D.

One of the Physicians of the Birmingham General Hospital, and Member of the Royal College of Physicians, London.

Elizabeth Martin, æt. 24, a married woman, was brought into the hospital on the evening of the 24th of March, at about half-past seven o'clock, having, by the report of those who conveyed her, swallowed half an ounce of arsenic, about two hours previously. She had taken an emetic, a little more than an hour after the poison, which acted promptly, bringing away a large quantity of fluid from her stomach, in which small gritty particles of arsenic were palpably noticed by the medical attendant. Her countenance is now pallid, extremities cold, eyes suffused, pupils somewhat dilated, but contracting on the approach of light. She speaks now and then, with effort, but appears insensible. Pulse 90, and very compressible.

She refused to swallow mucilaginous diluents, and consequently the stomach-pump was used. The stomach was filled and emptied six or seven times

with soap and water, and a little carbonate of soda. A mixture of castor and olive oil was then injected, but symptoms of collapse had already come on, the pulse became very weak, the pupils contracted and fixed, the surface cold, and she had violent purging. She lingered till midnight, and expired.

Postmortem appearances, 16 *hours after death.*—The vessels of the brain were gorged with dark blood. There was slight subarachnoid serous effusion, but the ventricles were empty. The substance of the brain was firm, that of the cerebellum firmer than usual.

The lungs were in a state of congestion sufficient to have completely impeded respiration. The heart was not particularly loaded; the cavities of the right side being nearly empty.

The œsophagus downwards, to within half an inch of the cardiac orifice, was inflamed. The whole internal surface of the stomach was of an intense scarlet colour, and the mucous membrane was constricted into rugæ, to which some of the arsenic, enveloped in a viscid, tenacious mucous, adhered in two places. The inflammation of the stomach terminated at the entrance of the œsophagus, by a well-defined limit. Numerous dark spots and small dark streaks were universally sprinkled over the internal surface, but particularly at the cardiac extremity of the stomach.

There was redness and increased vascularity of the duodenum, jejunum, and ileum. The colon was exceedingly constricted, barely admitting the passage of a finger. The solitary glands were more prominent than natural, and of a pale appearance. The veins on the inner surface of the cæcum and commencement of the colon, had a very beautiful arborescent appearance, from congestion; and this was still more conspicuous on the mucous membrane of the bladder.

The kidneys were of a darker hue than natural. The spleen and liver healthy, but the vessels of the latter were turgid, and its size rather large.

Arsenious acid is extensively employed in this town, is easily obtained, and its fatal properties are popularly familiar. Hence it is not surprising that it should be more frequently resorted to than any other of the poisons for the destruction of life. To no practitioner of medicine, but more especially not to any one whose duties may connect him with a large manufacturing commuuity, can the present subject ever seem trite or unimportant.

In treating cases of arsenious poison, the efforts of the *vis medicatrix naturæ* and the precepts of practice alike instruct us, with all possible promptitude, to evacuate the stomach by vomiting. This is the primary indication, and on its *effectual* fulfilment, I would say, the rescue of life depends. It is, however, not often possible: sometimes a previously loaded stomach may envelop in its contents the corrosive ingredient, and facilitate its ejection by vomiting. A medical friend informs me that he witnessed a case wherein, *after a full meal*, an ounce of corrosive sublimate had been swallowed; and, by timely vomiting, the subject of this rash act escaped with comparative impunity. We can, so far as my experience instructs me, seldom hope, in cases of arsenious poisoning, to divest the stomach, under any circumstances, by vomiting, so effectually of the noxious particles of the acid, as to supersede other subsequent indications of treatment, the next of which will be to remove or extinguish, by the aid of the stomach-pump and *proper* fluids, the remaining poison. Here, then, a question may be raised as to the proper fluid. I would say lime-water: certainly not the alkaline solutions. The latter were, I am of course aware, adopted, in the treatment of the case prefixed to these observations, by my colleague.

Indeed, distinguished authorities seem opposed to each other on this point of treatment. Dr. Paris (Pharmacologia, edit. 8th, page 193), enforces the use of lime water, and deprecates that of alkaline solutions, excepting (see his note) under circumstances and limitations which would apparently reduce the exception to little more than hypothetical. Dr. Christison (Treatise on Poisons, edit. 2d, p. 320) regards the arsenite of lime as useless, because, although insoluble in water, it is so soluble in the juices of the stomach as to allow of the introduction of a sufficient quantity of the poison into the blood to prove fatal. Orfila admits the efficacy of lime water, if the poison be in a liquid state:—"Il se forme dans ce cas un arsénite de chaux insoluble qui n'agit que très faiblement." He does not mention the action

of the gastric juices as interfering with this result; on the contrary, he continues:—"Nous avons donné à des petits chiens jusqu'à 4 grains de ce poison liquide: nous leur avons fait avaler de l'eau de chaux, et *ils n'en ont pas été incommodés.*" (Toxicologie, 3me. edit. p. 441.) M. Orfila, however, adds:—"Comme c'est presque toujours à *l'etat solide* que l'on prend cette substance vénéncuse, *l'utilité* de l'eau de chaux est presque *nulle.*" It is evident that authorities are divided as to the employment of lime water in fulfilling the second indication of treatment. Its use is discouraged by Dr. Christison, on grounds at variance with the result of M. Orfila's experiments, as to the action of the juices of the stomach. No interference, no counter-action, was observed by Orfila from the cause alleged by Dr. Christison, on apparently chemical grounds. No one, however, can possibly be better aware than Dr. Christison himself of the change induced in the quantity and quality of the secretions of organs, when acted upon by violent irritants; and it is consequently not determinable, *à priori*, what combinations will take place in the stomach under such circumstances. To the chemical objection stated by Dr. Christison, I would beg to oppose the results of the following simple experiments, which tend to prove chemically that arsenious acid may be changed by combination in the stomach, to arsenite of lime, *if its juices be saturated by carbonate of lime.*

Experiments.—1. One grain of arsenious acid was added to two ounces of lime water, and gently agitated. The arsenic was soon entirely changed into a substance that was suspended or floated in the liquid, being a precipitate of arsenite of lime. Muriatic acid and dilute acetic acid, forming the acid properties of the gastric juice, were then added; they redissolved the precipitate immediately.

A portion of the preceding mixture of muriatic and dilute acetic acids was then saturated with chalk, and filtered. This solution produced *no effect* on the arsenite of lime.

Reversing the order of the experiment—

2. One grain of arsenious acid was put into a phial containing some of the saturated (let it for distinction be termed) *gastric* acids; viz. the muriatic and dilute acetic acid. No effect was produced. Lime water was then added, and the same result, as in the first experiment, ensued: the arsenious acid was changed into a substance which floated in the liquid, being a precipitate of arsenite of lime, and appeared rather more dense than in the former experiment.

These experiments tend to show, that, by simply adding creta præparata to the lime water employed in washing out the stomach, we may entirely obviate the chemical objection opposed by Dr. Christison to its use. We are not to forget, too, that although primarily, it is not merely to the *stomach* that the deleterious contact of the poison is extended: we can trace its presence and effects throughout the whole alimentary tube. It is, therefore, a desideratum to obviate its absorption by destroying, and not increasing, its solubility: lime water has certainly the former, while the alkaline solutions have as certainly the latter tendency; and are directly calculated, therefore, to increase the deleterious influence of the arsenic. In a great many instances I know all efforts may be futile. The arsenious acid often imbeds itself in the thick tenacious mucus adhering to the inner surface of the stomach, and defies all solvents to reach it. An instance of this description is afforded in the case and dissection prefixed to these observations. I have noticed the same fact in others. Dr. Roupel, also, in his plates in illustration of the effects of poisons, gives another. M. Orfila (Loc. Cit. p. 382) mentions a remarkable instance of a *cyst*, in which arsenious acid was contained in cells. Dupuytren considered the formation of this cyst to have been the result of two poisonings antecedent to that which terminated the person's life. The sequelæ, even in cases of fortunate rescue of the patient from impending destruction, are always wretched; but still it is morally incumbent on the physician to omit no means or effort to save and prolong life: and in this view, I consider the choice of the fluid to be employed as not a matter of indifference, or unworthy of research.

J. K. Booth.

Birmingham, April 5, 1834.

A MANY-TOED AND FINGERED FAMILY.

Meckel, in his Pathological Anatomy*, has collected several instances in which the presence of supernumerary fingers and toes was hereditary in certain families. To these the following may be added.

Thomas Copsey, a man of 19, at present a patient in the Middlesex Hospital, under Mr. Arnott's care, has thirteen toes, and states that he had originally fourteen fingers. He has seven toes on the left foot, and six on the right. Each of these toes has the natural number of bones (phalanges), and is provided with distinct tendons. With the exception of the fifth toe, all the others are adherent (united closely to each other by integument). There are but five metatarsal bones. In each foot there may be said to be two great toes, which are articulated with the first metatarsal bone, and in the left foot two toes are articulated with the fifth metatarsal.

On the outside of the first phalanx of the little finger, close to its proximal extremity, is an elevated cicatrix, a tubercle, indicating the situation from whence two fingers were cut in his infancy; the middle and ring finger are adherent through their whole length in each hand.

Thomas Copsey has five brothers and four sisters; in all of whom, with the exception of one sister, there are six toes on each foot, and a sixth finger was removed from each in their infancy. The other sister has seven toes on one foot and six on the other, and had two fingers removed from each hand. His mother has the same number of toes, and had the same number of fingers, as himself and sister last alluded to. The same is the case with his only maternal uncle (who has no family), and was the case with his maternal grandfather.

Perhaps a better idea of the appearances will be formed from the subjoined woodcut, than any verbal description can convey.

THE DRUG TRADE.

To the Editor of the Medical Gazette.

Sir,

I send you some extracts from a lecture on the Materia Medica, delivered in the theatre of the College of Physicians, in the summer of 1830. They relate to an important subject—that of the drug trade. I should be glad to fix some part of the attention of our profession, now at last roused in matters of Public Health, on the source and quality of the physic we so largely prescribe.—I am, sir,

Your obedient servant,

A Physician.

March 29, 1834.

It was my intention, previously to exhibiting the choice samples afforded to

* Vol. i. p. 19.

us in the Burgess Collection of the Materia Medica, to have inquired somewhat closely into the present state of the London drug trade; but on approaching the subject, I find it so comprehensive, and of such grave importance, that I am unwilling, without further "investigation," and more reflection, to enter on it in detail. There is, however, one great truth impressed on my mind, from every view which I have ventured to take of the subject—one which I feel it my duty to communicate to you before we handle a single article from the cabinets; and this it is,—that no great improvement can ensue in our acquaintance with the Materia Medica, as physicians, until the drug trade be revised, and subjected to better regulations. Before we talk of the medical effects of medicines, and draw medical inferences from such effects, we must know what medicines we use,—a kind of knowledge to which, in the present state of our shops, we cannot at all pretend; for there is as much difference in the drug market, between certain samples of the same medicine, as between one medicine and another. Now this variety in the sample of the article strikes, I need not say, at the very root of all argument respecting its medicinal effects.

In our administration of medicine, we expect and assume an identity of operation, from things widely different in their elementary nature and composition. There is, I was assured only the other day, by a very intelligent chemist and druggist, a difference of 300 per cent. in the quality of certain articles of the Pharmacopœia, on sale in different shops. This was his own unreserved expression; and the statement may be verified by any one who will take the trouble to examine samples from various retail stores in the city of London. Indeed, this difference in the quality of certain drugs may be at once assumed, from the difference of prices in the bills of parcels from various *wholesale* houses. I have known this difference range from 12s. to 20s., from 1s. 8d. to 6s.; and this in medicines declared to be of the best quality, and furnished to a great public institution, where it was known that they would be submitted to a fair competition, and would be carefully examined by a competent professional committee. It is not likely that better drugs than the worst of these should be furnished to the solitary practitioner of the retired hamlet. Now, even admitting a range in what may be termed the fancy price of drugs, as measured by an imaginary excellence of colour and other properties of appearance, there still would be an alarming difference in their money value, only to be accounted for by a difference in their useful medicinal properties. By all great dealers this variety is admitted; and yet, be it remembered, every variety finds a purchaser. A great deal of this pernicious trash is disposed of to the London market; but the greater part of it, I am told, "goes into the country." The respectability of the drug trade has been greatly lowered by the demand of the retail dealers for these low-priced inferior samples. I have heard this stated by respectable druggists, who have lamented that it should be so. Those who conduct the trade with a sufficient capital—a capital enabling them to make a selection in the market, and who are impressed with the responsibility of their business, would, I am sure, be very glad to assist in placing it on a footing which, by affording more security to the public, would imply its greater respectability. There are some houses where wonderful pains are taken to insure excellence in the medicines furnished, and accuracy in the preparation of them. Like every thing in London that is fairly taken up, the business in these houses is done to perfection. Of all the numerous trades and corporations of our citizens, there is, indeed, no one more intelligent, and in every way respectable, than the better, I mean by this the scientific, class of our druggists.

I will not enlarge on the mischief that must ensue to the public from the circulation of so much bad physic; the nuisance is gross and palpable;—the simple expression of it is its strongest condemnation; and it does not attach to me more than to others to exclaim against it; but I may be permitted to express my sincere regret at the uncertainty in which it involves the science of medicine, and the discredit which it brings on its practice. In prescription no one now knows what he orders. How, then, can he presume to calculate upon the effects of his prescriptions, or to reason on them when obtained? From this constant source of error, I find it impossible to procure any information as to the effects of varied doses of medicine that can be at all depended on.

In the very outset of the Materia Medica, in the study of its elements, we are met by this perplexity, arising from the abuses of the drug trade. There is another bar to our improvement in the knowledge of drugs, as connected with the sale of them, which is the way in which they are dispensed. The materials may be of good average quality, yet may be compounded in wrong proportions, or by a process of manipulation inconsistent with the intentions of the prescriber. From the general ignorance among medical men on the subject of drugs, the prescriber may often mistake his medicines and their doses; he may be, and he often is, careless, or by chance inaccurate—but securities are taken from the Physician by age and education, to prevent, at all events to limit, his means of injuring the public. The compounder of his prescription is in most instances amenable to no inquiry—to no tribunal. In a shop that has been mentioned to me, a common porter has been habitually employed to compound medicines; and I have recently met with an instance in which the entire business was carried on, for months together, by a widow, with an infant in her arms. Boys and women have not unfrequently, in my own observation, been found in charge of a shop and its business during the absence of "the master."

The consequences are best illustrated by the police reports of the daily papers, and by such advertisements as the following:—"The gentleman who purchased the carbonate of soda, tartaric acid, and sp. lavendulæ comp. is requested not to use them, but to apply for information as soon as possible."—See Morning Herald, Sept. 8, 1829. Some of those now present may remember the attempts which I made, in my lectures of last year, to direct the attention of the College to the disgraceful uncertainty of the preparations of Colchicum in the different shops of this metropolis. In some the "Vinum Colchici" is a wine of the fresh root; in some a spirit of the dried root, or a spirit of the fresh root, and a wine of the dried; a wine of the seeds, or spirit of the seeds; varying again in the quantity of the ingredients, and in the time given to the preparation. In no two shops, visited consecutively, would the Vinum Colchici, as ordered by the Pharmacopœia, be met with; and certainly in no two would it be found to possess the same degree of strength *.

In a tour of visits made officially to the retail shops, it was found that the "Cathartic Extract" was hard and black in all places but one, where it was soft and mouldy, being "seldom asked for." A proprietor of Senna Confection candidly acknowledged, that the light brown, dirty-looking compound, was not fit for "human medicine, but might be worked up into boluses for horses." Oxalic acid was generally met with in a large bottle, on the lower shelf, and in front of the shop, next to sulphate of zinc, and other salts. White arsenic was in most instances kept as a powder. It was seldom marked "poison," and it did not appear that any precaution was observed in its sale. In more than one instance I found this deadly agent in a bottle bearing two different labels on its opposite sides.

Such a state of things is flagrant, and surely calls for public redress. Again, let it be remembered, the shops in which medicines are thus carelessly compounded are precisely those in which the worst drugs are found; bad, cheap drugs, originally, deteriorated into utter worthlessness, or positive mischief, from having been ill stored, and by long keeping.

The difference in the quality of drugs is notorious; the adulteration even of the baser sort, by other substances, is notorious; the carelessness in the compounding of them, the unfitness of many of the persons employed in this occupation, is notorious; and yet we continue to prescribe, and the public to swallow physic, without inquiry as to where the physic is procured. A more revolting inconsistency can hardly be imagined than that presented by the anxiety of all classes in this country to "have medicines ordered" for them; and their indifference to the quality of it, when "ordered." It is really a bitter satire on the state of the art in England. If we of the profession do not take the lead in reforming these abuses connected with the sale of our principal medicinal agents, and in preventing the danger to which I have alluded, others will assuredly do so for us.

* And yet in consultation we cavil about a few drops of the Vinum Colchici more or less, and discuss nicely the comparative merits of the Root and the Seeds!

Many plans might be devised for our common protection; but no better one occurs to me than that which would be afforded by a small well-paid board of working commissioners, obliged to report weekly on the stocks imported and disposed of by the broker; likewise empowered to require a declaration of the stock in hand, both from the wholesale and retail dealers; and enabled to convict, in a summary fine, before a magistrate, on proof of the sale of a spurious article, or of any gross negligence in pharmaceutical knowledge. I would not urge domiciliary visits, as now practised in a limited district; but I would not leave the drug trade without checks of the kind I have suggested. From the capital required to carry it on; from the great and varied experience which it implies; it will always be more or less of a monopoly; confined, as that is, to a few traders. The risk is very great, from the number of ports to be entered,—from the perishable nature of some of the articles imported, —from the caprice of fashion about drugs,—and, in some measure, from the ignorance of the home dealer in not knowing how to select the samples.

I was told, not long back, of a large parcel of Narcissus bulbs having been disposed of for squills, which were then very scarce; and of tulip roots having been preferred by a learned body to those of the colchicum.

There never can be a full, fair, free competition, in the drug trade; and therefore some special protection should be afforded to the public in their necessary dealings with those who conduct it. There are few trades that have a more direct influence on the safety of the public,—no one which it should watch with greater jealousy. The subject which it involves is that of health and life. There are commissioners of mad-houses, commissioners of vaccination, and others innumerable, appointed by parliament;—I do not see why there should not be commissioners of drugs,—a united commission, it might be, of physicians and practical chemists; but they should be paid for their work, and paid only in the ratio of it. By them only, or by some similar board, should retail druggists be licensed; and they should be answerable for the age and fitness of every person in every shop employed in the compounding of medicine. Some regulation of this kind is imperatively called for, now that so many new and active chemical medicines are prescribed, and that the old medicines are so often given in an essential or concentrated form. There should be, I think, a commission for the inspection of drugs; but the first duty of the commissioners would be to educate themselves,—to become acquainted with the character and properties of drugs,—with the tests of their purity,—not merely as conducive to the "elegance of the draught," or the "lightness of the pill," but as agents of Medicine, as apericnts, as emetics, as narcotics, as alteratives; in short, as serviceable articles of the materia medica. The only men to do this properly are the physicians of public institutions, assisted by practical chemists. What truths might they not establish in physiology!—what a body of Practical Medicine might they not collect!—what a Pharmacopœia might they not compile!—and no Pharmacopœia will be as good as it ought to be that is not compiled by such a board as this,—a board by which the effects of the materia medica would not only be tested in the laboratory of the chemist, but verified at the bed-side of the patient.

LIGATURE OF THE ABDOMINAL AORTA.

(Communicated by Sir James Mac Grigor, Bt.)

Cape of Good Hope,
Jan. 27, 1834.

My dear Sir,

I have just time to inform you that I tied the abdominal aorta last night, in a Portuguese man, in a desperate case of aneurism of the right external iliac artery, which reached as high as the umbilicus, and more than half way across the lower part of the abdomen. The man was in the greatest agony from the pressure of the tumor upon the nerves leading to the limb, and mortification of the limb was rapidly approaching. To attempt any thing on the right side was out of the question; so I cut directly for the aorta on the left side, nearly in the mode suggested by Guthrie and others, and effected my purpose without more difficulty than was to be anticipated. It is now nine hours after the operation, and the man is doing so

well as really to give some hopes of recovery. I shall give you the case hereafter, whatever be the result, with my reasons for undertaking this operation; which has hitherto been considered of so awful a nature. At present I have not time to add more, as a mail by a fast-sailing ship is just closing.—Ever sincerely yours,

JNO. MURRAY.

[The patient died twenty-three hours after the operation. Further details are expected.—ED. GAZ.]

MEDICAL GAZETTE.

Saturday, April 12, 1834.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

REFORM COMMITTEE.

THE Parliamentary Committee on Medical Reform is to renew its labours on Monday next, when it is understood that Sir D. Barry is to be again examined. Of the further proceedings we know very little; but it is generally believed that they are regulated as to the persons to be called, the questions to be put, chiefly, if not exclusively, by five or six physicians among the Licentiates who take an active part in what is called the "Association." This consists of a kind of society, formed of a certain number of those who signed the petition to Parliament against the College of Physicians; and those of the Licentiates who have hitherto been examined consist of some of its leading members. Now the existence of this association—probably the fact of the most active person connected with it being known to have been long on intimate terms with the chairman, and the influence which it soon became apparent the "Association" possessed in the committee-room—did not fail to raise a strong feeling among the "Fellows," that they were not likely to meet with fair play and equal justice.: a circumstance by which they have been roused into increased excitement. The truth is, as we hinted in a former number, there has been too much disposition evinced in the progress of the inquiry to convert it into a private and personal contest between two rival parties. *We* stand equally aloof from both, and equally independent of both; and while we say to the Fellows that important changes must be effected in their College, to suit it to the spirit of the times, and the vast improvements which have taken place in society since it was instituted, in the days of bluff King Harry; so we tell the "Association," with equal firmness, that they are suffering the acrimony of party feelings to appear amid their claims to public principle; and that they act as if they were more anxious to lower the College than to elevate the profession. Do they suppose that they truly represent the opinions of the physicians of England generally, or even of the Licentiates? *We tell them they do not.* The majority of the Licentiates refused to join the Association, which is composed of those entertaining *ultra* views of reform (by which we mean levelling doctrines), and of a minority of more moderate persons, who strongly disapprove of their proceedings, but not one of whom they have suffered to be examined.

Why are not some of those Licentiates called who refused to join the Association? Why not some of those who refused to sign the petition against the College? Shall we be told that those who have been examined as representing the opinions of the Licentiates, are either the men of longest standing—of most extensive practice—of highest reputation, or of most commanding position? Certainly they are very, very respectable men—nay, some of them are distinguished men; but their being confined to the committee of the Association, shews that other motives have had influence in their selec-

tion. Why is not Dr. Farre called? Why not Dr. Clutterbuck?—why not Dr. J. Clarke?—why not Dr. Hume?—why not Dr. Locock?—why not Dr. Ley?—why not Dr. Copland?—why not many others? The reason is obvious: none have been called, and, if the Association can help it, none will be called, except those whose opinions are ascertained to coincide with their own; and thus an impression is conveyed, with respect to the sentiments of the physicians in London, which is far from being correct. Three-fourths of the Licentiates desire reform in the College of Physicians, but they do not desire to overturn it. If the Association does not wish to bring the building down, they are but bungling workmen: let them take care that they are not themselves crushed in the ruins.

Let the inquiry be free as the most ultra-reformer would desire; but let it be fair—let both sides be heard—let not its freedom consist in suffering those only of a certain way of thinking, among the Licentiates, to be called—let not the notion be conveyed to Parliament, that the physicians who seek for reform wish for revolution. Such notion would be a false one. Above all, let nothing be done to lessen the confidence reposed in the impartiality of the Committee, to which the eyes of the profession are turned with so much interest and anxiety.

HOAX ABOUT A LATE OPERATION.

When, a fortnight ago, one of our veracious contemporaries,—we say *one*, for there are now two journals with about equal claims to the epithet;—when a veracious contemporary informed the world, that "an operation, which has created an extraordinary sensation in the medical circles at the west end of the town, has lately been performed at St. George's Hospital,"—we stated what was the simple truth—that no one could even guess to what case this announcement alluded, and we expressed our conviction that some one had hoaxed him. This accordingly proves to be the case. Goaded by our taunts, and choosing, like many honester men, to be taken for a knave rather than a fool, he has swallowed our bait; he has specified the particular case he had in view, and thus is fixed as being either the original inventor of falsehood, or as the tool made use of by some of those broken-down outcasts of the profession who are always to be met with in such a place as London, and who are ever ready to earn a dishonest guinea by getting up some piquant tale against any more prosperous man, and which their employer is equally ready to publish without inquiry. It is no bad idea, either, of Mr. Wakley's, that which he has now adopted; namely, in the absence of any reporters of his own at the hospitals, to make the medical officers contribute their own cases. "We call on Mr. Hawkins," says he; and again, "we call" upon the surgeons of the Middlesex; and "we call" upon the surgeons of St. Thomas's; and no doubt "we" will call upon those of St. Bartholomew's and Guy's; and long and loud, it is to be feared, shall "we call" in vain! Perhaps our contemporary has heard of one Owen Glendower, who could "call spirits from the vasty deep;" but as he found it easier to call than to make them come, so we suspect it will be with Mr. Wakley's calls. Time was, indeed, when parties accused by name of killing their patients, would have thought it necessary either to answer the charge or to prosecute the libeller, but now they do neither—because no one feels it to be necessary, so well and so truly are the weight, influence, and authority of the Lancet estimated. For this the profession have to thank the London Medical Gazette, and we humbly submit that the service is one of no secondary im-

portance: men's characters cannot now be openly written down, or indirectly insinuated away; because we have shewn, again and again, until it has become a familiar and proverbial dogma, that any statement being strongly urged by our contemporary, is a *prima facie* evidence of its being a falsehood. Nothing, certainly, can more clearly shew the estimation in which the publication is held, than the contempt with which the "calls" of the Editor are received.

But to return to the "late" operation at St. George's, and the "extraordinary sensation" which the Lancet is pleased to inform us has thrown the medical circles at the west end of the town into such a state of excitement. Finding that he had been imposed upon, and yet without the candour—the common honesty, to admit his error, the Editor of the Lancet returns *for the third time* (the subject has lasted him for three weeks) to the charge, or rather insinuation, about "Mr. Cæsar Hawkins's fatal operation;" and as we have forced him, albeit most reluctantly, to come to particulars, we gather his story to be as follows:—That the *recent* catastrophe occurred to a man named Fisher, on whom Mr. Hawkins proceeded to try an experiment, by the introduction of a new instrument, the movements of which he was afterwards unable to control; so that it stuck fast in the bladder, and thus rendered it necessary to cut through the perineum, as in the lateral operation of lithotomy, for the purpose of liberating the instrument; that the patient died in consequence, and that either a coroner's inquest was held on the body, or ought to have been held,—for the expressions admit of either interpretation*. Such is the version of this story as given by Mr. Wakley—on the authority, we fear, of *a disappointed man*. We shall give the real history of the case to which it is evident that allusion is made; and we pledge the character of our journal for the accuracy of our report,—a guarantee, by the way, which our opponents well know will make the balance kick the beam, if weighed against any thing they can adduce to gainsay us.

Not *very* "lately," but in June last—not quite a year ago—a patient named John Fisher, aged fifty-seven, was admitted into St. George's, under the care of Mr. Cæsar Hawkins, for stone in the bladder, from which he had suffered during two years. He stated that he had some time before been under the care of Mr. Costello for six months, who had nine different times performed an operation for the purpose of grinding or reducing the calculus. At the time of his admission his health was so far broken, that the introduction of a sound almost always brought on rigors, and some degree of fever. It is thus apparent that he was not a favourable subject for any farther operation, and, *à fortiori*, not fitted for experiment. We therefore feel convinced it will gratify the benevolent mind of our contemporary to learn, that *no experiment* was tried, and that *no new instrument* was used after his admission into the hospital. As there was reason to suppose that the calculus was small, it was decided that it should (if found practi-

* That there may be no room for doubt, as to the extent of the passage in the Lancet, we subjoin the following extract from the last number. It will be perceived that "lately" has been changed into a "few months:"—

"Has a man of the name of Fisher been admitted, within the last few months, into St. George's Hospital?

"What was his complaint?

"Did Mr. Hawkins try an experiment on him?

"If any, what was its nature?

"Was any new instrument introduced into his bladder, and was the operator capable of controlling the movements of the instrument after it was introduced?

"Did the instrument stick fast in the bladder?

"Was a cut made through the perineum into the bladder, as in the lateral operation of lithotomy, for the purpose of liberating the instrument?

"Was it extricated thereby?

"Did the patient expire on the 8th or 9th day after the operation?

"Was the body examined?

"Was there a coroner's inquest held over the body of the deceased?"

cable, without using force) be withdrawn with the forceps through the urethra, or, at all events, into the perineum, and then cut down upon in that situation. This purpose, however, could only be partially accomplished, owing to the unyielding state of the parts, through which it was found that it would be impossible to make the instrument, having the stone between its blades, pass without violence. An incision was therefore made into the perineum upon the forceps, which still grasped the calculus; a small portion of the prostate was divided, and the concretion, which was about the size of a hazel-nut, removed by the wound, the forceps being, of course, withdrawn through the urethra in the usual manner. In a word, Mr. Hawkins performed a more delicate but less dangerous operation than the common lateral one of lithotomy; and we appeal to every practical man in the kingdom, whether he did not act as any other skilful surgeon would have done in similar circumstances?

No local symptoms of importance occurred, and the wound went on well, but the poor man died on the thirteenth day after operation, *from the formation of abscesses in the joints*, a large collection of matter being found in the right shoulder, another in the knee, and several small depôts in the hands and fingers. The publisher of the libel probably is not aware, but the readers of this journal must be so, that these secondary abscesses not unfrequently arise from the most trifling injuries, such as the wound inflicted in venesection;—that they sometimes take place where there has been no wound of any kind; while they are such as no care can prevent, and no skill, except in some rare instances, can succeed in removing, when they have once been formed. We need scarcely add, that no "inquest" on the case was held, save that by the would-be coroner of the Lancet. The examination was, as usual, publicly made, and the particulars of the case are to be found in the hospital book, kept by the clinical clerk.

The history of the preceding case is so clear, and the facts so irrefragable, that it certainly shews some ingenuity to have distorted them into the shape they have been made to assume by our contemporary:—the lesson, however, will not be without its use to him; and though, as is his wont, he may bluster about it at present, he will, for his own sake, be more cautious hereafter in publishing information coming from the same quarter. One part of the story is correct—the case *has* excited an "extraordinary sensation," but it is an *ex post facto* result: we mean that the Lancet report has excited a "sensation" of *merriment* among the pupils at the Hospital, who could not have a better opportunity of judging of the credit which is to be given to those periodical reports of murders, catastrophes, and other high crimes and misdemeanors, described by Mr. Wakley, when he is in his heroics, and shadowing forth from his prolific imagination the "scenes of blood" perpetrated in our hospitals—those "human slaughter-houses," where the "suffering poor" are "butchered" for the amusement of the surgeons and their friends.

ANOTHER STRUGGLE FOR A CHARTER.

We perceive that Mr. Richard Taylor, "*Printer to the University of London*," has got up a petition from that most sapient body, the Common Council of the City of London, praying his Majesty to grant a charter to the establishment in Gower-street. Sir Peter Laurie, in reply to Mr. Taylor's *speech*, exposed the stock-jobbing origin of the project with a good deal of humour. He held in his hand a hundred pound share which had been purchased that day for £23!! No wonder that the shareholders, and Mr. R. Taylor among the rest, should make some effort to raise their price in the market.

CLINICAL OBSERVATIONS,

Delivered at the Middlesex Hospital, by

MR. ARNOTT.

MALIGNANT DISEASE OF THE HUMERUS—AMPUTATION AT THE SHOULDER-JOINT.

GENTLEMEN,—In the practice of a civil hospital, amputation at the shoulder-joint is an operation we are not often obliged to perform, the neeessity for its performance being most freqnently produced by the casualties of war—severe gun-shot fraetures of the humerus. It is, however, occasionally required for the relief of incurable, or at least otherwise intractable disease of the arm; and this was the case in the instance you have recently had under your observation. Indcpendent of the operation itself, which is one you are not likely to have an opportunity of again witnessing on the living body during the session, the case presented some points of interest; and you had more especially the opportunity of observing that in recognizing the real nature of the disease with which the patient was afflicted, diagnosis in surgery, as well as in medieine, is not always a matter of facllity.

The patient, John Slater, is 44 years of age, a tailor by oecupation, tall and well-formed. Eighteen months previous to his admission into this hospital, he received a blow with the edge of a flat iron on the left arm, a little below the point of the shoulder. He was immediately disabled, having lost the power of voluntary motion in the limb, and was taken to a surgeon, who stated it to be dislocated, and treated it accordingly. It was supposed to be reduced, and he shortly afterwards resumed his occupation, which he was able to follow, but he did not recover the use of the shoulder-joint; he could not raise the humerus. Nine or ten months afterwards, in shovelling coals, while in the act of preventing his spade from falling, he felt a sudden pain and tearing sensation in the seat of the original injury; after this, he was not able to make any effective use of the member, having still less power of moving it at the shoulder. Three months before he came here he had the influenza, for which he was largely bled. From this time the shoulder began to swell, and this was accompanied by excessive pain.

On presenting himself, the following was the condition of the parts. Great swelling of the shoulder and upper part of the arm; the swelling greatest above: it was ovoid, and preserved somewhat of the natural form of the parts, only much magnified. Its chief bulk projected forwards and outwards, and in the axilla, where it was felt large and round. The shoulder, or rather the swollen parts corresponding to the upper part and head of the humerus, seemed in a trivial degree lower than natural; that is, there was a slight depression below the acromion, like that attending fracture of the neck of the scapula. The swelling, of a uniform surface, was tense and elastic, and gave the idea of containing a fluid. The integuments were of their natural colour and temperature; there were no enlarged veins. Handling the parts did not give pain, but the attempt to raise the arm from the side did, and its elevation could not be effected. In attempting to do this, the humerus did not seem to revolve on its head, but appeared to move at a part below this, as if the bone was broken a couple of inches lower down. In rotating the limb a sort of grating or crackling sensation was perceived; but whether from bone or effused fluid could not be decided. The patient had lost flesh, but his countenance was neither sallow or expressive of anxiety.

What was the nature of the disease in this case? The question did not admit of an immediate answer, and there were difficulties in the way of a positive opinion. The round prominence in the axilla, and the slight depression immediately below the acromion, led to the inquiry, if the former was owing to the head of the humerus being displaced. No motion was communicated to it on rotating the limb; imperfectly, it is true, as this could be done. The cause of the accident, and the subsequent history, did not accord with the notion of an original dislocation; nor did the existence of swelling outwards and forwards simultaneously with that in the axilla, coincide with the existence of dislocation now. The head of the humerus could not clearly be made out in any situation; but this was supposed to depend upon a large quantity of fluid surrounding it, which prevented its being felt. The tense and elastic feeling of the swelling was attributed to the fluid being deep seated, and covered by the deltoid muscle and fascia.

Was the case one of ununited fracture and diseased bone? There were reasons for suspecting that this might be the fact. The blow on the humerus with the edge of the flat iron,—the loss of power in the limb,—the apparent motion in it at a point short of the head of the humerus, and the grating sensation felt in rotating it,—tended to support this conclusion; yet there was more swelling than attends ordinary cases of ununited fracture, and it extended seemingly to the joint. The latter circumstance might be explained by

supposing the whole of the upper portion of bone diseased.

Local applications, as leeching and blistering, with the view of reducing the size of the swelling, so as to procure a more satisfactory examination of the state of the parts, had been tried without any effect. I therefore determined to puncture it. Before doing so, I asked my colleagues to look at the case, and stated to them that I believed there was fluid and diseased bone, and that after evacuating the former, and ascertaining the extent of the latter, I should act according to circumstances,—removing a portion, perhaps the head of the bone, or it might be, amputating the limb at the joint. They examined it, and both declared there was a collection of matter here. The puncture was deferred for a few days, to allow of the patient's recovery from a slight attack of erysipelas with which he was affected; and in this interval he was seen by a French hospital surgeon, who happened to be going round the wards with me, and who placing his hand on the shoulder, said, "Ici est un vaste abcès;" recommending that it should be opened with a free incision. I mention this to show you how general was the deception as to the contents of the tumor.

You probably observed that I made the patient acquainted with the difficulties of his case. I allude to this, not with the intention of holding it up as an example to be generally followed. You will meet with patients who cannot bear the idea of their attendant having a doubt as to the nature of their complaint; and who, under every circumstance, expect him to have what they term "a decided opinion." With individuals of this character of mind, where the lapse of a short time may clear up the doubt, and when no object is to be gained, you will do best to evade, if you can, giving an opinion at all. In the present instance we had to deal with a sensible man, who, from the discordant opinions he had received from other surgeons before he entered the hospital, was already aware that his case was not an ordinary one. He had been told that the bone was dislocated,—that it was fractured,—and, by a very high authority, that it was both fractured and dislocated. I had also another object in view besides obtaining his consent to the puncture,—this was, to prepare his mind for the ulterior measures which might be required. The good effect of this you no doubt noticed: to these ulterior measures he at once assented.

A puncture was made with a double-edged scalpel in the swelling, on the outside of the arm: not a drop of matter escaped: a probe was passed into the wound, and went right across where the bone ought to have been, without encountering it. The puncture was enlarged, so as to admit the finger, with which bone could not be felt in any direction, upwards or downwards; but in its place a soft mass, which yielded to somewhat firm pressure, and could be broken up, feeling in its resistance like a portion of liver. The obscurity of the case was now removed, and its nature evident; we had to deal with malignant affection of the bone.

Being satisfied of the nature of the disease, the only question now was, whether amputation should be performed; and this was decided in the affirmative. In coming to this conclusion, the following circumstances were taken into consideration, and had their influence:—1. There were no enlarged glands below or above the clavicle. 2. The patient, though thin, pale, and exhausted, had not a sallow countenance or anxious look. 3. The disease had arisen from a local injury. 4. Although the impression communicated by the finger was, that the disease was medullary sarcoma, yet it was just possible it might be another affection of less malignant character. 5. It is not clearly made out that the diseased changes of bone, included under the general name of malignant, are all equally so, or that they are all identical with the medullary sarcoma of other parts. 6. And if it was medullary sarcoma, however common it unfortunately is that the disease reappears elsewhere after operation, there are not yet sufficient data to shew that it invariably does.

There was one unfavourable symptom, cough with expectoration; this had been present for a few days. I believed it, however, to be dependent on an affection of the mucous membrane of the bronchi merely, and did not therefore delay the operation, to which the patient at once assented, and which was performed two days afterwards.

Removing the arm at the shoulder is one of the most easy and most expeditious of amputations. There is none, however, for which a greater variety of modes has been proposed. The circular incision has been applied to it; more commonly it has been done by flaps, single or double, taken above from the deltoid, or below from the axilla; or anterior and posterior flaps; these being made sometimes by incision from without inwards, at others by transfixion, and from within outwards. Of the various modes of operating, no one mode can be exclusively adopted. Amputation in this situation being more frequently required for injuries than dis-

ease, the nature of the former must, in individual cases, decide the direction of the incisions. You must take the flaps, or flap, from whence the state of the soft parts will admit; you may even have to take a flap from the axilla. But where the state of the parts allows of a choice, lateral flaps and a perpendicular line of union are to be preferred.

In this, as in other amputations of the larger limbs, you must of course adopt temporary means for stopping the flow of blood until the member has been removed, and the mouths of the vessels are permanently closed by a ligature. In the mode of operating which I adopted, the two lateral flaps are formed, the joint is opened, and the head of the bone disarticulated, before the axillary artery is divided by the last stroke of the knife; so that it is in the power of the assistant, or even of the surgeon himself, to compress it before its division. You would observe, however, that although I meant that compression of the artery should be made in this way previous to its division, yet that I had at the same time pressure made above the clavicle. I adopted this precaution because there was a morbid state of parts in the axilla—there was a tumor, and I did not know how far the axillary artery might be displaced from its natural situation, and thus escape the grasp of my assistant.

The patient was seated on a chair, with his right side to a table, on which the sound arm rested. The left arm could not be elevated, so that it remained by his side until the joint was opened. Placing the point of a large scalpel on the top of the acromion, an incision was made through the skin and muscles, obliquely downwards and forwards, and then, beginning at the same point, another downwards and backwards; each terminating respectively at the anterior and posterior edge of the axilla: so that the incisions had the form of the letter V reversed, or with the point upwards. The flaps (which were slightly convex forwards) being raised from the subjacent parts and held a little aside, I ran my knife immediately below the acromion on the head of the bone, but meeting with no resistance, I put my finger in the wound, found the bone entirely gone, and only the cartilaginous covering of the head remaining, like a shell. Dividing rapidly the capsule of the joint and the attachments of the tendons, I drew this cartilaginous shell and the diseased mass to which it adhered outwards with my left hand, and passing a small amputating knife behind the whole, by a single stroke of it severed the remaining attachments of the limb; which attachments, including the axillary artery, had been previously grasped above by my assistant. Before doing this, in attempting to separate the diseased mass anteriorly, a portion of it was found to run so deep behind the inner edge of the glenoid cavity, that to dissect it out before the bleeding vessels (branches of the circumflex arteries) were tied, would have taken longer time than was consistent with the patient's safety. This portion was subsequently, and easily, removed, its surface being covered by a sort of cyst; its attachment, or rather its prolongation, extended to the inside of the neck of the scapula, between this part and the sub-scapularis muscle.

You no doubt remarked, gentlemen, that on the limb being severed, the axillary artery spouted; but that this was at once stopped by a finger placed on its mouth. You may ask how it came to bleed. The fact was, that by the tumor in the axilla, a portion of which, as I have just stated, at the moment remained behind, the artery, had been forced out of its natural situation, and had therefore eluded my assistant's grasp. But, again, how was it that the pressure above the clavicle did not prevent the bleeding? I presume that the patient being faint, had at the moment yielded, and therefore the pressure was not effectual. You will do well to recollect this occurrence; I mean more especially the ease with which you saw bleeding from an artery of the largest size stopped simply by the pressure of the point of the finger on its mouth: in cases of hæmorrhage from wounded or divided arteries, the remembrance of this fact may save you a great deal of anxiety and confusion; and by doing as you saw done here, you will have time to make pressure higher up, and thereby be enabled to put a ligature on the artery at the wounded part.

On examining the glenoid cavity of the scapula, it was found to be sound, and so was the fringe of capsule attached to it. The cartilage of the former was neither pared or scored, as have been by some recommended. I consider these proceedings uncalled for, and we had in this case no reason to regret their being omitted. In bringing the wound together, you saw that two stitches were used with intermediate straps. If there was formerly a prejudice in favour of stitches, there is now in the minds of some surgeons a prejudice against them, seeing that it has been sought to exclude them even here. To me this seems precisely one of the situations wherein they are most applicable: there is no support for straps or a bandage, at least so as to allow the edges of the wound to be kept by either of them nicely in contact.

On examining the limb, the upper part of the humerus was found to be

entirely absorbed, from the cartilaginous shell of the head of the bone down to the insertion of the deltoid; and on a longitudinal section of the parts being made, the distance between these two points was found by measurement to be four inches and a half. The upper end of the remaining portion of humerus and the articulating surface of the head were connected by a soft (relatively speaking) substance, covered by a firm membranous envelope. The consistence of the former substance was similar to that of liver; its fracture had a fibrous character; a section presented a smooth uniform surface and appearance; and had a straw colour, with a very slight tinge of pink. To the under and middle portions, the preceding description is more particularly applicable, for the upper portion, which was much more voluminous, was of a softer consistence, and of a dark red, almost black colour, but apparently composed of the other substance, broken up and mixed with blood. That the greater part of the latter appearance was occasioned by the fingers introduced into the puncture, and by the examination of the parts thereby made two days previous to the operation, I am satisfied. The portion described as having been prolonged on the inside of the neck of the scapula, and which could not have been reached by the finger, was, however, very soft, of a gelatiform, not brainlike appearance, and of a light claret colour. The disease seemed to have commenced in the medullary cavity of the bone, in the medullary membrane. Within the extremity of the remaining part of the humerus, for the extent of three-quarters of an inch, the same diseased mass existed as has been just described, and to a corresponding extent the osseus parietes were partially absorbed, the absorption taking place wholly from within. On the outer surface of this portion of bone no absorption had taken place, and it preserved its natural dimensions. There were no bony spiculæ throughout the diseased mass, two or three short ones at the ragged end of the bone, and attached to the periosteum, excepted. The morbid formation was contained in a dense fibrous capsule, composed of expanded and thickened periosteum, and continuous with that of the remaining bone. Into this dense capsule the tendons of the scapulary muscles were inserted, as well as those of the latissimus dorsi and pectoralis major; the origin of the middle head of the triceps was also traced from it. The contained mass was united to the cyst by a very fine cellular membrane apparently, but probably delicate blood-vessels. I have already told you that the cartilaginous covering of the bone was whole; the articulating surface of this had a perfectly natural appearance, and so had the synovial membrane, which was entire. In fact, the disease had not penetrated into the cavity of the joint, although by expanding the periosteum it had overlapped this and extended on the inner side over the edge of the glenoid cavity and the neck of the scapula.

The diseased mass is evidently a new formation; it does not arise from expansion and softening, from conversion or alteration of the osseous tissue: the two are perfectly dissimilar and distinct; there is no gradual transition from the one to the other; the bone at its ragged end is as hard as a healthy bone. The large portion of the humerus which has disappeared has been absorbed in consequence of the pressure of the tumor from within, just as sometimes occurs in cases of aneurism from pressure without. The mention of aneurism leads me to advert to the preservation of the cartilaginous covering of the head of the humerus, as analogous to what we find happening in aneurism of the aorta compressing the spinal column, when the intervertebral cartilages sometimes remain entire, the bodies of the vertebræ being absorbed. The bone and the new formation are in the present case contiguous, not continuous; they are connected by periosteum externally, by medullary membrane internally. The periosteum forms the external envelope of both; on the tumor it is expanded and thickened, but not otherwise diseased. The latter remark does not apply to the medullary cavity, in which, at the distance of an inch from the end of the bone, you can indeed state that the medullary membrane is natural and healthy, but where, in the recent section, you could not point out the limit of the healthy condition, nor where the diseased change commenced; but since the parts have been in spirit, a change of colour in the morbid formation has given to it a seemingly distinct line of demarcation.

Here you have again an opportunity of seeing the diseased parts; and although the colours have undergone some change from the action of the spirit, you will, with the assistance of this drawing, readily recognize the truth of the description I have given.

You are now also prepared to understand and to explain the symptoms which attended this case; you may perhaps even be ready to express surprise that the nature of the disease was not recognized until the swelling was punctured. But gently! The sense of fluctuation is in these cases sometimes so apparently distinct, that the most experienced surgeons have been deceived by it, acknowlege their having been so, and that they have punctured similar tumors under the idea that

they contained matter or other fluid. The cause of this deceptive sensation is to be found in the nature of the tumor and its envelope: the reason of the motion in the limb is self-evident; that of the grating sensation on rotation depended on the motion of the osseous spiculæ and ragged end of the bone, whilst the extreme pain attending the least motion of rotation, or the least attempt at elevation of the limb, arose no doubt from the action of these on the adjoining diseased periosteum.

From the day of the operation, Slater had not a bad symptom; from the first night he enjoyed sleep, such as he had not known for months. The cough was indeed for some days troublesome, and the expectoration copious, but both gradually ceased. The ligature from the artery came away on the thirteenth day, the wound healing kindly. Under a liberal diet he has rapidly gained flesh and strength, and on Tuesday, a month after the operation, he leaves the hospital.

In conclusion, what are the patient's prospects of permanent health? The answer will depend on the nature of the disease for which he has undergone amputation. Was this clearly medullary sarcoma, or was it one of those diseased changes of bone which are included under the name of osteo-sarcoma, which, though malignant, as regards their intractibility, are not yet proved to be universally so malignant as connected with a diseased state of constitution, as genuine medullary sarcoma is? We do not yet know if in bone tumors of a different nature are not included under one and the same name. In any case the operation was his sole chance. The diseased parts have been entirely removed; he has recovered. Let us hope the future will confirm the present favourable appearances.

MIDWIFERY HOSPITAL REPORTS.

GENERAL LYING-IN HOSPITAL.

To the Editor of the Medical Gazette.

Sir,

The publication of hospital reports has long been a desideratum in this metropolis, and although it has been, to a considerable degree, supplied by the Medical Gazette, as respects cases in medicine and surgery, still, with the exception of a few cases interspersed here and there, scarcely any report of midwifery practice has been given to the public; and I believe I am correct in stating, that no *Midwifery Hospital Reports* have as yet appeared in your valuable journal. I therefore take the liberty of sending you a short account of the various cases of interest which have occurred at the General Lying-in Hospital during 1833.

I have the honour to be, sir,
Your obedient servant,
Edward Rigby, M.D. F.L.S.

Assistant Physician-Accoucheur to the General Lying-in Hospital; Lecturer on Midwifery at St. Thomas's Hospital.

44, Parliament-Street,
March 28, 1834.

During last year, 180 women were delivered in the hospital; of these, the exact position of the head was ascertained in only 57, chiefly owing to the absence, for some months, of house pupils. In 35 cases the head was born in the first position—viz. with the posterior fontanelle turned towards the left foramen ovale; in 22 it was born in the second position—viz. with the posterior fontanelle towards the right foramen ovale; of these last, six were originally felt by myself in the third position—viz. with the posterior fontanelle towards the right sacro-iliac synchondrosis; but which, as labour advanced, gradually moved forwards to the right foramen ovale. My being able to record but six cases of the third position, is simply owing to other engagements having prevented my being with the patient sufficiently early before the head had passed into the second position *.

I will briefly mention two cases where the head presented in the third position, as tending to give a good view of the mechanism of parturition under such circumstances.

"July 23, 1833, Anne Zuber, æt. 28, delivered of a girl; fourth child. Posterior fontanelle could be reached with difficulty towards the right sacro-iliac synchondrosis; anterior fontanelle very distinct behind the left foramen ovale; little liquor amnii between the head and membranes, although she is very large, and always has had a great quantity in her former labours. As the head advanced, the small fontanelle came more and more forwards; the child was unusually large, and was followed by a torrent of liquor amnii. A slight hæmorrhage appeared, and a very large placenta was expelled."

"December 20, 1833, Mary Abbott, delivered of a boy; first child. When the os uteri was nearly dilated, the sagittal suture was felt in the right oblique diameter; the posterior fontanelle to the right sacro-iliac synchondrosis. As the head entered

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* During a three months' residence at the hospital, shortly after my return from Germany, of forty-one labours, where the position of the head was carefully ascertained at an early period of labour, twenty children presented with the head in the first position, and twenty-one with it in the third. Generally speaking, the head does not present so frequently in the third as in the first position; being in the proportion of two to five.

the cavity of the pelvis, the posterior fontanelle gradually advanced towards the right foramen ovale; the occiput passed under the right ramus of the pubic arch; the child's face turned towards the back part of the mother's left thigh."

Of twins there has been but one case, and that was at the eighth month of utero-gestation.

"May 4th, 1833.—Anne French, æt. 31, first pregnancy, delivered of twins (boys). First child presented with the head, the second with the feet; premature; both children alive, but very weakly; the second larger than the first; menstruated last in the middle of September. When she was about three months gone, she had a slight discharge of blood, which lasted for some weeks. The placentæ were united, each child being in a separate bag of membranes. In the sulci between the cotyledons, numerous openings, irregular both in size and shape, were observed; their edges well defined, and evidently not produced by laceration. In one of the larger ones, canals could be distinctly seen branching off in different directions, much in the same manner as the bronchi do."

The subject of the placental circulation, at the time this case was taken, interested me considerably, and I am glad to have this opportunity of laying before you one of the original cases from which my observations last year upon this subject were taken, and give it as it stands in the hospital case-book.

The placenta of twins has been rather a subject of controversy among accoucheurs and physiologists, and must, I apprehend, still remain, to a certain degree, in obscurity. The form here described is that which occurs most frequently, nor have I myself seen any twin case where there was much material difference. In some instances, it is true, the edges of the placentæ might not occasionally meet, having a membranous space of half an inch, or even more, between them; but in all the cases which I have had the opportunity of examining, the circulation of one placenta was *distinct* from that of the other; and although, in the greater number of twin cases, the placentæ are, to all appearances, united, still, if we carefully separate the membranes of one ovum from those of the other, we shall frequently succeed in separating the placentæ also. A twin placenta which has been injected with wax, and is still moist, shews this very distinctly. The formation of twin placentæ has been noticed by Mauriceau, Portal, and in some degree by Levret. "If (says Mauriceau) there be two children in the womb, they have commonly but one placenta, which has as many cords attached to it as there are children; nevertheless, they are separated from each other by their several membranes, in each of which the children are distinct with their liquores amnii." The fact of two fœtuses being enclosed in the same bag of membranes has been asserted by some authors, but I know of no authentic case to prove it. There is no reason, I apprehend, for supposing that a Graafian vesicle can contain more than one ovum, nor does it seem very probable that, in twins, both ova come from the same ovary, although in cases where there are more than two children, this must necessarily have been the case. Preparations, however, of twins are rare, more especially cases of first pregnancy, which, of course, are best adapted for inquiries of this sort. I had once an opportunity of examining a beautiful preparation in the museum of Professor Soemmering: there were twins in utero at about the third month, and a corpus luteum in each ovary. The subject, although still imperfect, is interesting; and if any of your readers meet with cases tending to elucidate it in any way, I trust that they will communicate to you the result of their observations.

There has been but one presentation of the face, and that was under rather peculiar circumstances; so that I regret not having been able to be with the patient during the whole course of her labour.

"July 9th, 1833, Anne Roe, æt. 22, delivered of a girl; first child. Was called to her, as the face was suspected to present. Upon examining, I found the os uteri not fully dilated, with its edge thin, and the head presenting at the left side of the pelvis; but, either from the size of the pelvis or smallness of the head, it was in constant motion, and although I at first felt something like the face to the right of the pelvis, after a considerable movement of the head I merely felt the suture and cranial bones, and pronounced it to be the small fontanelle backwards and to the right. I was obliged to leave the patient, but the intelligent midwife of the hospital informed me afterwards, that as the os uteri dilated, the face entered the pelvis, and the chin passed with tolerable ease."

I am inclined to think that my diagnosis was so far right, that it was the great, and not the small fontanelle, which I felt towards the right sacro-iliac synchondrosis, after the movement of the child's head. We know it to be a fact that the most common presentation of the head is with the occiput to the left foramen ovale, and of the face with the chin to the right. It might be fairly asked, are not such presentations of the face originally first positions of the head, which, by turning upon its transverse axis, has allowed the face to come down in this position? I merely mention this as a curious fact; if,

indeed, the above case can be considered as sufficient evidence to even justify its being called so.

The only case of nates presentation which has occurred, was premature—viz. in the sixth month; so that the child was too small to follow any regular course. It had been evidently dead some hours before birth.

A considerable variety of labours have occurred during this period, which I shall bring under the general head of Dystocea; treating of them in the order under which Professor Naegele arranges this subject.

[To be continued.]

MEDICO-CHIRURGICAL SOCIETY.

At a very full meeting of this society, which took place on Tuesday, March 25th, a paper, by Mr. Mayo, was read, entitled "*Observations upon Ulceration of the Cartilages of Joints, and on Anchylosis.*" The following is a summary of its contents.

The author distinguishes three different forms of ulceration of cartilage, which appear to him to have been confounded partly with each other and partly with an affection of a different nature. The cases are thrown into three classes, each illustrative of one of the forms of ulceration of cartilage which has just been mentioned. Class 1 contains instances of absorption of cartilage, beginning upon its synovial aspect; the new surface, if of cartilage, being perfectly smooth; if of bone, healthy: the absorption of cartilage having been attended with inflammation of the capsular synovial membrane.

Upon the cases brought forward in this class Mr. Mayo observes, that they are of rare occurrence; that the absorption of cartilage is very rapid; and that it is attended with severe inflammatory pain and inflammation of the capsular synovial membrane; generally, also, with suppuration in the cellular tissue adjacent to the joint. The only favorable termination of the disease which Mr. Mayo has observed has been anchylosis.

Class 2 comprises cases of ulceration of cartilage, beginning upon its synovial aspect, producing an irregularly-excavated surface, with fibrous or brush-like projections of the cartilage or synovial membrane, where it is reflected over the cartilage; the bone, and the surface of the cartilage towards it, being healthy. In the 3d class are to be found instances of ulceration taking place on both aspects of cartilage, but principally on the surface next the bone, attended with inflammation not only of the synovial membrane, but of the adjacent surface of the bone, and in some instances with inflammation of the cartilage itself. This disease the author considers to be clearly distinguishable from ulceration of cartilage beginning on the synovial surface. As far as his experience extends, he has found it considerably less manageable; for though it may occasionally yield for a time to treatment, the joint remains strongly disposed to relapse into the disease. The circumstance which appears most opposed to recovery in this affection is the partial or complete separation of flakes of cartilage from the bone, which would prove an additional source of irritation. This complaint, again, is not less distinguishable from the disease which Mr. Brodie has described under the name of "scrofulous disease of the joints, having its origin in the cancellous structure of the bone."

Upon the subject of *anchylosis* Mr. Mayo observes, that there appear to be three varieties in the mode in which the ends of bones become united after partial or complete destruction of their natural articular surface: one is *bony anchylosis*, in which the ends of the bones become united after the complete absorption of the cartilages and synovial membrane; a second is *cartilaginous anchylosis*, in which a junction takes place between surfaces, both of which are cartilaginous, but have previously been partially ulcerated; the third may be called *mixed anchylosis*, in which a surface of cartilage, that has undergone partial ulceration, is united to a denuded surface of bone. These distinctions, indeed, refer to differences rather in the nature of the surfaces that become united than in the mode and process of union. In each of the three kinds it may be proved, or rendered very probable, that an exudation of coagulable lymph, adhering to the surfaces which are in apposition, first glues them together mechanically, and afterwards becomes an organized medium of union. The layer of lymph is of very variable thickness: when in contact with bone, it is presumable that it gradually becomes ossified; when interposed between cartilages, it probably becomes absorbed after union has taken place. The same may happen, in part, in bony anchylosis.

The opinions advanced by Mr. Mayo in this paper, are supported and illustrated by an extensive series of preparations (injected and otherwise), from the Museum of King's College. Some of the specimens were of remarkable beauty, and strongly corroborative of Mr. Mayo's views. We particularly noticed some preparations, shewing the *vascularity* of cartilage under states of diseases, the different forms of ulceration and absorption of cartilage, and the varieties of anchylosis.

ROYAL INSTITUTION.

Friday, March 21.

"A DAY AT POMPEII."

A most vivid and satisfactory description of Pompeii, *as it is*, was given this evening, by Mr. Hellyer, who visited the spot in the winter of 1832-3. By the aid of abundance of drawings, prints, plans, and sketches, illustrated by highly-interesting oral explanation, the lecturer was very successful in the object which he specially had in view—namely, that of comprising in an animated narrative, an hour long, an account of things seen and done by him at Pompeii in the space of a day. Vesuvius, at the time of his visit, was in full eruption, or at least sufficiently so to enable him to appreciate the fine description given by Pliny, in his Letters (vi. 16 and 20), of the awful catastrophe by which the city was drowned with ashes in the year 79. Mr. H. read an ample extract from the work, pointing out the localities mentioned, and explaining several of the incidents alluded to. It would be difficult to follow the learned lecturer in all his details; nor, indeed, could we afford space for even an enumeration of the various objects which he so well described: the freshness of most of those objects, after so long a period of concealment, seems chiefly to have impressed his mind; and he dwelt with satisfactory minuteness on the interior arrangements of the houses, the triclinia, the bath-rooms, and the sleeping apartments. Some of the houses appear to have contained above 60 rooms: the walls of some of them were found highly decorated with colouring and paintings; and the furniture, the bronze chairs, and candelabra, were of much elegance and taste. It added not a little to the gratification of the visitors to see lighted before them this evening one of the lamps found in Pompeii, which had not been so used for at least 1755 years. We may add, that some glazed windows were found in the ancient city, particularly where they were absolutely wanted as skylights for the baths. Abundance of skeletons were discovered about Pompeii, many of them in painfully interesting attitudes. And we should not forget to mention that we derived much pleasure from the description Mr. Hellyer gave of the fine Mosaic of the battle of the Granicus.

MOSCATI'S PAMPHLET.

We have received the pamphlet. It is indeed a very extraordinary production. But although the disclosures about the *Gazette* and *Lancet* are not a little amusing, we cannot notice them as we presume it was intended we should: we do not wish to interfere in the dispute, and have not forgot the old adage—*inter amicos ne sis judex*. As to the rest, the circumstantial nature of the details (times, places, and persons, being mentioned, as well as extracts from letters being given) will make it easy for Dr. Elliotson to disprove any parts of the narrative which may be untrue; and we take it for granted he will feel himself called upon to do so.

NEW MEDICAL WORKS.

Pathological and Surgical Observations on the Diseases of the Joints. By B. C. Brodie, V.P.R.S. Third edition, with alterations and additions. 8vo. 10s. 6d. bds.

The Signs, Disorders, and Management of Pregnancy; the Treatment to be adopted during and after Confinement; and the Management and Disorders of Children. By Douglas Fox, Member of the Royal College of Surgeons. 8vo. 6s. cloth.

The Principles and Practice of Obstetricy, as taught by J. Blundell, M.D., with Notes by T. Castle, M.D. 8vo. 21s. bds.

In the Press.

Cox's Essay on the Symptoms, Treatment, Tests, &c. of Poisons. With coloured Plates. Sixth edition.

WEEKLY ACCOUNT of BURIALS,

From Bills of Mortality, *April* 8, 1834.

Abscess	2	Hooping-Cough	8
Age and Debility	43	Inflammation	50
Apoplexy	4	Bowels & Stomach	2
Asthma	20	Brain	5
Cancer	4	Lungs and Pleura	10
Childbirth	2	Insanity	3
Consumption	82	Jaundice	1
Constipation of the Bowels	1	Liver, diseased	2
Convulsions	27	Measles	10
Croup	1	Mortification	3
Dentition or Teething	5	Paralysis	2
Diarrhœa	1	Rheumatism	1
Dropsy	11	Small-Pox	6
Dropsy on the Brain	22	Sore Throat and Quinsey	1
Dropsy on the Chest	3	Spasms	1
Erysipelas	1	Thrush	2
Fever	10	Tumor	1
Fever, Scarlet	3	Venereal	1
Fever, Typhus	2	Unknown Causes	84
Gout	3		
Hæmorrhage	1	Stillborn	27
Heart, diseased	2		

Increase of Burials, as compared with the preceding week . . . 287

Erratum in last Number.—Page 11, line 21, *for* "head," *read* "feet."

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THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, APRIL 19, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XXVIII.

DIFFICULT LABOUR.

Orders.—We shall now proceed to the consideration of the second part of our division—viz. *difficult labours*; which class embraces two orders—the first *lingering*, and the second *instrumental*.

Definition.—I have defined a lingering labour to be a case in which the head presents; which occupies more than twenty-four hours from its commencement to its termination; which is concluded without the necessity of instrumental or manual interference; during the progress of which no dangerous or unusual symptoms manifest themselves; and in which nothing calling for anxiety occurs, except the length of time that elapses under its continuance: so that it differs only from a natural labour in respect of its duration.

We sometimes hear of a woman being in labour a week, ten days, a fortnight, or even longer. Such an idea is perfectly absurd: the powers of the system could not bear up against the exertion of labour for so protracted a period. Besides, the active agents could not support their operations for so long a time: for the uterus, whether it is muscular in structure or not, obeys the laws of muscular action under parturition; and we know that if muscles be compelled to work for a great length of time, they at length refuse to act; and the same occurs with regard to the uterus. Its powers become gradually enfeebled, under a continuance of excessive toil, and it is at last entirely disabled through exhaustion: with the cessation of its action, the process of labour is also at a stand. This is exactly analogous to what we observe daily and hourly in the truly muscular structures: when fatigued, they contract feebly and unwillingly, and when their powers are exhausted they can exert themselves no longer. Such cases, then, of reputed prolonged parturition, are dependent on false, irritable, spasmodic pains, situated in some other part of the body; by which, as we have already learned, women are frequently harrassed towards the close of gestation; but which are perfectly unconnected with, and independent of, contraction in the uterine fibres.

In estimating lingering labours, we calculate from the first commencement of true uterine action; but in estimating the length of labour, in reference to the patient's strength, and its effects on her system, we only take into consideration the time that has elapsed since the membranes broke; for it is reasonable to infer that no great exertion has been sustained—consequently that little or no exhaustion has appeared; and particularly that scarce any injurious pressure can have taken place on the soft parts within the pelvis, while the membranous cyst remained entire. Thus, when called to a case of lingering labour, in considering the chance of injury from its duration, our mind should be directed, not so much to the interval which has elapsed since the first accession of uterine pains, as to the time at which the membranes ruptured; and that should be looked upon as the period when it was possible for dangerous pressure to have commenced.

Causes.

Many and various are the causes which may produce a lingering labour. They may be arranged under two distinct heads—namely, those which are referable to the mother, and those in which the ovum is at fault. The causes referable to the mother are—

First, the want of sufficient power, or the absence of sufficient energy, in the uterus itself;
Secondly, the want of sufficient room in the bony pelvis, to admit the ready passage of the fœtus;
Thirdly, the presence of one or more tumors in the pelvic cavity;
Fourthly, rigidity of the os uteri, vagina, and perineum—one of these organs being affected singly; or a combined rigidity of the whole tending to retard the progress;
Fifthly, a cicatrix in the vagina; and,
Sixthly, obliquity of the os uteri.

Those causes of lingering labour referable to the ovum, *are said to be*—
First, preternatural toughness of the membranes;
Secondly, the head being enlarged, from natural healthy formation, deformity, or disease;
Thirdly, the head being too strongly ossified, though not of larger dimensions than ordinary;
Fourthly, malposition of the head;
Fifthly, ascites or tympanites of the fœtal abdomen;
Sixthly, the umbilical cord being unnaturally short, or being twisted around the body or limbs of the fœtus;
Seventhly, unusual bulk of the trunk or limbs; and,
Eighthly, monstrosity.

Some of these causes in which the child is at fault, we shall find, exert a great influence over the duration of labour; while others, as we shall hereafter have an opportunity of seeing, possess no power whatever in retarding the process.

Inefficient uterine action.—We shall proceed, then, to consider the first cause of lingering labour referable to the mother—namely, a want of sufficient uterine action. Labours rendered tedious, or lingering, from this cause, are usually observed in constitutions debilitated by previous disease, by excessive discharges, or some other depressing action. We often remark, also, that the uterus acts feebly when the woman has previously borne a large family. In this latter instance, indeed, the organ does not obey the laws of muscular action; for the more frequently muscles are called into powerful exertion, the stronger they become;—the uterus, on the contrary, usually acts the more feebly when its peculiar powers have been often called into operation.

This cause of lingering labour is known by the pains being feeble; the intervals at which they succeed each other distant; the space of time during which they continue short;—while, at the same time, there probably exists a good pelvis, and a sufficient degree of dilatation and laxity of the passages to allow the escape of the fœtus, provided the propelling power were adequate to the end. In some cases, where the delay is attributable to inefficient uterine action, we may also have the sanguiferous system acting with diminished energy; and there are perhaps other symptoms present, indicative of general debility. It is not very likely that we shall find much difficulty in detecting this cause of lingering labour.

Treatment.—Under these circumstances, it becomes our duty to endeavour, if possible, to rouse the uterine energies; by doing which we may probably prevent the necessity of instrumental delivery. This object we can sometimes easily effect. The pains may be augmented both in frequency and force, and may even occasionally be restored after they had been suspended for many hours;—*first*, by warm diluent drinks; *secondly*, by stimulants taken into the stomach; *thirdly*, by particular medicines; *fourthly*, by friction and other external means; and, *fifthly*, by a change of posture.

Of all the methods employed for the purpose of increasing inefficient, and restoring declining pains, none are more frequently had recourse to, and none are less injurious, than warm diluents; they are the simplest that can be used, and are frequently successful. It is a very common observation of the nurse, when the uterine contractions are weak, short, irregular, and distant, "Shall I give my mistress a little warm gruel to bring back the pains?"—If such nourishment be grateful to the patient, if there be no tendency to vomit, or if she feels to desire it, there can in few cases be any objection to the exhibition of warm diluents; and they may be given almost *ad libitum*. To stimulants, as a general principle, under labour, I decidedly object; and consider it as a maxim never to allow them, unless some degree of faintness be present; or a languid state of the general system indicate their propriety: because the excitement they produce must be followed by a corresponding depression; and they may tend either to induce fever, or hurry on exhaustion. Before stimulants are exhibited, many things must be taken into consideration; such as the state of the pulse and skin; the length of time the labour has lasted; the strength of the pains; the degree of faintness the patient is suffering; and the kind of discharge. Should the pulse be weak and slow, the surface colder than natural, the uterine contractions powerless, and the system depressed, while at the same time there is no blood flowing through the vagina, nor any indication of internal and concealed hæmorrhage, stimulants are indicated; and either the domestic or medicated may be allowed.

Various specific medicines have been recommended at different times, to increase the parturient throes, and facilitate the child's birth; but I believe that the whole of these substances, one only excepted, act upon the womb through the excitement induced in the arterial system. They first stimulate the nervous, then the arterial, and through the medium of those systems, the uterus. Almost the only medicine now used as an uterine excitant, is the *ergot of rye;* and I have no hesitation in declaring my opinion that its action is specific, and that the uterus is not affected through any disturbance first set up in the arterial system.

The secale cornutum, ergot, or spurred rye, is the produce of a disease in that plant, with which one or more of the grains in different ears are simultaneously affected. When attacked with the "spur," the corn first becomes softish and pulpy; and soon bursting from its husk, obtains solidity, and assumes a lengthened form, slightly curved, and pointed at the extremity; its hue is in the first instance red, but it soon changes to a dark violet, or blackish colour. The diseased grain varies much in length, sometimes being perfectly concealed within its husk, at others growing to nearly an inch and a half; its usual length is about an inch, and its general appearance resembles much the spur of the cock. As we obtain the grain, it is dry, and breaks with an irregular fracture, somewhat like a dried almond. On being divided it is seen to consist of a dark cortical and internal bluish white substance; it has no heating qualities, is not unpleasant to the taste, but possesses a slight mawkish flavour. Almost all the grains, and many of the other grasses, are subject to this affection; but it is most usual in rye, and most frequently taken from that grain for medicinal purposes. Wet seasons are particularly favourable to its production; it is sometimes observed in this country, but is more common in Switzerland, the south of France, and in North America. The disease destroys the germinating power of those particular seeds which are attacked with it; but does not affect the sound grains in the same ear. In this plate you have an opportunity of observing the appearance of an ear partially affected with this disease. (The figures are of the natural size.)

The ergot in fine powder is of a perfect ash colour; and its infusion of a dingy violet.

For the facts contained in the following sketch I am principally indebted to a very ingenious pamphlet, published six or seven years since, by the late Dr. Adam Neale.

Although this medicine has only been had recourse to very lately by practitioners,

to increase uterine action, the substance has been known to possess deleterious and poisonous qualities for more than 800 years, and has been employed on the Continent by female midwives, as a promoter of labour pains, for nearly 150 years. If taken in large quantities, mixed with the healthy grain, as food, it produces giddiness, spasms, and convulsions, on which gangrene and sloughing of the extremities supervene; and to this disease the name of *ergotisme* has been given. Its deleterious effects were first recognised so early as the year 1096, by Sigebert de Gremblour. Wendelin Thelius, a German physician, gave an account also of an epidemic which raged in the kingdom of Hesse in 1596, attributed to this disease being so frequent in the grain. In 1648 and 1649, both Saxony and Sweden became ravaged by a si-

milar epidemic; and twenty years afterwards the same accidents took place from the same cause in Blois and Montarges, in France. Nevertheless, it was not till 1670 that the Académie des Sciences de Paris became informed of the singular accidents which had occurred in Cologne, owing to the use of bread there, made with spurred rye. In 1777, M. Tessier witnessed the same occurrences also in Cologne, and made numerous observations and experiments, a very interesting account of which he published in the Memoirs of the Ancient Royal Society of Medicine. Since that time, in different years, its baneful influence has been more or less remarked in France. And although some observers, as Paulet and Model, have thought that the deaths occurring at those times, in these particular districts, were caused by the great vicissitudes in weather and temperature, rather than by the diseased grain; yet the various experiments of Tessier, on animals removed out of the influence of such exciting causes, fully prove the accidents were attributable to the grain itself.

Like many other valuable medicines, the knowledge of this substance was for many centuries entirely confined to its poisonous qualities; and even those who had studied its history in the closest manner were ignorant that it could boast any other. Of spurred rye, first mentioned for its noxious effects in 1096, no notice occurs of it, as an uterine remedy, till 1688, when R. J. Camerarius stated that in some parts of Germany midwives were in the habit of using it to accelerate parturition. No author, however, mentioned it for this purpose from that time till 1774, when Parmentier, in a brief letter to the Editor of the *Journal de Physique*, made known that it was frequently used by Mad. Depille, a midwife at Chamont, as a child-bed remedy.

This letter merely announced the simple fact; it was to M. Desgranges, an able obstetrical practitioner at Lyons, that we are indebted for rescuing this medicine from the hands of the women. He, in 1777, having met with many females who, from a traditionary knowledge, were accustomed to employ it with no little mystery in cases of lingering labour, made some trials with it himself, and published in various journals, at different times, the result of his practice and observations. The American physicians were in the habit of using it many years before it became generally known in England. Both Drs. Dewees and Chapman, of Philadelphia, and Drs. Hosack, Bibby, Prescot, and Stearns, of New York, had employed it repeatedly. It was not, indeed, till the year 1820, that it attracted particular notice in this country, when the attention of the profession was called to it by my friends, Dr. Merriman and Dr. H. Davies, both of whom published some cases in which it had been tried. Since that time it has come into very common use; its powers are almost universally known to the profession; and the medical periodicals of the day teem with the history of cases in which it has been found of service.

On its first introduction, many practitioners,—especially those of the old school,—received with much distrust the accounts of its virtues, published in the various journals, and elsewhere: at last, however, from the mass of evidence accumulated on the subject, they were forced to admit its powers and efficacy. They then took a different ground,—acknowledged its power, but objected to its use:—it was argued, that if it really possessed such influence over the uterus, that virtue must be obtained at the expense of the system; that it must act on the uterus through the medium of the arterial system; and thus should be classed as a stimulant, and as such must produce dangerous excitement. This, as I have just mentioned, is certainly not the case,—as all unprejudiced observers agree in testifying: it acts on the uterus entirely through the nervous system, not necessarily exciting the heart and arteries to increased action; its influence is specific, confined to the uterine organs; and the pulse is only secondarily affected.

Another objection was then urged against it; it was said to be dangerous to the child's life, and a number of instances were brought forward in which the children were born dead after the ergot had been exhibited. It was supposed that the noxious properties of the drug were so great as to have destroyed the child through the mother's system. In most of the cases, however, which were brought forward to determine this fact, the labour had been very lingering, and the child had probably been destroyed, not by any poisonous quality resident in the drug, but by pressure either on the fœtal head, during its passage through the pelvis, or most likely on the funis umbilicalis. I have myself seen many cases in which ergot was given where the children were born alive; and therefore I am warranted in saying that the drug does not necessarily destroy the fœtus. It is by no means impossible, however, that the fœtal body may be greatly affected by drugs received into the mother's system; of which fact I have had an opportunity of seeing some examples, and one of a particularly striking character. A lady, in consequence of suffering severe pains during the last few weeks of pregnancy, was in the habit of taking opium to some considerable extent, until the accession of actual labour: her children, on

such occasions, were always expelled in a drowsy, stupid, almost comatose state, which continued for some hours after their birth. If opium is sometimes capable of producing such an effect upon the fœtus, it is not unreasonable to believe in the possibility of the ergot also being able to produce an injurious influence in a similar manner; and it has happened to me, in four different instances, to witness the death of the fœtus, a few hours after birth, by convulsions, *postquam partus prematurus inductus fuerat ope solùm secalis cornuti.* Three of these cases occurred in the children of the same woman, and in all four the medicine had been given for four or five days in full doses. Thus, then, although I am persuaded the exhibition of the drug does not *necessarily* injure the child, I am not sure that some ill effects may not *possibly* arise to it occasionally.

Again, it was objected that the medicine, if commonly introduced into practice, would be dangerous, because it might be given in cases perfectly unfitted for its use; and that contusions, inflammations, sloughings, and lacerations of the uterus, vagina, and perineum, would frequently follow its injudicious employment. It surely is neither sound logic nor fair argument to adduce as an objection against a valuable remedy, the possibility of its abuse. I would ask, is bleeding never liable to be abused; or mercury—that most powerful perhaps of all our internal medicines?—Would we discard these remedial means because a bungler might misapply them?—Neither is this nor any other medicine to be prescribed at random; we must only have recourse to it in consequence of certain conclusions at which our mind has arrived after a system of severe reasoning.

Still another objection has been taken, stronger and more difficult to refute than any of the former;—namely, that if the medicine possesses such powers in increasing the action of the uterus, it must also possess the power of producing uterine action *ab initio;* and if such were the case, it would be little less than criminal to admit into our pharmacopœia, or into common use, any drug which might be had recourse to, both by unprincipled men and females, to occasion abortion. This argument was answered by a denial; it was said it only possessed the power of increasing the contractions of the uterus when that organ was disposed to act, and did not produce them *ab initio.* In confirmation, it was declared that in those countries and seasons when *ergotisme* was prevalent, miscarriages were not more frequent than usual, which must have been the case if ergot produced abortion.

Forsàn hæc omnia vera;—egomet ipse tamèn permulta vidi exempla, in quibus partus prematurus inductus fuit—septimo vel octavo graviditatis mense peracto—solo secalis cornuti usu; ovuli membranis integris servatis; ore uteri occluso neque digito, neque ullo alio modo ad patefactionem excitato. Quarè hoc medicamentum opinor, etiam ab initio, pactûs dolores inducere posse: et si tam insignes illi vires, appropinquante graviditatis fine, adjudicemus; similem facultatem eidem, dum recens sit exiguumque ovum, negare, absurdum esset. Haud, profectò, propter eam causam secale cornutum ab usu expellendum est. Nihilo minùs medicos oportet notitiam ejus virtutis a vulgo, precipuèque a mulieribus, diligentissimè celantes, in suis pectoribus occultè ferre *.

The drug has been exhibited in various forms, chiefly in powder, infusion, decoction, and tincture. The two first are in my opinion the best modes; the tincture I look upon as almost inert. If given in fine powder, about twenty grains is the proper dose; but I am myself generally in the habit of giving it in infusion. Two drachms may be infused in four or six ounces of boiling water for twenty minutes; a fourth part of the strained liquor should be given at a time, and the dose may be repeated every quarter of an hour, until either its action becomes apparent, or the whole is taken: for I consider it useless to persevere with the medicine if the quantity mentioned produces no effect. I have found that if the infusion be allowed to stand much longer than the time I have specified, it acquires a nauseating property which greatly distresses the stomach. Desgranges used only the black, cortical part, in which he considered its active principle to reside: he gave it in doses of four or six grains, which he found as efficacious as thirty grains of the whole powder. Villeneuve administered it in *lavements;* and he considers this the most efficacious means of employing it, provided there be present much irritability of the stomach.

I have given the ergot in the doses I recommend, for many successive days, on several occasions, and never knew it produce any bad effects upon the mother, except occasionally nausea and vomiting. Usually there is no more evident effect on the system generally than would be observed after taking a cup of tea; but its effects upon the uterus are speedy, powerful, and astonishing. Its action, under labour, mostly commences within fifteen or twenty minutes after its exhibition; and the character of the pains induced through its agency differs materially from the ordinary throes of parturition; so that it is possi-

* The professional reader will easily comprehend the reason of my being desirous to put forth the sentiment conveyed in the text, in a language not *universally* understood.—F. H. R.

ble in many cases to discriminate them, as being actually produced by the drug itself;—they are stronger and more constant than the common pains of labour. When the ergot has obtained a full power over the system, the uterus often acts without any decided intermission for many minutes together;—there being only a slight remission observed,—no interval of perfect ease. M. Chevreul was, I believe, the first to record this observation; the same remark has been made by many other physicians; and I have had myself an opportunity of observing its truth on many occasions.

As the ergot undoubtedly possesses such a strong influence over the uterine system, it is evident that if exhibited improperly it is likely to do great injury.

There are many cautions, then, necessary to be attended to in adopting and employing it. Its exhibition must not be thought of in any case where a disproportion exists between the head of the child and the pelvic cavity; we should incur great danger of inducing contusions, inflammation, and laceration. Neither must it be exhibited where there is any disposition to rigidity of the parts either of the os uteri, the vagina, or the perineum, through fear of the same dangers. As a principle, it is not usually necessary in first children, and therefore this is a case in which we generally should make an exception. It must not be given in any case where the lingering labour depends upon a malposition of the head. It may be admissible occasionally in breech presentations; but in no other kinds of preternatural labour should we ever think of administering the ergot. It must only be given in cases where the sole cause of delay is a torpid or feeble state of uterine action; or where it is desirable to terminate the labour speedily,—and that too by means of the natural powers,—in consequence of hæmorrhage. I have found it very useful in accidental hæmorrhage after the membranes have been ruptured; in loss of blood under abortion, where it was impossible to empty the uterus by manual operation; and where the patient would perhaps have sunk from the continuance of the bleeding.

But although I am perfectly convinced of the powers which the ergot sometimes displays under parturition, I am by no means inclined to agree with those practitioners who think that this medicine will entirely supersede the necessity of any other means being used in lingering labour. I cannot coincide in the opinion expressed by Mr. Michell, of Cornwall, who published a treatise on this drug in the year 1828, and stated his conviction that its general introduction will so completely supersede the use of the forceps, that "he would not be surprised if in twenty years that instrument should be known only by name." And again, he says, "that except in the rare cases in which the Cæsarean operation was formerly recommended, I conceive there will now be no occasion for instrumental aid in midwifery." Such sentiments as these, and such recommendations as Mr. Michell has promulgated, are calculated, in my opinion, to do immense mischief, because they must lead to the blind, injudicious, and dangerous employment, of a very powerful agent. Nor is he likely to serve the cause of which he has stood forward as such a staunch supporter, for he would raise our expectations so high as to lead us to look for impossibilities; and, as often happens with overzealous advocates, he has ruined his case by proving too much.

Authors vary very much in the statements they furnish of their success with this grain. This discrepancy may partly be accounted for by the ergot not being fresh, since by being kept it loses its virtue. It may also be owing partly to the constitution of the patient not being susceptible of its peculiar action. We know that some persons are not susceptible of the peculiar action of mercury; and we may easily believe that in the same manner some constitutions may be insusceptible to the peculiar action of the ergot of rye.

Stimulating clysters, principally composed of the purgative salts, have often been found useful in exciting the uterine fibres to more powerful contraction.

External means are sometimes had recourse to for the purpose of increasing uterine action. Warmth, applied by means of hot flannels to the uterine region, to the legs, the thighs, and the back, has been tried, but has seldom been found efficacious in restoring uterine action, unless there has been depressed arterial energy, or a cold surface. Under such circumstances we should not only apply warmth externally, but give warm diluents, or perhaps stimulants. Pressure and friction are found to possess greater power over the uterine fibres than warmth externally applied. The pressure occasioned by a bandage, or the pressure of the hand, will often excite the uterus to increased action both before and after the birth of the child. I shall hereafter tell you, that in cases of hæmorrhage after delivery, dependent on a flaccid state of the uterine fibre, we possess no more serviceable means to ensure its permanent contraction than the application of pressure by the grasp of the hand; and although I consider that pressure acts more energetically upon the uterus when the organ is more or less emptied of its contents,—at any rate

after the birth of the child,—yet I am persuaded that firm steady pressure will sometimes excite the uterus to more vigorous contraction, even while it contains the fœtus within its cavity. Friction previously to the birth of the child is more frequently had recourse to under lingering labour than pressure, and a most efficient means it sometimes proves. I do not allude to friction in the way that Dr. Power recommends, viz., with the tips of the fingers all brought together, and run quickly over the abdomen, as if playing on a tamborine, and continued for six, eight, or ten hours successively. If friction be employed, it should be done gently with the open palm of the hand for ten minutes or a quarter of an hour together. This, however, is inconvenient; in many cases it is not possible to have recourse to it at all; and in no instance should a medical man himself be the operator, but a female friend might be directed to do it. Dr. Power thinks that friction in the manner he has described both excites the uterus when dormant, and relieves the patient from false pains, by determining the "parturient energy" from the particular part which is the seat of pain to the uterus. He considers false pain as a metastasis of the parturient action; and he thinks the excitation of the friction over the uterine region restores the action to its proper and natural seat, the uterus. It is an ingenious idea, but I think, like many other ingenious views, this also is fallacious.

I am inclined to think that electrical shocks—particularly derived from the galvanic battery—would excite the flagging powers of the uterus under labour, and perhaps even induce action *ab initio*. This is a means, however, of which I by no means recommend a trial; and I only judge by analogy, in consideration of the influence the electrical fluid exerts over the nervous system generally, and, through that system, over muscular contraction.

We may sometimes also succeed in rendering the uterine contractions stronger and more efficient by changing the patient's position, particularly from the recumbent to the upright posture; and as this is a very simple means, as it is often useful, and as the change brings great relief to the patient, she may be advised to sit, stand, or walk, as her own inclination dictates. The effect is most probably produced by the gravitation of the head upon the os uteri.

Before concluding this part of my subject, I must again repeat to you the caution previously given against unnecessarily rupturing the membranes during the first stage of labour. It has of late become very much the practice,—attributable in some measure, perhaps, to the recommendations inculcated by Professor Burns,—to evacuate the liquor amnii in all cases where the uterus is acting feebly; and many instances have come under my own observation in which not only has this act disappointed the intention of the operator, but been followed, after the lapse of some time, by such symptoms as required that the labour should be terminated instrumentally. I do not mean to say that a protracted case is always a necessary consequence of such interference; for in many instances where the os uteri is perfectly dilatable, where it has acquired the diameter of the size of a crown, and especially where there is an excessive quantity of liquor amnii present, the evacuation of the water,—by causing the head to bear more decidedly on the os uteri—will sometimes increase the vigour of the contractions, and bring about a more speedy termination. But I allude to it as a generally-adopted principle; and cannot but consider that such an interruption of nature's ordinances requires in practice the greatest possible judgment and discrimination.

Nor must I allow the custom of irritating the mouth and neck of the womb with the finger, and rubbing the finger down the back face of the vagina, along the rectum, to pass unnoticed; nor that still less justifiable mode of proceeding—the endeavour to dilate the os uteri by the two first fingers introduced within it; which last means also has received the sanction of the deservedly great name of Professor Burns, as applicable to some states of the os uteri; but which I do not feel myself warranted in mentioning except in terms of reprobation.

CASES AND REMARKS,

By R. Arrowsmith, M.D., Coventry.

I.—Partial Peritonitis.

The following case of partial peritonitis terminating in the formation of pus, which perforated the diaphragm and lung, is an unusual example of circumscribed peritoneal inflammation. The narration is abstracted from notes taken almost every day during the progress of the case, which is interesting from the phenomena which occurred during its course, as well as from its cause. It is presented to the profession for the purpose of elucidating the diagnosis in similar states of disease. The only analogous cases with which I am acquainted are two—one related in Dr. Abercrombie's work on Diseases of the

Stomach, &c., the other by M. Andral, in the fourth volume of the *Clinique Médicale*. As the latter work is untranslated, I have subjoined M. Andral's statement of the case. Dr. Abercrombie's is more briefly described in the sequel also.

Case I.—August 23d, 1833.—J. Postin, æt. 30, waggoner, of spare habit, but muscular, and, previous to the present complaint, always healthy, applied for advice, having walked with difficulty about half a mile. His countenance was pallid and anxious, and indicative of suffering; the breathing was short and difficult, but *without cough;* the pulse quick; the epigastrium was much distended, hard, painful, and tender. As the case seemed important, he was sent home to bed, in order that a more deliberate examination might be made.

24th.—On farther investigation, it appeared, that between two and three weeks previously, being much intoxicated, he was struck on the right hypochondrium with the shaft of the waggon, and knocked down. He was so much hurt as to be obliged to be carried home. An embrocation was first applied, and afterwards six leeches to the right side. Two or three days subsequently he lost a very small quantity of blood from the arm, and he took a good deal of medicine. Nine days after the accident he had an attack of hematemesis*, when his wife says he vomited nearly a gallon of blood, containing much coagulum. Blood passed by stool from Sunday to Wednesday; and on the following Monday he was brought to me.

His breathing is now short and difficult, but without cough or pain in the chest; the epigastrium and whole of the left side of the abdomen, down to the ilium, is much distended, hard, painful, and tender, and sounds completely dull on percussion. The right hypochondrium, the right side of the abdomen, the umbilical and hypogastric regions, are soft, free from pain, and have their natural resonance; the skin is hot; the pulse 120, and weak; the tongue is covered with a creamy fur; he has anorexia and sickness; the bowels are freely open from the medicine, and the motions abound in bile; the face is pallid, not sallow; the right side of the thorax sounds clear; the respiration is natural. About and below the sixth rib on the left side the chest sounds very dull, and the respiration is obscure—higher up it is more audible; there is no *râle*. Decubitus is habitually on the left side.

He was bled freely from the arm, leeches were applied to the epigastrium, and calomel was ordered every four hours, and castor oil if necessary.

Sept. 3d.—The blood drawn on the 24th was not buffed; leeches have been applied daily to the epigastrium, and the calomel continued. The tumor is undiminished, but the epigastrium is no longer tender, unless pressed deeply. There is pain in the left hypochondrium. The sound continues dull on percussion over the parts already described; the breathing is freer. At five P.M. yesterday he had a severe and protracted shivering fit. To-day the urine deposits a thick white sediment, having previously been clear. Pulse 125; head unaffected. The gums are tender, the bowels open, and the motions abound in bile. The tongue is much clearer.

5th.—On the night of the 3d he had a severe pain of the left side, near the margin of the false ribs, for which a blister was applied yesterday, and the pain is now much relieved. At four A.M. he was *seized with cough*, which immediately became distressing, with dyspnœa and free *frothy expectoration*. Pulse 140, and weak. He lies constantly on the left side; and if he changes his position, the cough and dyspnœa instantly become worse. The

* From the alteration in organization which the spleen was found, on dissection, to have undergone in this case, and from the inflammatory symptoms which preceded the hematemesis, it is probable that this hæmorrhage originated in an *active hyperemia* of that organ in particular, and of the contiguous structures. Hematemesis, when it is not a vicarious discharge, or the result of a morbid diathesis of the blood itself, in the greater number of instances is nearly allied to inflammation. About this time I witnessed two cases of severe hematemesis in persons who had been previously affected for a considerable period with gastrodynia and pyrosis, and in whom—notwithstanding the loss of blood by vomiting—venesection, blistering, and purging, were necessary, and led to the perfect restoration of health, so that no organic change in the structure of the stomach or adjacent organs could reasonably be supposed to have pre-existed. Portal has related some curious cases of hematemesis, in which the blood came from the liver through the ductus communis choledochus, and was then discharged by vomiting and purging. On dissection, in the fatal cases of this description, the morbid alteration of the liver consisted of red softening, with enlargement. According to this excellent observer, hematemesis sometimes resolves hepatitis. Portal's observations on these subjects are very exact and instructive. See his *Mémoires sur la Nature et le Traitement de plusieurs Maladies*, I. 242-245, and II. 154-210; and also Latour's *Histoire Philosophique et Médicale des Hémorrhagies*, I 274. Few recorded cases of internal hæmorrhage have escaped the research of the latter author.

epigastric tumor has decreased towards the right hypochondrium, and in that part there is now resonance on percussion. The swelling occupies the remainder of the epigastrium and left hypochondrium down to the left iliac region, and those parts sound quite dull. There is not much heat of skin *.

6th.—Perspiration came on last night for the first time; no rigor or chilliness. The cough and expectoration became *suddenly* very much better about eight P.M. yesterday. The breathing is free if he lie on the left side, and he is then perfectly easy; but if he turn on his back or right side, the breathing becomes immediately distressing. The fluid coughed up yesterday was about a pint in quantity, of a light brown-coloured watery fluid, with about two ounces of pus, which subsided to the bottom of the vessel. This pus was homogeneous, white, and opaque. The epigastric tumor is a good deal diminished; pulse 130.

8th.—Cough slight yesterday, pulse 118; to-day troublesome, pulse 130. Very little fluid coughed up, but it was of a more purulent character, and he says of bitter taste: it has a heavy aromatic odour, not unlike that of bitter almonds. He complains of distress in breathing, but it is not accelerated if he confine himself to the left side. Any change of position causes so much distress, that a stethoscopic examination is scarcely practicable. The chest sounds well, and respiration is natural, *not puerile*, on the right side: on the left the sound is dull, and (in the hurried and imperfect examination which could be made) the respiration scarcely audible†: nevertheless it is heard, and there is no bulging of the intercostal spaces. The epigastrium is quite soft, scarcely at all full, and resonant all over.

11th.—The cough became worse about 11 o'clock last night, but no increase of expectoration took place till eight this morning. During twelve hours from that time, at least three pints of fluid were coughed up. The cough was constant, not violent, and without dyspnœa. At eight this evening the cough and expectoration ceased altogether, and the breathing became difficult; pulse 136. The skin is rather hot; no chilliness. The fluid coughed up had a turbid bluish colour, and was rather thick: some pus subsided, and there were two or three streaks of blood. He says the fluid has a decidedly bitter taste. The tumor of the abdomen has disappeared, and it is now soft and resonant all over.

12th.—The cough has been frequent during the night: at least three pints of fluid have been coughed up in the last 24 hours; it comes up with the slightest effort of coughing. Pulse 135. Decubitus on the left side only.

15th.—The cough has been very troublesome the last few days, and at least a pint of fluid has been coughed up daily. The breathing has been oppressed at times, and the pulse has varied from 135 to 116, small, and compressible. The left side of the chest sounds clearer over the whole upper part, and respiration is audible there, but not very distinct; it cannot be heard at all below the level of the left nipple. The chest was carefully measured to-day with a tape carried over the nipples, and the left side was found to exceed the right by half an inch. The intercostal spaces do not bulge, and the respiration is not puerile on the right side. The tongue is clean; the appetite good; the bowels regular; the sleep refreshing. He has neither chilliness nor perspiration, and his spirits are good.

17th.—A pint of fluid was coughed up yesterday; somewhat less to-day, and of the character hitherto described. To-day there were two masses of blood.

* A saline antimonial mixture was ordered. The calomel was continued at more distant intervals, and castor oil given occasionally. From this period the treatment was directed to the relief of symptoms as they arose.

† The feeble sound of respiration I could not well account for. I am not a skilful stethoscopist, but so obvious a phenomenon as the natural or feeble sound of respiration scarcely admits of mistake. This case at the time was supposed to be a formation of pus involving the left lobe of the liver; and I presumed that the diaphragm was so pressed up that the free expansion of the lung was thus prevented. With the evidence of the post-mortem examination before me, I still infer that the sound of respiration was indistinctly heard, in consequence of the condition of the diaphragm not permitting the expansion of that lung; and this view is supported, I think, by some observations of Laennec on *pleuritic effusion*: "In the instances now under notice," says he, "it would seem that the lung is suddenly choked, as it were, and ceases to admit the air in respiration, although it has hardly yet lost one-fourth of its volume, and is only slightly compressed. And it frequently happens, after the lapse of a few days, that the lung, becoming habituated to the pressure, recommences its functions; so that we again can hear the sound of respiration in some points, although the effusion continues undiminished, or even is somewhat increased."—*Forbes's Laennec*, 2nd edit. p. 438.

19th.—Coughed less the last two days; the expectoration but little, and of frothy muco-purulent character. He breathes well if he lie on the left side, and for a short time even when lying on the right side. Pulse 120; skin rather hot; no chilliness nor perspiration; no thirst; appetite good; tongue clean; bowels regular. It is found that the apex of the heart beats precisely opposite the left nipple; that is, just below the fourth rib *.

20th.—He had a very bad night from coughing, the quantity coughed up being about ten ounces. The fluid does not now separate, but is of a frothy muco-purulent appearance, of a light straw colour, and now and then streaked with blood. He is free from pain.

23d.—Has coughed and expectorated but little since the 20th, till to-day, when twelve ounces of very thick pus were coughed up. The breathing is easy; pulse 116, firmer; the appetite moderate; no thirst; tongue clean. The apex of the heart beats at the nipple; the left thorax sounds moderately well to about that point, and the respiration is feebly audible. He prefers lying completely on the left side, but he can lie for a short time without inconvenience either on the back or right side.

26th.—The cough has been frequent during the last three days, and has occasioned pain in the epigastrium. Nearly a pint of pus has been coughed up daily. To-day, without its visible characters being changed, it has *become decidedly fœtid* †. He has perspired the last three nights; pulse 120.

28th.—A pint of fœtid pus has been coughed up yesterday, and to-day the colour is darker; it is brought up by the very slightest cough. The breathing is oppressed at times; pulse 120, weaker. Emaciation increases, and his spirits fail. He has no rigor nor perspiration. The bowels are regular, the tongue clean, and appetite good.

29th.—He has been sitting up since early this morning, in consequence of a deep-seated pain rather to the left of the epigastrium, which came on yesterday at six P.M., and which rendered the recumbent position painful. The cough and expectoration ceased entirely at four P.M. yesterday. The pulse remains unchanged, nor is the breathing accelerated. The abdomen is soft, and the epigastrium bears moderate pressure. The apex of the heart beats to-day be-

* "The apex" (of the heart in its natural and healthy state) "beats between the cartilages of the fifth and sixth left ribs, at a point about two inches below the nipple, and one inch on its sternal side."—*Hope on the Diseases of the Heart*, &c. p. 4.

† The fœtid character which the pus suddenly assumed might have given rise to a suspicion of gangrene, had the general symptoms authorized it: but they did not. And on what the fœtid smell did depend, is even now difficult to determine. The diagnosis of gangrene of the lungs is not so certain and free from fallacy as is generally supposed, perhaps. Dr. Williams says, "it is the putrid fœtidity which most distinguishes gangrene (of the lungs); and this test enables us to discover the presence of slight degrees of gangrene where it is not sufficiently extensive to affect the colour." (Cyclop. Prac. Med. III. 412.) Laennec, in his description of circumscribed gangrene of the lungs, observes, "in this affection the expectoration is so characteristic, that all the physical signs would be quite insufficient without it. It is sometimes green, greenish or brownish, or of a yellow ash-grey verging on greenish, more or less puriform, and with the gangrenous fœtor. In the beginning of the disease it is frequently different: it has not then the peculiar fœtor of gangrene, although it has a fœtor nearly as insupportable." But he afterwards qualifies the above diagnostic signs by saying, "from the result of several cases of recovery, I am tempted to believe that the fœtor and aspect of the expectoration above described do not necessarily indicate the existence of a gangrenous eschar in the lungs;" and subsequently adds, that in two or three dissections which he had made, he "found nothing which would account for the gangrenous fetor, unless it were the rapidity with which the body generally, and the mucous membrane of the lungs more particularly, ran into putrefaction." (Forbes's Laennec, second edition, p. 226.) But the assumption of a putrid diathesis contained in this remark of Laennec is not borne out by late observations. Thus in Andral's "Clinique Médicale," t. I. 198, a case of chronic bronchitis is related in which the expectoration was grey and fœtid. At p. 224, a similar one, with greenish, very fœtid, expectoration; at p. 448, an instance of fœtid expectoration following hemoptysis, without gangrene; and in T. II. 154, another case of bronchitis, with greenish-yellow, very fœtid, expectoration, and a reference to a similar example recorded by Haller. In none of these cases was there reason to presume the existence of a putrid diathesis, whilst they plainly indicate the necessity of caution in inferring the presence of gangrene from the fœtor and colour of the expectoration.

There is a cause of fœtidity in peritoneal abscesses which has recently been investigated in an ingenious memoir, by M. Dance, in the Archiv. Gen. de Médécine, on the fœtid and stercoral odour of certain abdominal abscesses, which the author refers to the imbibition or transudation of stercoral matter through the intestinal tunics. (Dublin Med. Journal, No. XII. p. 472.) But although M. Dance has fully established his opinion as to one source of fœtid odour in abdominal abscesses, and gangrene is an incontestible cause of fœtid expectoration, there are modifications of fœtid purulent secretions, both in the chest and abdomen, which are not explained by the suggestions hitherto thrown out, and which remain for future investigation. In the case detailed in this communication, pus had been expectorated nearly three weeks, when it became suddenly fœtid, whilst it is almost certain that the parietes of the sac must have been more contiguous to the intestinal tunics in the earlier stage than at a later period, when the sac had become diminished by free expectoration.

low the fifth rib, an inch to the left of the nipple.

30th.—Sat in a chair all night, from dread of a return of the epigastric pain if he lay down: he has, however, no pain or tenderness there now. He has expectorated about a teacupful, with very little coughing; pulse 104, irregular; feet œdematous; appetite not bad; bowels regular; tongue clean, moist. Went to bed at 11 A.M. and lay on his left side, with his head low, without cough or dyspnœa. He is languid, pallid, and extenuated. He has neither chilliness nor perspiration.

Oct. 2d.—To-day he has pain rather below and to the left of the epigastrium, which is augmented by the recumbent posture, by deep pressure, and *immediately on taking food.* He is free from neausea; the appetite is good, and the tongue clean, *moist, and pale.* He has very little cough, and no expectoration, to-day; yesterday he expectorated a teacupful, which was so fœtid that it could not be kept for inspection; the colour was unchanged. To-day severe diarrhœa came on, with feeling of exhaustion, but without pain of the bowels; the motions are of natural colour, but very offensive; pulse 120, weak.

5th.—The diarrhœa is not restrained; the bowels are free from pain, and the tongue is clean, moist, and pale. He has scarcely any cough, and no expectoration. The pain near the epigastrium continues slightly; it is always increased by taking food and lying down.

9th.—The diarrhœa has been severe till to-day, when it rather abated. The cough is sometimes troublesome, attended with very little expectoration, which is of a light straw colour, and always fœtid. His breath is very offensive, particularly when forcibly expired, as in coughing. For several days past the atmosphere has been very humid, and he remarked that the fog, night and morning, oppressed his breathing. Appetite good; tongue clean, moist, and pale.

21st.—The diarrhœa has been variable, but he has seldom had fewer than four or five motions daily, of natural colour, but very offensive. The pain near the epigastrium recurs occasionally, and when present is always augmented by taking food. The cough is not severe; the expectoration trifling in quantity, but fœtid. He complains of the breathing being "tight," but it is not quickened even when he complains most. The emaciation and debility increase.

Nov. 7th.—The diarrhœa has continued*; the cough has been troublesome for the last three days, with oppressed breathing, but scarcely any expectoration till yesterday, when about a tea-cupful, of the usual appearance, and fœtid, was brought up. The pulse has varied from 106 to 130, sometimes irregular. He takes food well, but the emaciation is extreme.

14th.—The cough is troublesome, and the breathing felt to be oppressed, but it is not accelerated. A tea-cupful of pus has been coughed up within the last 24 hours. The diarrhœa has returned, and the pulse is quick and weak. On percussing the left thorax to-day, some blood was immediately coughed up, but it did not recur. The chest sounds clearer in that part, and the respiration is heard rather below the sixth rib. The apex of the heart pulsates in its natural situation. He now lies chiefly on the right side; at first, on turning over to the left, cough and dyspnœa are excited, which soon subside. His appetite continues good, and the tongue clean, moist, and pale. He has neither chilliness nor perspiration, nor exacerbation

* The diarrhœa was treated, after the first day or two, with opium in enemata, as well as by the mouth, chalk mixture, catechu, and kino. He took port wine freely, as well as animal food, from the same period to his death. But the diarrhœa was very little restrained by the remedies employed, nor could the strength be supported by the nourishment which was so liberally taken. The emaciation, debility, and fatal issue of the case, were doubtless attributable immediately to the diarrhœa. On what did that diarrhœa depend? Not, I believe, on any morbid condition of the intestinal mucous membrane, but, as I conceive, on the absorption of the fœtid (it might be incorrect to say putrid) contents of the abscess. I do not know that there are on record any observations which go directly to prove that the absorption of fœtid pus will produce diarrhœa; but the experiments of Gaspard render such an hypothesis not improbable. In Orfila's "Toxicologie Générale," ii. 494, M. G. observes, "L'injection de *pus plus ou moins fetide* dans les veines, dans le tissu cellulaire, et dans les cavités séreuses, permet d'etablir, 1mo., que ce liquide introduit dans les vaisseaux sanguins à petite dose, peut y circuler sans causer la mort, pourvuqu' après avoir determiné un trouble considérable des fonctions, il soit expulsé de l'économie au moyen de quelque excrétion critique, surtout de l'urine ou des matières fécales," &c. The results of the experiments of Magendie are in accordance with those of Gaspard; and the practical conclusions arising out of them appear to be countenanced by Dr. Alison. (Elements of Pathology, 201): see also Andral's "Clinique Medicale," I. pp. 481 and 490, for two cases of gangrene of the lungs, giving rise to profuse diarrhœa, which hurried on the fatal event.

of fever, but the skin is permanently rather hot. For some days past he has not complained of pain of the epigastrium. The face and lips are very pallid; he is greatly extenuated and weak.

18th.—Died at seven A.M., the symptoms having continued without any important variation.

Dissection.—The body was examined at two P.M. by Mr. Nankivell and Mr. Bicknell, Surgeons of the Coventry Self-supporting Dispensary. On opening the abdomen, about a pint of colourless transparent serum was found there; numerous small albuminous accretions were scattered about the peritoneum, but there were no flocculi or false membranes. The liver was seen to occupy its proper situation and relations, but the peritoneal tunic was slightly opaque and thickened. On opening the thorax, the right lung was found perfectly healthy, but very pale; the left had a similar appearance. On that side there were adhesions between the pleura pulmonalis and the pericardium, which were, however, easily separated. The heart was healthy, and in its proper position. There was no effusion in either cavity of the thorax or pericardium; and the pulmonary and costal pleuræ were smooth and transparent. On turning aside the left lung, for the purpose of ascertaining its connexion with the diaphragm, a laceration accidentally took place, and a pint, at least, of straw-coloured fœtid pus was immediately poured into the left thoracic cavity. The diaphragm, near the laceration, was of a dark colour, very thin, and soft. On further examination, the abscess was found to have been situated immediately to the left of the left lobe of the liver, and behind the stomach. It was bounded above by the diaphragm; in other directions, by the edge of the left lobe of the liver, by the stomach (chiefly by the larger end and posterior surface), by the spleen, left kidney, and pancreas. All these parts were so connected as to enclose the pus in a shut sac. The colon, at the left angle, where it begins to descend, also adhered; and this bowel had contracted firm adhesions to the peritoneum down to the sigmoid flexure. The parietes of the abscess were thick, smooth internally, and of a brown colour. A small portion of lung, not exceeding a square inch, adhered to the diaphragm, and, like that muscle, this portion was of a dark brown colour, and very soft. A considerable bronchial tube communicated with this portion of lung. With the exception of the portion now mentioned, the left lung was throughout as perfectly healthy, crepitating, and pale, as the right lung, and the pleural covering was thin and transparent.

The liver was sound even to the edge of the left lobe, but the peritoneal covering was thickened and opaque. The pancreas was healthy. The cavity of the stomach was small, but the several coats were sound. At the larger end the rugæ were strongly marked, but exhibited neither redness, thickening, softness, or hardness. The spleen was small, and of unusual appearance; it had lost all trace of cellular organization, was denser than liver, of a bright red colour, and resembled muscular fibre; in short, it appeared *carnified*. The mucous membrane of the large intestines was rather thick and red; the ilio-colic valve was sound, and no ulceration was observed.

The cartilage of one of the false ribs on the right side had been broken by the injury; the ends overlapped, and in that position they had united.

[To be continued.]

FURTHER OBSERVATIONS ON THE FUNCTIONS OF THE FIFTH PAIR OF NERVES.

To the Editor of the Medical Gazette.

Sir,

At the time I dispatched to you the paper which you did me the honour to insert in your journal of March 8th, I had not observed the lecture on affections of the fifth pair of nerves, by Sir C. Bell, which appeared on the 15th of February. Since then I have had an opportunity of perusing that lecture, which has given rise to the following observations, and for which, if they appear likely to interest your readers, perhaps you will also find a place.

In the lecture referred to, Sir C. Bell has detailed the history of two cases of disease of the fifth pair; viz., the case of M. A. Webb, and of Windsor. It is not my intention to dispute that these were

cases of disease of the fifth pair; quite the contrary: I wish rather to draw attention to the fact, that in both these instances the muscles of the face participated in the affection. Thus, in the case of the female, in addition to inability to move the food on the affected side of the mouth, and difficulty in speaking, there was, at one period of the disease, a distortion of the countenance. So, in the case of the soldier, as well as the general loss of sensibility, we are told that he was seized with paralysis of the muscles of the left side of his face. This state seems to have continued for some months, but at length the muscles regained their power. The case terminating in death, Sir Charles had an opportunity of observing the cause of the disease, which was found to be a tumor involving the third, fourth, fifth, and sixth nerves of the affected side. "The nerves of the left side, which were not imbedded in the tumor—viz. the seventh, eighth, and ninth pairs — possessed their natural and healthy appearance." In order to explain the paralytic condition of the muscles of the face, whose healthy action is commonly supposed to depend entirely upon the portio dura, Sir Charles imagines that at one time some inflammation must have extended to the seventh pair.

If the portio dura bestow upon these muscles their voluntary power, then it is necessary for such an hypothesis to account for their paralysed state; and yet no evidence could be found of any affection of the seventh pair. Is it not equally reasonable to suppose that here, with a palpable and indisputable affection of the fifth nerve, the paralysis was dependent upon that affection? It is not more unreasonable to imagine that the particular portion of the nerve in communication with the muscles of the face might be less involved in the disease than other portions of it. And this view appears somewhat strengthened by the fact that nothing is mentioned of the taste or sensibility of the tongue having been impaired,—from which circumstance it may be inferred that the portion of the nerve connected with the tongue must likewise have remained free from the disease.

This case is also deserving of attention, inasmuch as it shews that sensation of the side of the face may be lost without a loss of the muscular motions of the same parts,—a matter which has apparently been misunderstood, or at least not properly explained, by more physiologists than one. Thus sensation was first lost, muscular motion was afterwards destroyed, and the latter restored without any return of the former. In Sir C. Bell's experiment of dividing the fifth on the ass, the sensation of the face and lips was destroyed, and the animal was unable to use his lips, because, it was said, the sensibility was lost. Here is a case in which sensation was gone, and yet the muscles retained their voluntary power—a sufficient proof that these qualities are independent of each other. Mr. Mayo seems to have fallen into the same error, because he does not allow the fifth to have anything to do with voluntary motion. After dividing the fifth, he says, "the lips were indeed no longer used in the prehension of food; —a curious phenomenon, but sufficiently explained by the loss of sensation in these parts *.

In my former paper, I alluded to a theory I had suggested of the sensibility to light being derived from the fifth pair of nerves: I appeal to this case of Sir Charles Bell's in confirmation of my views. Here was a case in which the sensibility of the conjunctiva was lost, "so that drawing the finger along the eye gave him no pain;" but "he complained of the uneasiness which the red light of the candle, seen through the eye-lid, gave him." His vision was good as long as the eye remained transparent, and the dissection evinced that the optic nerve was unaffected. Whence, then, this morbid sensibility to light? I answer, from the affection of the fifth nerve. We are told, "while the eye was transparent, if the candle was brought near the patient, he suffered unusual irritation in the eye." Here there is no affection of the retina; vision was good: this irritation could not, therefore, be owing to any other cause than the affection of the fifth. This case goes to prove that sensibility to light is something more than a modification of common sensibility, though both are derived from the same source; and that one may exist when the other is destroyed. In the case appended to my former paper the sensibility to light was gone, vision being also perfect, whilst common sensibility remained.

In the instances mentioned by Sir C. Bell of division of the frontal nerve, ap-

* Outlines of Physiology, p. 238.

parently without paralysis of the muscles of the forehead, I should explain the circumstance as follows:—If the cutaneous branches alone were divided, the operation could not affect the muscles. If the muscular branches were also divided, it then becomes a question, what are the functions of these muscular branches? If we find them to bestow voluntary motion merely upon these muscles, we may still refer to the agency of the portio dura for their motions in expression, a modification (I think Sir Charles says) of respiration. Loss of expression would certainly produce greater deformity than mere loss of voluntary motion. There would have been no deformity from paralysis of the orbicularis oris, after division of the fifth on one side, but for the peculiar circumstance of that muscle being connected with both sides of the face. I cannot see that loss of voluntary motion necessarily implies distortion, excepting in the instance of one half of the orbicularis oris being paralysed; hence I think we may conclude that the voluntary motions of the muscles of the forehead might be paralysed without deformity being a necessary result.

My friend and colleague, Mr. Hunt, has favoured me with his memoranda of a case in which paralysis of the muscles of one side of the face, with distortion of the mouth and difficulty of separating the jaws, was followed by general tetanic symptoms and death, in which no affection of the portio dura could be discovered in a post-mortem examination. A tumor had some time previously been removed by Mr. Hunt from the orbit, suppuration had followed, and the supra trochlearis and frontal branches of the fifth were found involved in the diseased structure. A small tumor was likewise discovered in the dura mater, between the olfactory nerves, with considerable vascularity and effusion between the membranes of the brain, and some disease of the spinal cord.

It would be too much to expect that I had made out these various points to the satisfaction of every one. Feeling, however, a consciousness that there was a good deal of obscurity with regard to the functions of the fifth nerve, and that it had more to do with the motions of the eye and face than is generally allowed, I could not help considering it a subject worthy of discussion.

Apologizing for having occupied so much of your space, and trespassed at such length on the patience of your readers, I have the honour to subscribe myself, sir,

Your very obedient servant,

JOHN WALKER,
Assistant-Surgeon to the Manchester Eye Institution, &c.

March 23, 1834.

FORCEPS FOR THE SAFE EXTRACTION OF STUMPS OF TEETH.

To the Editor of the Medical Gazette.

SIR,

I SHALL offer no apology for sending you the following remarks, as I consider it a duty to make known to the profession and the public any improvement in a branch of surgery at present so little understood, and productive of so much anxiety on the part of the patient; the very name of punching out a tooth being sufficient to deter many from obtaining that relief which extraction alone can often afford.

I need only refer those who are imperfectly acquainted with the subject to the method of proceeding recommended in very difficult cases by the most esteemed author on dental surgery of the present day, viz. Mr. Bell, who, from his high character as a scientific man, and his rank in the profession, may be supposed to have given us the concentrated information of previous authors, combined with his own experience. Without referring, therefore, to any other, I shall briefly quote the passage contained in his work. He says, "It has in a very few instances occurred in my practice, that the root was so far decayed as to render it impossible to reach it by the elevator, applied in the usual manner. In such cases I have successfully adopted the following method of bringing it away:—a crucial incision is made in the gum, as nearly as possible opposite to the apex of the root; the gum is then separated from the bone, so as to expose a very small portion of it, which is to be cut away with the point of a strong knife, till an opening is made into the alveolar cavity, and the end of the root is exposed. By placing the point of the elevator between this and the bottom of the socket, the root may be forced out through the natural opening of the alveolar cavity."

Now, to say the best of it, this is a painful and tedious operation for the patient, and one that I, as a dentist, should be very unwilling to undertake; for I am strongly disposed to consider it as unworthy of the present enlightened state of the surgical art.

All other works that mention the punch or elevator, speak also of the danger and difficulty attending its use.

Having met with several difficult cases, and having always entertained a particular dislike to employ an instrument so unscientific, painful, and dangerous, I was led to consider attentively the difficulties to be overcome, and the most certain and safe means of overcoming them,—feeling quite confident that an instrument might be constructed free from most, if not all, the objections to which the elevator is liable. I mentioned the subject in a conversation with Mr. Lemale, a very intelligent mechanical dentist, who immediately produced a small instrument, somewhat similar in principle, but differing in certain essential points, from the one I have contemplated making; with this, therefore, and suggesting certain improvements to the instrument-maker, I have produced a forceps not only free from all the objections of the elevator, but possessing other very desirable qualities. I may venture to assert, that it is certain of removing any stump without the chance of breaking it, and certain of doing no mischief to the jawbone; nor can it possibly slip and pass through the cheek or other soft parts (all which accidents may occur with the instruments hitherto in use, and the tooth be still left in the socket); added to which, it gives very little pain in its application; lances the gum and removes the tooth nearly at the same moment; requires very little time for its use; does not obstruct the sight of the operator by the bleeding which takes place in ordinary lancing of the gum; and, lastly, is perfectly harmless in the hands of even the ignorant and inexperienced. It is, moreover, very simple in its construction. I can particularly recommend such an instrument to the notice of the profession, and to country practitioners especially, as it renders the breaking of a tooth much less to be dreaded when such simple means of removing the stump are at hand.

Its use is applicable to all the incisors, canine, and bicuspid teeth, when either decayed or broken, and to the molars when the crowns are so far gone as to leave the roots separated, thereby rendering their removal by the elevator more difficult. Teeth, also, that grow irregularly, and are enclosed between, or placed behind, others, may be often removed more safely with these forceps, when no other will easily reach them. It is necessary to have two of them for use; one for the upper teeth, made straight, the other for the lower, bent at almost a right angle. I had wished to have accompanied my description with a drawing, but it would scarcely give a more correct idea of its form than can be done in words.

The straight instrument, then, is nothing more than a pair of forceps, about the length of the common pairs (five or six inches), with the blades hollowed out and smoothed, similar to a speculum auris, so as to slip easily by the side of the root, and converging towards the points, so as when closed to leave a conical space corresponding to the shape of the root, the edges at the points being perfectly sharp, like a knife. The manner, therefore, of using it is simply to insert their sharp edges between the root and its socket, keeping as close to the tooth as possible, and gently to insinuate the blades down on each side of it: however deep the stump, this is easily done, and my patients tell me with very little pain indeed. When deep enough to feel a secure hold, a slight rotation should be made, and the stump being loosened, is removed with the greatest ease and certainty.

The advantages of this over any other instrument that I have seen, or believe to be in use, are so palpable, that nothing more need be urged in favour of it: it is scientific in its principle of action, which certainly the punch is not. The cases which I have now treated with these forceps have been so completely successful, even beyond my most sanguine expectations, that I speak with confidence, and should feel the greatest pleasure in receiving a visit from any medical gentleman who would wish proof of its superiority over the old instrument; for which purpose I would beg them to select the most difficult cases they can meet with, (for the more difficult, the more satisfactory the result). My object is the simplifying, and rendering very much less painful, that much-dreaded operation, the ex-

traction of stumps; to discard from use entirely, if possible, that unscientific instrument, the punch; and to substitute for it one that I look upon as a very valuable improvement in our branch of surgery. Whether or not I have succeeded, must be left to the consideration of all who are anxious to adopt, without prejudice, the safest and best means of alleviating the sufferings of their fellow-creatures.

An early insertion of these remarks will greatly oblige,

Your obedient servant,

J. Glasford Shepherd,
Dentist; Member of the Royal College of Surgeons.

53, Baker-street, Portman-square,
April 8, 1834.

SKETCHES OF THE PRESENT STATE AND FEELINGS

OF THE

MEDICAL PROFESSION IN THE PROVINCES;

And more especially in Liverpool and its vicinity.

By James Collins, M.D.

I have already detailed some of the peculiarities that distinguish us from other places: our system of dunning, collectors, and bills, and the low and degrading charges of our medical men—the first physicians attending for seven shillings a visit, and often a great deal less; and the surgeons in the same rank for from one to five;—how we are thus all reduced to the rank and condition of tradesmen, without their protection from the law, being compelled like them to send in our bills, without having any legal right for their payment;—and how, in consequence thereof, people in easy circumstances are every day availing themselves of our time and attendance without paying for them;—in short, that there is not a poorer or more depressed class in the community, than the medical men of this neighbourhood; and that, unless something be done, they must necessarily become more and more so, until they tumble to the lowest grade in society. It is true, their own practices have contributed a great deal to produce this state of things; but the anomalies and defects of the law have contributed a great deal more.

I shall now endeavour to convey some ideas of the state and practices of the chemists and druggists; of our clubs and club-doctors, and their mode of payment; of the management and exclusive system of our public institutions, and of the ways and means devised to make them tell for the benefit of those attached to them; and of the jealousies and bad feelings engendered thereby. The subject is one of some interest, and admits of considerable extension; but I shall endeavour to condense it, without impairing a full view of its workings and effects; as it is only by doing so that strangers can judge of us, and see what they share in common with us; and how that which is good may be retained, and that which is bad may be got rid of.

Every person here who weighs or prescribes a dose of medicine is usually called "a doctor;" chemists and druggists receive the appellation as well as others. Colleges may make the distinctions, but the people are every day getting rid of them; and it is not easy to change their customs or language with pieces of parchment. They merge into one general appellation of "doctor," all grades and distinctions; chemists, druggists, apothecaries, and physicians, are mingled and confounded together; all discrimination in this respect is passing rapidly from their minds; nor, perhaps, would it be a matter of much importance, if it did not tend to destroy all distinction between the educated and uneducated, and thus give the sanction of public opinion to unlicensed and unqualified practitioners. There is no class which gains more by confusion than the chemists and druggists. The law requires from them no education, no apprenticeship, no previous or subsequent examination; it allows any one, male or female, to follow this business without check or control; and, in point of fact, many who are now in it commenced without any knowledge of it; they never served a day to it. Some of them were grocers, ironmongers, carpenters, chandlers, or in various other branches of trade or industry, and not succeeding, turned all at once chemists and druggists, without further ceremony or preparation. They usually get some runaway apprentice, or some person who knows more or less of the business, to conduct it until they pick up as much as they think sufficient to do it themselves: then they take behind the coun-

ter, young, raw, uneducated lads, some of them not twelve years of age, as apprentices, to whom the compounding and dispensing department is often intrusted. One is astonished at their youth, ignorance, and inexperience, and the risks the public run; and yet the law sanctions the system.

There are several highly-respectable and well-educated men here in this business, but they are comparatively few; and the public have not always the means to discriminate them from others. The whole system is one of abuse and inconsistency, and calls loudly for reform. Very few confine themselves to their regular business, the compounding and making up of medicine; if they did, they could scarcely live by it, their numbers and competition are so great. Hence they unite other branches with it; they sell all sorts of stuff for culinary and domestic purposes—brick-dust, rubbing-stones, pickles, cigars, &c. The same scale and hands that have to weigh and make up the delicate, and often dangerous ingredients of a prescription, have, perhaps, at the same time, to prepare the blacking or varnish compounds of the next door. Many of their shops have more the appearance of hucksters than of chemists and druggists. All this of itself would be bad enough, if the evil stopped here; but it does not. They are now the regular physicians and surgeons of the public; they prescribe for all sorts of diseases, and have almost ruined the general practitioner; they are becoming every day so familiar in this respect to the public, that the first merchants, as well as the lowest tradesman or mechanic, are occasionally their patients. They often retire with large fortunes, and are the only branch of the profession that do well here; most of the others struggling through life, and dying in poverty or debt. They visit, like the regular practitioners, their patients at home and abroad; and the people like their open shops, their cheap charges, and mode of doing business. They crowd into them for something that is good for a cough, a pain, or an ache; that can cure a broken leg, or straighten a crooked one; for they think that there is a specific in them for every thing; and thus they keep coming and going, and paying as they go along, until they pay in trifles often much more than they would have to pay the first medical man in the town. Hence the surgeries are empty, whilst their shops are full. No matter for what medicine the people come; be it dangerous or not, they get it. They can buy more or less of laudanum, or of stuff, as they term it, to kill the rats, and which they too often apply to more criminal purposes.

On the Continent no medicine is given of a deadly character without a physician's or surgeon's prescription, and the address and name of the party prescribing. Here there is no such check or precaution. Can any thing be more absurd or ruinous than this state of the chemists and druggists? It takes the bread from the regular practitioner, and gives it to the quack and pretender. It allows a numerous and enterprising class, of popular manners and flexible habits, to treat all sorts of diseases, to be the retailers of all sorts of articles, the compounders of family and medical prescriptions—in short, *factotums* in medicine, without any test of their fitness or capacity. Can any thing be more unjust towards the public or the profession? or who can tell the lives thus sacrificed, or the pains and aggravation of sickness thus caused, by this wholesale license of quackery and empiricism? The coroners may answer, that the law is silent. What is the use of the trouble and expense of a regular and protracted education, if, when one settles in practice, he finds no protection against the inroads of those who never attended a lecture or an hospital? What is the use of degrees to him, if the public make no distinction between him and them? And yet such is the state of the profession and the law, notwithstanding all the charters and privileges of our medical corporations, given ostensibly to protect us! How well they deserve the thanks and support of the profession!

Club Doctors, and their effects on the profession.

The next subject to which I wish to direct the attention of the profession, is to the peculiar character of the clubs that exist here; they are merely sick and provident societies, quite distinct from what are termed the trades' unions; the latter being political associations, for mutual aid and protection against what they deem the aggressions and tyranny of their employers. They form, by their connexion with medicine, a very remarkable feature in the profession here,

and have, more than any thing I know, laid the foundation of the most degrading and unprofessional practices. The medical men are so poor, and their immediate wants so pressing, that they grasp at any thing that seems to proffer a prospect of relief or practice. Hence their encouragement of these clubs, which extend here through almost all the middle and lower classes of the community. Every club has its surgeon, or, as they term him, their "doctor:" the cheapest is generally preferred, except where peculiar interest is made to supersede economy: there is no trade or branch of industry without them: mechanics, whether journeymen or masters, shop-keepers, retired tradesmen in easy and comfortable circumstances, men and women, have all their separate and respective clubs and club-doctors. The mode of election is by tender and ballot: each candidate sends in his terms and claims on their suffrages: he states his country, his religion, and every thing else that may tell in his favour, or tickle their fancy; for almost all the clubs have their peculiar prejudices or prepossessions on these subjects. On an average, there are, perhaps, six or seven candidates for a vacancy. All are obliged to use the tactics of contested elections, and to curry favour by all sorts of blandishments: they treat the electors to ale and porter; they eat and drink with them in low pot-houses, and are, in every sense, "hail fellows well met" with them; and these practices they continue, more or less, after their election. Hence, gross and low habits and company are engendered and propagated, and the better and more refined feelings of gentlemen blunted and destroyed. Others, again, gain their elections by religious cant and humbug: they come recommended as class leaders, pious and edifying christians, and often become, as a matter of course, the club-doctors of peculiar sects and denominations, and then officiate often in the double capacity of preacher and surgeon. Perhaps there are not four surgeons in the town who did not, at one time or other, belong to these clubs; they are the anchor by which they attempt to keep from drifting, when they commence practice or settle here.

Women, also, have their clubs and club-doctors; they generally consist of dress-makers, hucksters, small shop-keepers, the wives or widows of seafaring people, and others. Like the men, they, too, have their monthly, quarterly, and annual meetings, and are governed by a *chairman*, secretary, and committee, elected annually from among themselves. They admit no one in the shape of man, but the doctor, who often acts as chairman, secretary, and factotum. Surgeons are very active and anxious to get an appointment among them: they deem them the best and most productive. These clubs prefer, more than any other, a religious man for their doctor, or at least one who can wear well its mask: they think, for decency and propriety, they ought and are bound to do so; though they generally are better customers to the public-house where they meet than the men, and know how to spend and enjoy themselves as well, if not better, and are much oftener the subject of scandal and talk, notwithstanding their godly habits and God-fearing doctor. I cannot state with accuracy the number that belong to them; but I presume there are at least a thousand women enrolled in these associations here. The number of men is probably not short of fifteen thousand; and the number of clubs, including both male and female ones, must exceed sixty or seventy.

From all that has been said, it might be presumed that these clubs are good things for the doctors, and that they are well paid for the trouble and degradation to which they are obliged to submit. It is not so, but the very reverse: they would be far better paid, and have more to do, if those clubs never existed, or could be suppressed. The medical men who attend them are paid, on an average, about two shillings a-head every year, for every member in the club. Thus a club of one hundred members is worth about ten pounds a-year to the club doctor: for this he is obliged to supply medicine and advice, and to attend under all circumstances, when called or required. He is liable, on all occasions, for apparent or real negligence, to be arraigned, or, as they term it, *shown up*, before the managing committee, and then obliged satisfactorily to exculpate himself, or be dismissed: no matter however trifling may be the ailment, or late the hour, or great the distance, if summoned he is obliged to attend; and is often met, on his arrival, with a volley of abuse and low epithets for not being quicker or more attentive:

in fact, he is subject to all sorts of insults and reproaches. None but they who have experience of it can appreciate the dog's life he leads in this respect. In consequence of all this, and the vigilance required to detect those scheming to be put or kept on the sick list, or, as they term it, "the box," his professional visits are more frequent and troublesome, and infinitely more disagreeable, than on the same number of patients in ordinary practice: and what is his remuneration for all this? Just about three-halfpence a visit for advice and medicine. There are at least three patients to be visited, on an average, every day, for every hundred members in the club; and for this he only gets ten pounds a year, subject to all the drawbacks alluded to above. He pays about 1095 visits a-year, which, at two-pence a visit, would amount to *9l. 2s. 6d.*; calculating the expense and trouble of making up the medicine, they do not receive three-halfpence a visit, at the rate of ten pounds a year, for every hundred members of the club. The paupers cost the public charities much more: but the club patients are far from being paupers; they consist of that class which pays medical men the best, if things were not so turned upside down: they consist of the middle and industrious classes,—some of them worth thousands of pounds, and most of them in easy and comfortable circumstances, all well able to pay their medical man; and yet, by this system of contracting, they get attended for three-halfpence a visit. Thus the profession is ruined; the materials by which it ought to live are taken from it, and destroyed. Thus, sixteen or seventeen thousand persons, almost all the heads of families, in easy, if not affluent circumstances, get their medicine and medical attendance for about three-halfpence a visit. What is this but the principle and scale of the self-supporting dispensaries applied to the better and upper classes? But the evil does not stop here, for the club doctors ("and they include 19-20ths of the practising surgeons in Liverpool") make also a contract for attending the families. The wives they deliver, and attend for one month after delivery, for about ten shillings; the other items in proportion. Now, when one considers that from sixty to seventy thousand patients are relieved for nothing at our dispensaries and public charities, and many of them in easy and comfortable circumstances, and by no means worthy of these charities,—when, I say, one considers this, and that sixteen or seventeen thousand of those who ought to be the best and most paying class, are attended for almost next to nothing—three half-pence—we cannot be at a loss to comprehend how the chances and emoluments of practice have declined, and the causes of the general poverty and degradation of the profession.

I view the subject, not as a political economist, with reference to its effects upon the community, but with respect to its debasing and impoverishing result upon what was once an honourable and high-minded profession; but, alas! how changed and fallen!

We may talk, Mr. Editor, as we please, about medical reform; and get up meetings and petitions, and descant day after day on the anomalies and defects of the law; but unless we set about reforming ourselves, and discountenancing the vile and unprofessional practices that have crept in amongst us, and thereby testifying in private the sincerity of our principles in public, it is in vain to expect from acts of parliament the good we anticipate, or that they can reach abuses far beyond their province or control. I rejoice as much as any man at the appointment of the Parliamentary Committee now investigating the state of the profession; but there are many things in our local habits and practices that necessarily cannot come before them; or if they did, that they could neither remedy nor remove. No; the reform for these things must spring from ourselves: it must be the result of a better tone and more elevated feeling than exist at present,—of the union and co-operation of medical men for mutual interest and protection; in fact, of a system of *trades' union*, organized and directed by the heads of the profession. But, thank God! if we have many vices, we have some virtues. Amidst all the political corruptions of this borough, only one medical man sold his vote. This, perhaps, in other circumstances would scarcely be worth mentioning, were it not for the general corruption that prevailed, and for the feelings of self-respect that medical men must necessarily feel in having one solitary instance among them.

Liverpool, April 1834.

CASE OF FISHER AT ST. GEORGE'S HOSPITAL.

To the Editor of the Medical Gazette.

SIR,

IN the last number of the Medical Gazette my name is introduced in connexion with a case of stone, which was operated for in St. George's Hospital by Mr. Hawkins. With what occurred in the hospital I have no concern; but it is important for me to correct the impression which even this incidental mention is calculated to produce. It is stated that I operated by lithotrity *nine times;* and that subsequently the patient Fisher was received into St. George's. This is an error. I find from my notes that I used my instrument five times; and perhaps there was one sitting which had not been set down; this added, will make six. The inference from the article in your journal is, that Fisher, in applying to St. George's, had lost confidence in the efficacy of lithotrity, or that he had been tired out by the number of applications of the instrument; this, however, was the very reverse of fact, as you will learn from what follows.

I was applied to by Sir John Chapman, on behalf of Fisher, who then was, and had been for two years, unable to work as a labourer and thresher. I operated on him in presence of between sixty and seventy practitioners of the neighbourhood, in the Town-hall of Windsor. It was ascertained that the bladder contained several calculi, of the size of a pigeon's egg. The second operation took place also at Windsor; the subsequent applications were made at considerable intervals, in Egham, at the houses of Mr. Furnival, and Mr. Gilbertson, surgeons. Fisher resumed his work as a thresher after the second sitting; and to every sitting afterwards he came from his work, and returned to it—a distance of between three and four miles. His general health was restored, and his sufferings had ceased almost altogether, when I last saw him. After the last sitting, I told Mr. John Furnival, that if Fisher did not void all that remained of the broken-down calculi, I should be ready to come down, on his dropping me a line, at any time. I was never applied to afterwards on account of the poor man, and I concluded he was cured.

I learned from Fisher's wife, some time ago, that when he began to get bad again he went into Egham, where he was told that I had left England, and was living in France. He often said to her, if he could see me once more he should be all right. His sufferings increased, and he was persuaded to go to St. George's Hospital, on the assurance that the surgeon of the hospital would employ *the same means that I had used.*

This is a statement of facts which cannot be controverted; and I believe a simple appeal to these facts will not inflict any injury on my professional reputation. On the conduct of other surgeons it is not my desire nor object to comment.

I have the honour to be, sir,
Your obedient servant,
WM. B. COSTELLO.

38, Bruton-Street, Berkeley-Square,
[April 17, 1834.]

[From the preceding letter, it appears that Mr. Costello and Fisher are at issue as to the number of times the operation of lithotrity was performed: the latter, when in St. George's, called it *nine;* the former says it amounted only to *six.* As Mr. Costello seems to regard this difference as of importance, we readily give insertion to his letter, *although it has nothing whatever to do with the question at issue.* The piece of information, that "*his sufferings having increased,*" the man was persuaded to go into St. George's Hospital, on the assurance that the surgeon would employ "the same means" as Mr. Costello had done, is curious, and was probably the origin of the falschood in the Lancet, that Mr. Hawkins had actually attempted to use some new instrument experimentally. Mr. Costello hopes that no injury will be done to his "professional character"—we hope so too; and beg to remind him, that we never alluded to his character professionally or otherwise—we did but express our contempt of the knave who fabricated the hoax which appeared in the Lancet, and was made the basis of an attack upon "the professional character" of another.—ED. GAZ.]

ANALYSES AND NOTICES OF BOOKS.

" L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

The Physiology, Pathology, and Treatment of Asphyxia; including Suspended Animation in New-born Children—and from Drowning—Hanging, &c. By J. P. KAY, M.D.

THERE are few subjects which it would be more desirable that the community at large should be acquainted with, than those treated in the present volume. It was the intention of the author, as we learn from his prefatory letter, addressed to the Royal Humane Society, to supply this desideratum, and to produce a work which should be suited for general use. But the difficulty, which is almost proverbial, of composing books for the people on subjects connected with medical science, seems to have stood in our author's way, as it has in that of many a man who has gone before him; and we scarcely think he has done much to remove it. The very title which he has adopted—the Physiology and Pathology of Asphyxia—is surely a stumbling-block for the uninitiated: Suspended Animation, and its Treatment, might probably have answered his purpose better; though we question if even this would not have seemed too learned for the multitude.

With the exception of this fault, which we fear may operate in depriving the work of much of its destined usefulness, Dr. Kay has acquitted himself with great ability: he shows a very extensive knowledge of his subject. We have here the results of several years' close research and experimental inquiry: and the volume may be considered as a summary of the author's matured opinions regarding several points of deep interest and importance which he has on former occasions discussed.

The various theories which have been broached in explanation of the phenomena of asphyxia, are very well stated in the introductory portion of the work. Bichât's views and experiments are fairly given; and his hypothesis of death being produced by the deleterious agency of the unarterialized blood upon the organs which require the accustomed stimulus of arterial blood, is candidly put forth: while it is satisfactorily shewn, by inferences from experiments ingeniously devised by our author, that not the depraved quality, but the deficient quantity, of the blood with which the left cavities of the heart are supplied, on a deprivation of the powers of respiration, is the chief cause of death. As we are unwilling by any possibility to misstate Dr. Kay's opinions on this matter, we shall present our readers with the author's words:—

" Death occurs in asphyxia because the air, not being admitted into the lungs, the changes which are necessary to the transmission of the blood through their structure no longer ensue, and the circulation gradually becomes fuller, and is at length arrested in the capillaries of the pulmonary tissue. Before this result takes place, the blood by degrees assumes more and more of the venous character, until its qualities become incompatible with the activity of the minute vessels of the lungs. A certain quantity of dark blood, not so changed as to be inconsistent with vital action, is transmitted by the heart and arteries to all the organs of the body, and circulated through the nervous system and muscles; but it is found that this fluid, far from destroying the vitality of these parts, maintains the life of the muscles for a certain time, though comparatively in a feebler manner; and it is also discovered, that, though it is deficient in stimulus to the nervous system, and is not capable of supporting the greatest activity of the nervous power, it has no positively noxious power upon it. The cause of death in asphyxia is the arrest of the circulation; but during the supply of the darker blood to the organs (as it is deficient in stimulating and nutritious qualities), and the rapid diminution of even the supply of this fluid, the functions of every part are enfeebled, and sense and voluntary motion extinguished, before the circulation has actually ceased."

Certainly, if Bichât's theory were true, there could be little hope entertained at any time of recovering persons in a state of asphyxia, which, according to that illustrious pathologist, should be a state of death by poisoning, with what he held to be a most deleterious substance—the blue unarterialized blood. But Dr. Kay has not only established a more correct pathology on this vitally important subject, but has shewn that

reasonable expectations may be entertained, in most cases of asphyxia, of recovery being attainable by the adoption of proper methods. His book abounds with valuable suggestions regarding those methods; and we cannot but recommend it strongly to the attention of practitioners. We perused with much interest the remarks on death by cold, and from respiring noxious gases; and we think the observations on the caution to be taken in attempting to inflate the lungs of drowned persons, particularly worthy of being generally known.

MEDICAL GAZETTE.

Saturday, April 19, 1834.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

PAROCHIAL REGISTRATION,

WITH REFERENCE TO MEDICAL STATISTICS.

THE returns of the Worshipful Company of Parish Clerks for the last year, have only recently been published; and by them it appears that the total of *registered* burials for the year commencing Dec. 1832, and ending Dec. 1833, amounts to 26,577. Of those *unregistered* we have no returns: but of this more anon. For the corresponding period of 1831-32, including the cholera season, the number was 28,606; so that there has been a decrease of 2,029; and if we were to compare the number of the last year's burials with that of the population within the Bills—or of the parishes which are presumed to give returns—we should seem to fix the rate of mortality of the metropolis at one in forty-four, while for the preceding year it was one in forty-one.

But we must take care how we draw these hasty inferences, however they may seem to coincide with statistical statements which have been given to the public; for nothing can be more fallacious than such data as our Bills of Mortality afford, if used for the purpose of arriving at conclusions in medical or general statistics; and foreigners, if made acquainted with the mode in which the returns of births and deaths in this metropolis are procured, would, we apprehend, scout all our pretensions to superior salubrity;—our assertion, for example, that the mortality of London is but one in forty, or forty-two, or even forty-four—a rate to which no other capital in Europe, or in the world, can pretend. This very fact itself were surely enough to excite suspicion regarding the accuracy of our statements; for there can be no such great contrast in the modes or habits of life in London, compared with those of other European capitals, as to warrant such a superior rate of salubrity; and when the *savans* of other countries come to investigate the grounds on which we arrive at our self-satisfactory conclusion, it is to be feared we have none very convincing to afford them.

We yield to none in a desire to uphold the superiority of our capital in point of salubrity: we are fully aware of the flattering reflection which it must more or less throw on the medical art, if it can be made out—though there are many other elements, undoubtedly, to be taken into consideration, to account satisfactorily for such a result; but we cannot help expressing our fears that the degree of mortality which prevails in London is greatly underrated by those who state it at *one in forty*, on the credit of the Bills. We are aware that it has been supposed to be otherwise established by such data as are afforded by the lists of tontine nominees, of life annuitants, or of insured lives—sources which, so far as they go, are much less liable to error than the returns to which we at present more particularly refer; but we doubt if the rate

in question have not been chiefly gathered from those clumsy averages which, in default of other simple data, and from mercantile habit, we are but too prone to be content with. We are by no means disposed to undervalue the Bills of Mortality; for, under the circumstances in which they are constructed, they are, perhaps, rather to be esteemed for their steadiness, than rejected for their deficiencies. "The agreement of the Bills with each other," says Dr. Heberden, "does alone carry with it a strong proof that the numbers under the several articles are by no means set down at random, but must be taken from the uniform operation of some permanent cause." There are great deficiencies, however, to act as drawbacks on the value of the Bills. We know, it is true, the population of the vast district—consisting of above a million, one hundred, and eighty souls—to which they are supposed to apply; but some of the parishes in this vast circuit give no returns, and many give them irregularly. Nay, this is far from being the worst that the system has to contend with. The births and deaths which are entered in the parish registers are almost exclusively those of persons having Church of England connexions; neither the Jews, Quakers, or Dissenters of any denomination, are included in them; and we all know how the latter class especially has been increasing in numbers and importance of late years. We have said births and deaths—we should say baptisms and burials—a still more unaccommodating circumstance; for with regard to the former, though Dissenters might be satisfied to enrol the *fact* of birth in the parish register—the only direct evidence allowed on many questions in courts of law—none of them can be presumed willing to submit their children to the baptism of the established church, for the sake of securing any such civil right; and even if they did, in the mode in which things are at present managed, it is only left to be *inferred* that every child which is baptized has been born *some time* previously, although the exact date is of so much importance for statistical purposes. So also with respect to burials: though there is certainly more chance of Dissenters being *buried* in the cemeteries of the parish churches, yet the fact of death, and its exact date, is only left to be inferred, in like manner, from the entry of the burial.

It will at once be perceived how grossly deficient for all these reasons are our parish registers, as sources of the necessary data for the statistics of the metropolis. But may we not, by some approximation to the number of deaths annually among the Dissenters, who constitute the great mass of the unregistered, arrive at a conclusion as near the truth as the circumstances of the case will admit? In the year 1811, when the number of burials registered in the bills was about 20,000, it was calculated in the Population Abstract of that date, that the number of those not registered in the parochial returns was not less than 7000; that is to say, above one-third. Since that time, we may safely affirm that the proportion of Dissenters in the metropolis has not diminished: we are accordingly, we presume, fully justified in increasing the yearly returns of the bills by at least one-third of the total. Now this gives us for the number of deaths, or burials, in a population of 1,180,502 (that within the parochial returns), 35,436; that is to say, *one in thirty-three* (33·3) for the mortality.

We are thus brought, with respect to the salubrity of our capital, somewhat more nearly on a par with Berlin*, Paris†, and other well-regulated cities of

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* The mortality of Berlin in 1832 (when the cholera had nearly disappeared) was 1 in 31·7; or, allowing for the deaths by cholera, 1 in 33·2.

† The mortality of Paris in 1830 was 1 in 32·4; in 1831, one in 29·7; and in 1832, when the cholera carried away about 19,000 of the inhabitants, it mounted up to 1 in 17·4.

the continent: and that in thus raising it from one-fortieth to one-thirty-third, we have not transgressed the limits of probability, we shall offer an additional proof, from documents which may be readily attested.

We have before us the "General Statement of Baptisms and Burials for the Parish of St. Luke, Chelsea," for the last two years. This, by the way, is one of the five large parishes whose returns are not included in the Bills of the Metropolis. Its population, according to the late census, is 32,371; and its registered mortality amounted last year to 997. What ought we to add for the unregistered? for it is expressly mentioned in the document that the burials that take place in Chelsea Hospital, or the Jews' or Moravians' burial-grounds, are never entered in the registers of the parish church. Suppose we add only a *fourth*, it fixes the mortality at one in twenty-six nearly (25·9); if we only add a *tenth*, still it is more than one in thirty (29·5). Yet we have no reason to consider Chelsea an insalubrious, or in any respect an ill-regulated parish. What, then, becomes of the supposed one-fortieth or one-forty-fourth, when applied in this instance? Facts, truly, are stubborn things; and those just quoted may serve to warn us how we draw hasty inferences from our partial parochial registers.

It may perhaps be said that our Bills of Mortality were originally intended only to apprise all whom it concerned of the prevalence or decrease of certain diseases in the metropolis—the plague and other pestilences more especially; but who can deny that in setting down all who were buried in the parochial cemeteries, dying of whatever disease they might, it was intended thus to supply a weekly return of the amount of mortality in the metropolis? And we only find fault with them for not doing what they ought to do. The Government recognized the Company which originally undertook to provide those registers (The Fraternity of St. Nicholas); chartered it; and gave it certain privileges to encourage it to proceed: but for above a hundred years—notwithstanding the advances made in every branch of knowledge subservient to statistical science—no improvement has taken place in the Bills. It is true we come at results *per saltum*, through the census which, since the commencement of the present century, takes place every ten years: but why should we not have such means of satisfactorily noting the progress of the population during the whole of the decennial period, as that the census should serve for a comparison or a confirmation of results already arrived at? This is the course pursued in almost every civilized nation of Europe except our own; and until something of the sort is done among us, it is surely frivolous to think of cultivating statistics, medical or general, here as a science: and not a little premature, we should think, to boast of the fact of *our superior salubrity*, founded on *our statistical information*.

A legislative measure, we believe, is likely soon to pass into a law, for admitting as evidence the registers of Dissenters, respecting births, marriages, and deaths. Such a measure, no doubt, is very desirable; although we cannot help thinking it may probably prove only another sample of the patch-work legislation for which this country is already but too famous. How much more desirable would it not be to have one legalized system of registration for both Dissenters and Churchmen, and that the entries of births, marriages, and deaths, should be secured by positive injunction, and not left in its present anomalous state. Until something of this sort is done, we can expect at best only a jumble in our manner of managing matters of the utmost conse-

quence. *We*, however, for obvious reasons, can only allude to the importance of such a legalized system as a means of arriving at a correct arrangement of medical statistics, and as leading to the adoption of well-organized plans of medical police. With the deeply-interesting bearings of the question, with reference to the expediting of litigations which constantly arise about property, and the tracing of pedigrees, and in which a more accurate parochial registration would be of such essential benefit, we cannot of course meddle. It is, however, but too obvious, that for all these purposes our present mode of proceeding is positively the worst in Europe: it is fraught with absurdities, imperfections, and inconveniences: it belongs to a very rude age, and to one in which, if diversity of religious opinion existed at all, all toleration for its exercise must have been set at nought.

We have inquired with some curiosity into the mode in which these things are managed in France; and it is with no small regret we are obliged to admit the humiliating comparison which must be drawn between the institutions of our Gallic neighbours in this respect, and those of our country. There, the *Code* lays down the general formalities to be observed for recording births, marriages, and deaths; and an efficient class of municipal officers is provided, which has the whole under its immediate superintendence. The clergy are relieved of the duties of parochial registration, for reasons most of which would be nearly equally applicable here; and the officers of the *Etat civil*, the *Maires* of communes, are the parties to whom those duties are intrusted. The accuracy and regularity of the register-books are guarded by minute regulations, which are particularly worth noticing. They are kept *in duplicate*, and closed at the end of each year with such care as to prevent the possibility of interpolation. *Indexes* are formed immediately on the closing of the register, and one copy of the register and index is sent to the office of the *Tribunal de premiere instance* of the district, where it is placed under the superintendence of the *Procureur du Roi*; the other, with its index, remains in the *archives* of the commune. Every ten years another complete alphabetical index is made; and the extreme accuracy of all these operations furnishes the means of forming the most perfect statistical documents for every purpose for which a government can wish to concern itself with the state of population.

Births in France must be registered within three days of their occurring; and in cases of Death it is the duty of the public officer to attend on the spot, and receive the legal declaration of the fact; and without his certificate of this duty having been duly performed, no burial can take place. This visit, of course, affords a proper opportunity for ascertaining the *cause of death*, not only for purposes of statistical inquiry, but with a view to the investigation of cases of violent death. In such cases the officer, after availing himself of medical opinion, transmits information of the circumstances to the *Procureur du Roi*.

It surely is high time that we had, as almost every country in Europe has, some officer of the description just mentioned, who should have a permanent establishment in all conveniently-sized districts, to whose acts and certificates, under an official seal, for defined purposes, faith should be given in all courts,—who should have all notarial functions, and at whose office all municipal business should be transacted, and its documents deposited. Within the scope of such an officer the registration of births, deaths, and marriages, would in a special manner come. With regard to deaths, for example, the registration might (as in France) be made necessary

to be certified *previously* to burial, and the entries might be made on *declarations* delivered in a suitable form. The present institution of "searchers" would easily combine with regulations of this sort, so that each would become more effectual: and many minor improvements, no doubt, would suggest themselves, if once the subject were fairly taken into consideration. As to the propriety of such a reform as that proposed, we presume there cannot be a reasonable question raised,—especially when we seriously think of the miserable mode in which the parochial registration of the metropolis is at present managed; and, on the other hand, the perfect success which has attended the working of the improved system in France.

CLINICAL OBSERVATIONS

ON

FRACTURES OF THE NECK OF THE FEMUR.

By Baron Dupuytren.

From the "Leçons Orales," published periodically, under the Baron's inspection.

Age at which such fractures occur.

Fractures of the neck of the femur have attracted so much attention of late years, that we are told in most *ex professo* treatises on such subjects, that they are as well understood as those of other parts of the thigh-bone. Those, however, who have witnessed the practice of the Hôtel Dieu during the past winter, must be aware that much additional light has lately been thrown upon the subject. If you examine, said M. Dupuytren, the age of different individuals who are at this moment in our wards with fractures of this part, you will perceive that they are all past 50 years of age; and among those to be hereafter pointed out to you, you will remark that there are no children, and in general very few adults. But in both sexes you will find this accident more common after 60 years of age. Never in any instance were the predisposing and exciting causes more conspicuous than here. I have never seen fracture of the neck of the thigh in an infant, and very rarely in a young person. Sabatier, however, in his interesting memoir in the transactions of the old Academy of Medicine, mentions the case of a youth of 15, who had a fracture of this kind. These lesions, on the contrary, become more frequent as life advances, and are most common between 70 and 80 years of age. It is impossible but that there must be some cause for this difference in the period of fracture of the neck of the femur at different epochs of life.

Predisposing causes.—This cause is well ascertained, and resides in the anatomical disposition of the parts, which is not the same at different ages. The neck of the femur, in fact, has not at all periods of life the same direction; and this disposition of the parts is important to be known. In early life the axis of the neck approaches to that of the body of the femur; the angle which it forms being the most open possible. The great trochanter makes but a very small projection; and we shall see by and by that falls upon this part are the most frequent cause of fracture of the neck of the femur;—that the frequency of the accident is in direct proportion to the extent to which the trochanter projects;—and that such projection is in direct proportion to the length of the neck of the bone, and the greater or smaller angle which it forms with the bone. Now we know that the trochanter lies deep in children, and that it hides itself, so to speak, behind the prominence of the os coxæ; the result of this is, that in falls on the side the blow does not come upon it, and consequently the risk of the fracture is diminished. Another anatomical disposition renders fracture of the neck still more difficult. The shorter the bone is, the less of a right angle it makes with the body, and consequently the more it approaches the direction of the axis of the femur, and the causes of fracture have therefore less influence on the neck. There is also another reason why fractures of the neck of the femur are rare at an early age—namely, the great flexibility of the bony tissue, in consequence of the abundance of organic matter in the bone. In adult age, fracture of the neck of the femur, but less so than in infancy, the earthy matters are more abundant than at an earlier age, but less so than they subsequently become. The neck also presents a different disposition: it is larger, and the angle which it forms with the body is much more marked than in the child. There results from this more projection of the great trochanter, and consequently more purchase for the causes tending to produce fracture, to act upon. This greater length of the neck of the femur, however, and this projection of the trochanter, vary further according to sex and individual peculiarities. Women have the neck of the femur longer, and consequently a more projecting trochanter, than men. The nearer, in the male sex,

the formation approaches to that proper to the female, the more risk is there in this fracture.

If the relief of the muscles of the haunch in men presents an obstacle to the production of fracture, by diminishing the effect of falls upon the great trochanter, the thickness of the layer of fat in women fulfils the same indication: but when there is much emaciation present, women become more liable than men to the accident. Let us next inquire how it happens that old men are so much exposed to this kind of fracture. At this period of life the pelvis has acquired all its breadth: the trochanter major is more projecting; the neck of the femur longer, and inclined to a right angle. Besides, the skeleton of the old man weighs much less than that of a young adult, which depends upon the bones having lost much of their organic substance,—having a greater quantity of earthy matters, and being thus also more friable. In aged women, while the anatomical disposition retains the peculiarities formerly mentioned, the friability is even more marked than in men. Accordingly, at the Salpetrière, which is an asylum for old women, there are more cases of this nature than at the Bicêtre, which is a receptacle for old men. These considerations are of importance in reference to the theory of fractures. Thus in the child the cure may be effected in three weeks or a month, while in the adult it will require a much longer period; and in those advanced in life, a hundred, or a hundred and twenty days, are required.

If we recapitulate briefly what we have said with respect to the predisposing causes of this fracture, we shall see that shortness of the neck of the femur; very considerable openness of the angle which it makes with the body; deficiency in the projection of the great trochanter; flexibility of the osseous tissue; abundance of adipose matter; render it almost impossible that fracture of the neck of the femur should take place in children. In women, the less considerable obliquity of the neck relatively to the axis of the body of the bone, the length and prominence of the great trochanter, explain the greater frequency of this accident in them than in men. Sir Astley Cooper has said that fracture of the neck of the femur rarely took place before the age of fifty; but there are many exceptions to this rule. There are also some other particular circumstances which predispose to fracture of the neck of the femur—such as rachitis and cancerous diathesis; but as these are common to all parts of the osseous system, it is unnecessary to dwell upon them here.

Exciting causes.—What, then, are the exciting causes of this fracture? Almost all the patients whom we interrogate reply that they have fallen on the great trochanter, and in such manner that they have been unable to protect themselves with the arm, as is usually done instinctively. The frequency of this cause has been recognized by all authors. Thus of thirty-six cases observed by Desault, twenty-four were produced by falls of this kind. In infants and young persons who have fallen on this part, and who have been preserved from fracture, we meet with decollation of the epiphysis. Falls upon the trochanter are, however, far from being the sole cause of fracture of the neck of the femur; I shall point out others which play an important part: but I must first make a remark which has excited little attention; namely, that at the same time the patients fracture the bone they bruise the external parts. I this morning examined a woman who stuttered, and answered badly the questions put to her; but on touching the great trochanter it gave her much pain, although the limb was not moved; and this led me to examine the part, when I found a large ecchymosis upon it. This fact is of some importance in directing us to the part which the patient has struck, when he is himself unable to give an account of his fall.

I have said that other causes might produce fracture of the neck. This is, in fact, what happens in a fall on the soles of the feet, or still more upon the knees; but in both cases it is necessary that the muscles be stretched and inflexible. Sir Astley Cooper has observed that in London fracture of the neck of the femur often arises from a false step off the edge of the pavement. Under such circumstances the head of the femur impinges strongly against the cotyloid cavity: from this there results an effort tending to diminish the angle which the neck makes with the body of the femur. I have also frequently observed a depression of the cotyloid cavity produced by the head of the femur, in consequence of a fall on the feet or knees.

It is not from this cause, however, but from a fall on the haunch, that this accident generally happens. In this case the neck is placed between two opposite forces; the head of the bone being pressed on the one side by the weight of the body, while the great trochanter is opposed by that against which it strikes, and the point which most usually gives way is that immediately under the head of the bone, at the upper and internal part of its neck.

It has also been thought that this fracture may arise simply from a muscular exertion, and a case is quoted in which this is said to have taken place in a negro affected with tetanus.

Diagnosis.—This fracture may likewise arise from causes directly applied. These are usually projectiles—such as cannon-balls, several instances of which I saw after the days of July.

The diagnosis of fracture of the neck of the thigh-bone is not made without difficulty. It sometimes happens that a fall on the haunch, accompanied by bruising of the muscles and joints, imitate this fracture, while, on the other hand, the same cause may produce an actual solution of continuity, and yet the patient be able to run and walk. It is thus that persons have been known, after they had fractured the neck of the femur, to be able to reach home without presenting any shortening: it is thus that displacement of the fragments does not take place till after some hours, or even many days, either in consequence of some movement on the part of the patient, or of the examination made with a view of ascertaining the nature of the malady. Before we proceed, let us say a few words with regard to the cause of this consecutive displacement.

Secondary displacement.—It is known, that at the first period of its formation, the callus of long bones often yields, and produces deformities, when a perfect cure has been expected. Who has not seen oblique fractures of the femur give way under the weight imposed upon it when the patient began to walk again, at a time when all risk seemed to be at an end? The same thing happens in fractures of the neck of the femur. At the end of two or three months the provisional callus yields beneath the weight when the patient rests upon it, and shortening is the result. I have seen this at the end of two, three, and four months. It is therefore necessary that patients should be controlled by the proper apparatus, during 100, 120, or 140 days, or even more.

The weight of the limb, and still more that of the body on the broken part, is to be regarded as the great cause of displacement, whether primitive or consecutive. But another active power in producing displacement consists in the prolonged action of the cause which has given rise to the fracture. It sometimes happens that it buries the upper fragment in the thickness of the spongy tissue of the superior extremity of the lower fragment, and consolidation takes place in this situation rather quickly. Numerous anatomical preparations in the museum of the Hôtel Dieu exhibit this phenomenon, and sufficiently demonstrate the reality of its existence. Lastly, there exists another cause of displacement in fractures in the neck of the femur—namely, muscular action.

The primitive symptoms take place when, in a fall upon the heel or the knee, the shortening and displacement take place at the moment. In this case it is clear that, the upper fragment remaining in its place, the lower one is pushed up by the weight of the body. But a vertical fall is the least common cause of fracture of the neck of the femur; and when, as is most common, the blow is received on the trochanter, the cause tends not to shorten but to lengthen the limb. There exists, then, another cause of this shortening, which has until now been but little understood. It depends upon the adductor muscles, which, being designed to carry the limb outwards when it has rotated, are inserted on one side in the ischium, and on the other terminate behind and along the linea aspera of the femur. It is on these that the displacement, and, in part at least, the shortening, depends.

When there is displacement, the fracture is always easily recognized; but when this is not the case, the nature of the mischief may be suspected—without, however, being placed beyond doubt. I suppose that the symptoms are well characterized; that there is shortening, displacement of the limb outwards, and inability on the part of the patient to raise himself: it is necessary then to inquire if the limb preserves this shortening, or may be made to lose it by extension, and if the great trochanter turns on the axis of the femur, or on the extremity of the lever.

If the shortening is only some lines, it is difficult to distinguish it from that which is produced by an upward movement of the pelvis, caused by the contusion: the diagnosis becomes more evident if it extends to half an inch or more. Displacement, however, may also depend on luxation of the head of the femur, or upon an ascent of the pelvis. In luxation forwards, the head of the femur passes on the horizontal branch of the os pubis—then there is shortening; but the cause is detected by the hard tumor which may be felt to roll when the femur is moved. In luxation into the sub-pubic region the member is also turned outwards, but it is elongated; and there is in this situation an enlargement and unusual tension of the muscles.

In luxation upwards and outwards, the head of the femur is in the external iliac fossa: the limb is shortened, but the point of the foot and the patella are turned inwards, the heel being thrown out.

There is, finally, a luxation downwards and backwards, which I have only observed two or three times: the limb is then turned inwards, and sometimes a little elongated, and it cannot be brought into its ordinary state except by the effort of reduction, and, once reduced, the displacement does not again recur. Thus, the distinctive character is as follows:—shortening produced by a fracture yields to the smallest effort at elongation: shortening

produced by a dislocation is more difficult to remove, but, once reduced, the deformity disappears.

Let us now inquire what are the material effects of these fractures upon the bones, beginning with the glenoid cavity. I have found this cavity driven in several times by the head of the femur: this accident was produced by a fall on the feet or knees. In this case, the head of the femur impinges with violence against the bottom of the cavity, and, being the more resistant, breaks it. The most remarkable case which I have observed was this: the bottom of the glenoid cavity had been driven in, and the head of the femur, which remained entire, had passed completely into the pelvis; the neck, which had not experienced any solution of continuity, was so firmly locked in this accidental opening, that it was very difficult in the preparation to disengage it, and thus reduce this new kind of dislocation. In other cases, the cotyloid cavity is broken without the head of the bone being displaced; but the most common effect of the fracture is to be seen at the upper extremity of the femur in the radiated comminution of the head of the bone, the neck remaining entire. The most common cause producing this accident is a gun-shot wound acting directly on the part; though it also arises from falls on the great trochanter, and even on the soles of the feet.

The neck, however, is much more frequently the seat of fracture, because it forms a lever. Its diminished size towards its middle part also contributes to the production of the fracture. This may take place from below upwards; or from above downwards, depending upon the manner in which the fall occurs; but usually it is at the base of the neck that the fracture takes place: there are, however, infinite varieties in this respect.

M. Dupuytren's Opinion of Sir A. Cooper's Views.

I request you to observe, that according to the seat of these fractures they are called intra-capsular, or extra-capsular; and it is a distinction which has been much dwelt upon, because many practitioners think that it is difficult, or impossible, that the consolidation of the fracture can be effected when it has taken place within the capsule; while they admit that it is possible, and even easy, when it has taken place without the capsule. Astley Cooper, whose authority is so imposing in surgery, says expressly, that in all cases of transverse fracture of the neck of the femur within the capsular ligament, which he has had occasion to examine, he has never found bony callus; and he is persuaded that it is impossible; he has also made experiments upon living animals, which have confirmed him in his opinion; and the English surgeons have equally adopted the doctrine of their countryman.

But to the facts which they adduce in support of this non-consolidation, numerous facts of an opposite nature may be advanced. A considerable number of anatomical preparations show intra capsular fractures exceedingly well united; and those which exist in the cabinets of the faculty at Paris, and the school at the Hôtel Dieu, prove that this consolidation, with or without deformity, is real. Sir Astley Cooper has probably only seen fractures of the neck of the femur, which have not been cured, which have been treated ill, or not treated at all. This is the only way of explaining the opinion of the English surgeon, which is evidently erroneous. An examination, however, of these anatomical preparations, though eminently calculated to convince us of the reality of this union of fracture within the capsule, does not appear to have produced this effect on the other English surgeons who have visited our museum. Mr. Cross says he has attentively examined the preparations in the School of Medicine at Paris, and that none of them appear to him calculated to prove that bony union ever takes place, when the head has been completely separated from the capsular ligament. When one has seen the preparations at the Hôtel Dieu, which every person may examine at leisure, if he then denies the possibility of consolidation within the capsule, of a surety, I know not of what nature the proofs would require to be to produce conviction. For myself, I regard this consolidation as demonstrated, despite of what the English surgeons say.

Of the indications of Cure in Fractures of the Neck of the Femur.

At first sight one might suppose that it was the same in fractures of the neck of the thigh-bone as in other solutions of continuity—that it was sufficient to replace the fragments and maintain them in contact. But how many difficulties present themselves? How are fractures of this kind to be produced? Are we to use powerful extension and counter-extension? Certainly not; because we should thus increase the tension of the muscles, which is already very great. In luxations this may be effected by diverting the attention of the patient, and seizing that moment to produce the reduction. But in fractures this cannot be done, because the attention of the patient is concentrated on the injury. Another method of overcoming muscular action is to place the limb in a state of flexion, as recommended by Pott in fractures generally. I believe I am the first who applied these rules to fracture of the neck of the femur. I suppose, that, in

a case of this kind, extension and counter-extension have been used: it is evident that, if the limb be placed in a state of flexion, no difficulty is experienced in overcoming the displacement as to length and turning. But how are the fragments to be kept in contact? I suppose that the fracture of the neck of the femur is transverse, and that this part is divided into two halves; it may then easily be reduced; but the difficulty is to maintain it so, because the fragments are not placed fronting each other. Suppose, now, that the fracture is oblique; if the patient attempts to walk the lower fragment impinges slightly upon the upper one: this fragment, then, will offer some obstacle to its displacement; so that this is less easy than in a perpendicular fracture.

The general indication, then, is to reduce the fragments, and keep them in contact; and the principle established by Pott fulfils this purpose, by preventing the muscles from contracting; but, in order to obtain consolidation, it is necessary to employ at least twice as long a time as in fractures of the body of the bone; while it is necessary to add twenty, thirty, or forty days more, in order to obviate the risk of displacement occurring when the patient begins to walk. If this treatment be pursued, I venture to assert, that secondary displacement scarcely ever occurs,—that, when it does happen, it is extremely slight.

The best method of effecting reduction of fractures in general, and that of the neck of the femur in particular, is to diminish the resistance of the muscles, by placing them in a state of relaxation or demi-flexion, as you see us do every day, in the following manner:—The patient being laid on his back, and his pelvis fixed by means of assistants, the thigh is bent on the abdomen, by raising it, and making moderate traction; the limb is also bent upon the thigh. Scarcely is this done, when, without difficulty and without effort, the limb resumes its ordinary length, and the foot its natural direction.

MIDWIFERY HOSPITAL REPORTS.

GENERAL LYING-IN HOSPITAL.

Dystocea from Faulty Position.

Of dystoeca from faulty position of the child, there have been two cases, each of which possessing considerable interest.

Case I.—"August 14th, 1833, Elizabeth Cross, æt. 21, delivered of a girl; first child. Pains came on in the morning; large quantity of liquor amnii; membranes broke at 7 P.M.; an elbow descended into the vagina. I introduced my hand into the uterus, but not being able to reach a foot, on account of the violent straining of the patient, I hooked my finger into the bend of a knee, and brought it, and then the foot, into the vagina. The hand of the prolapsed arm, which had gradually descended between the labia, now went up of itself, and the child was turned without peculiar difficulty. The cord, which I had felt from the first, had no pulsation; the child was delivered with much ease. A dose of ergot was given, but little uterine action followed. Hæmorrhage appeared, and the cord being extremely weak, I was obliged to pass up my hand and bring away the placenta. She fretted very much about her child; and to this I in great measure attribute the repeated and profuse floodings which followed. Cold was applied externally and internally; water was poured from a height upon the abdomen, and constant circular friction employed. The uterus became firm, but still she flooded; and I removed some very large coagula. At last, after a considerable time and great loss, I succeeded in producing a permanent contraction. She recovered without interruption."

In a lecture of mine upon this subject, which was published in the Gazette (Dec. 22d, 1832), an unusual quantity of liquor amnii was there mentioned as a cause of faulty position of the child; for from being over distended, the uterus cannot give that direction to the child's body which it otherwise would do. For a further consideration of this subject, I must refer to the above-mentioned number of the Gazette. This case, moreover, illustrates the necessity there is of being able to distinguish the different parts of the child by the touch. If I had satisfied myself that what I felt was a knee, I should have allowed the labour to take its natural course; but being convinced that it was an elbow, I had no choice but of passing my hand into the uterus and bringing down the feet. Examining the joints and extremities of a new-born child by means of the touch, is a practice which cannot be too much recommended to the younger members of our profession, as the experience it gives in cases of this sort is of the utmost value. An elbow is easily distinguished from a knee, from having a small prominence (the olecranon) in the middle; whereas in a knee there are two prominences (the condyles), with a depression between them. Another important practical point is illustrated in this case, viz. that we should never attempt to push back the arm before attempting to turn. The moment we bring down the feet, it will return of itself; and the presence of the arm in the vagina is no great obstacle. It was La Motte who had the merit of first pointing this out. In the present case I brought down but one foot, which, although it is a practice recom-

mended by Portal, Gifford, and others, I cannot advocate, whenever both feet can be reached with tolerable ease. Boer, La Chapelle, and Naegele, say the same; and these are by far the most experienced names in midwifery upon the continent in modern times. It has been considered by some practitioners as advantageous to bring down but one leg of the child, as the other is thus turned upon the abdomen. In this manner it resembles more nearly a presentation of the nates; and is not only supposed to protect the cord as it lies along the child's body, but, by rendering the labour more gradual and slow, the uterus is not so suddenly emptied; and we are more likely to avoid that state of inertia which but too frequently comes on at the moment of greatest danger to the child, viz. when the head is beginning to enter the pelvis. Nates presentations are decidedly more favourable than those of the feet; and this is, I apprehend, entirely owing to the child producing much more resistance than where the feet present. Still, however, in cases where turning is by any circumstances rendered at all difficult, I should not feel satisfied in merely bringing down one foot, if any considerable extractive force be required. We run a serious risk of injuring the child's pelvis, either at the acetabulum or sacro-iliac synchondrosis of that side. The hips of the child also do not so readily engage in the pelvis, when extracted by one leg, as when both are brought down.

Case II.—The other case of dystocea from faulty position of the child, viz. where the head and arm presented, is one which occurs very rarely. It is not uncommon to feel the fingers at the side of the head; but as it descends into the pelvis they slip up, and nothing more is felt of them; nor do they at all retard the labour. In the following case, however, it was very different.

"—— Dixon, æt. about 35, out-patient, 3d June, 1833, mother of a large family. Actual labour pains did not come on till early this morning, when the membranes ruptured, and the midwife found the hand presenting with the head. The pains continued strong, and a portion of the head slowly advanced through the brim of the pelvis. At one P.M. I was sent for, and found the head with a considerable swelling; the posterior fontanelle forwards, and to the left; the hand and wrist passing before the right sacro-iliac synchondrosis; the pains very strong. Her former labours have always been severe, requiring long and powerful pains to make the head pass. This being the case, I left her, desiring to be called in three hours if the head had not made any considerable progress. At five P.M. I was called: the pains *very* violent, and the head but little advanced. I attempted to apply the forceps, and introduced one blade, but the violence of the pains prevented the second from being introduced. As the head did not advance, I began to fear that with such pains the uterus would rupture. I had even introduced my finger, for the purpose of passing up the perforator, when a cessation of the pains shewed me that there was room enough for the forceps to pass on each side (transverse diameter.) I introduced the first blade, and, waiting for an interval of pain, I succeeded, without much difficulty, in passing the second. The forceps locked immediately, being in complete opposition; and the first extracting effort convinced me that the head had passed the obstruction. The pains now forced it down very rapidly; and in less than two minutes the head was born, shewing a very deep indentation over the anterior fontanelle, which I can account for from having felt the posterior fontanelle nearly the most depending part of the head. The child was alive; and I now compelled the shoulders and rest of the body to come slowly through the external opening. The hand (right) was born with the head, and the shoulders and rest of the trunk gradually followed. It breathed feebly at first, but being put into a warm bath before tying the cord, it recovered quickly."

Another case of a similar presentation occurred in the hospital, where the arm was bent completely over the head, the fingers reaching the ear of the opposite side. The child was, however, premature, and therefore the case not so interesting as it otherwise would have been.

Dystocea from faulty size, &c. of the child.

The following case does not belong so much to dystocea pelvica as to *dystocea from faulty size and form of the child*, (Naegele's second species), for I could not satisfy myself that her pelvis was unusually narrow, or in any way deformed.

Case III.—"Nov. 19th, 1833.—Elizabeth Barron, æt. 33, delivered of a girl, eighth child, second position. Pains came on the day before; but not having any effect upon the os uteri, an opiate was given at night. All the next day she had smart pains: the os uteri dilated well; and I thought I felt the posterior fontanelle towards the left foramen ovale. Towards night the head had not advanced, although the os uteri was fully dilated, and the vagina very spacious. A small quantity of liquor amnii had collected and come away. At ten I applied the forceps (the bladder and bowels being previously emptied); they passed up with perfect ease, and after some little exertion, the head began to move. I now found that there was a fontanelle towards the right foramen ovale, the sagittal suture in the left oblique diameter of the pelvis, and the other fontanelle, which I had pre-

viously felt forwards, and to the left, now no longer within reach; there was no swelling of the scalp, and a considerable fœtor of the liquor amnii, which was thick, brown, and slimy. I therefore concluded that the child was dead, forgetting that the soft passages had not made sufficient pressure upon the head to produce the usual swelling of the scalp. The head, on clearing the upper aperture, advanced quickly through the other parts, and the face turned towards the left thigh as it passed over the perineum. An immense living child was born, which weighed nearly ten pounds. A smart hæmorrhage followed, but ceased on the removal of the placenta."

[To be continued.]

CHARTER TO THE LONDON UNIVERSITY.

The petition from the proprietors of the London University for a Charter, which would give to them alone, of all the schools in London, the privilege of conferring Degrees in Arts, Medicine, and Law, is immediately to come before a Committee of the Privy Council appointed for that purpose. The prayer is opposed by Oxford and Cambridge, on general grounds; by the College of Surgeons, as adverse to the interests of medical science; and by the medical lecturers of the metropolis, as calculated to constitute a new and unjust monopoly in medical teaching. The latter body, amounting to above one hundred of the most eminent members of the profession in London, have petitioned for the establishment of a great Metropolitan University, without reference to ecclesiastical distinctions—in which all the efficient schools would stand on an equal footing, and be considered as colleges; the duty of examining candidates being placed in other hands than those of their own teachers. The parties are severally to be heard by counsel: and we may add, that if the Privy Council come to a decision on this point, one of the most important duties of the Parliamentary Committee will be at an end.

TREATMENT OF SYPHILIS.

To the Editor of the Medical Gazette.

Sir,

I beg permission to point out an error in your report of my paper, read before the College of Physicians, on the Laws and Treatment of Syphilis.

You have stated, that in the treatment of venereal sore-throat, that I have been accustomed to use the unguentum hydrargyri nitratis, and when that failed, the unguentum hydrargyri nitrico oxydi. The ointments I have been in the habit of using are the unguentum hydrargyri mitius, and when that has failed, then the unguentum hydrargri nitrico oxydi: I have never used the unguentum hydrargyri nitratis. You will much oblige me by correcting this, perhaps not unimportant, mistake.—I remain, sir,

Your obedient servant,

Robert Williams.

39, Bedford-Place, Russell-Square,
April 17, 1834.

MIDDLESEX HOSPITAL.

We are gratified to learn, that this hospital is forthwith to receive a considerable enlargement, so as to fit it for the accommodation of 260 patients.

WESTMINSTER HOSPITAL.

Mr. Thomson, of New Cavendish Street, has been elected Assistant Surgeon to the Westminster Hospital.

JACKSONIAN PRIZE.

The Jacksonian Prize, for the year 1833, has been awarded, by the Council of the Royal College of Surgeons in London, to John Green Crosse, Esq., of Norwich, for having produced the best Essay upon the "*Formation, Constituents, and Extraction of Urinary Calculi.*"

WEEKLY ACCOUNT of BURIALS,

From Bills of Mortality, *April* 15, 1834.

Abscess	2	Inflammation	59
Age and Debility	53	Bowels & Stomach	1
Apoplexy	6	Brain	3
Asthma	29	Lungs and Pleura	4
Cancer	2	Insanity	1
Childbirth	9	Liver, diseased	19
Consumption	82	Locked Jaw	1
Convulsions	37	Measles	6
Croup	5	Mortification	5
Dentition or Teething	4	Paralysis	4
Dropsy	19	Small-Pox	7
Dropsy on the Brain	25	Sore Throat and Quinsey	1
Dropsy on the Chest	1	Spasms	2
Fever	8	Stone and Gravel	1
Fever, Scarlet	6	Tumor	1
Fever, Typhus	3	Unknown Causes	10
Gout	1		
Heart, diseased	1	Stillborn	19
Hooping-Cough	11		

Decrease of Burials, as compared with the preceding week . . . 29

Notice.—The letter of a Provincial Hospital Surgeon is in type, and has only been postponed for want of room.

Erratum.—In Mr. Thornhill's paper, in our last number, at page 56, line 37 of the second column, *for* "has been no digestion," *read* "has been no alvine dejection."

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, APRIL 25, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XXIX.

DIFFICULT LABOUR.

Having considered the first cause of lingering labour,—namely, inefficient action on the part of the uterus,—we are next to direct our attention to the second—embracing those cases in which the uterus is acting powerfully and energetically, but where there is a want of due and proportionate space in the passages for the ready exit of the child: and first I shall speak of

DISTORTION OF THE PELVIC BONES

as a cause of delay, and of increased suffering.

If we look upon these organs in a mechanical point of view, (which we must do before we can arrive at a true knowledge of the process of parturition, even in its simplest and most easy state), we shall immediately perceive that *size*, as it respects the head and pelvic cavity, is entirely a relative term; and that a pelvis preternaturally small from original formation, or a head unusually large, will each also in practice produce difficulty in the same degree as they deviate from the standard dimensions: so that it matters little whether the disproportion be the result of diseased action or not;—provided it exists to a certain extent, it must always be the occasion of a protracted struggle.

We learned at the commencement of this course, that the pelvis may be distorted in the brim, the cavity, or the outlet; either of which parts may singly become diminished in capacity, or they may all have been involved in a more general disease. When one part only is the subject of distortion, it is usually the brim that suffers.

Three varieties.—As on a former occasion I arranged pelves in general into four classes, so we may now, for practical purposes, divide distorted pelves into three varieties:—the *first*, in which the pelvic brim is so contracted as not to permit any part of the child's head to enter through it; the *second*, which has allowed the head to descend so low as to occupy the whole or the principal part of the cavity, but whose outlet is too narrow to admit of its escape; and the *third*—of that intermediate size—which has permitted some portion of the head to enter through the brim, and partially to occupy the cavity; while the principal bulk remains above. These three cases practically assume a very different character, and require somewhat of a distinct consideration.

First variety.—I shall first speak of those cases of a more aggravated nature, in which the pelvis is so much distorted as not to allow any part of the child's head to engage in the superior straight. If the woman's person be very much deformed; if the spine be curved, and the thigh-bones also bent; we may suspect,—even previously to making an internal examination,—that the pelvis has likewise suffered severely. But when we have positively ascertained that this is the case, and that the diminution of space is so excessive that no part of the child's head can be received into the capacity of the brim, it becomes our duty not to allow the patient's strength to be undermined by the fatigue necessarily attendant upon such a labour; but early to have recourse to some means for the purpose of relieving her: because,—as you may satisfy yourselves by the specimens before you,—it is physically impossible for a head to be eventually expelled whole through a pelvis whose capacity in

the superior aperture is so contracted as not to admit any portion of it after the evacuation of the liquor amnii;—provided, indeed, the expulsive pains are fully established.

Such being the case, then, it would become a question what means we should adopt: and if upon an examination, conducted with the utmost care, we find that there is less space at the brim than two inches and a half laterally, by one inch and a half in the conjugate diameter; or three inches by one inch and a quarter; we ought to consider it our duty,—however painful and appalling that may be,—at once to propose the Cæsarean section, as the only means by which it is possible to save the mother's life; and as offering also the sole chance of safety to the child. If it be thought necessary that this operation should be performed at all, it ought at any rate to be undertaken before the patient's system has sunk, from a long continuance of the excessive fatigue of labour. This recommendation I think it right to inculcate, in consequence of the difference so remarkable in the result of the cases operated upon in this country and on the Continent. In the British isles, out of nearly thirty operations, we have only two instances of recovery on record—one under the excellent care of my friend Mr. Barlow, and another in which the operation was performed in Ireland by a midwife named Dunally;—whereas, on the Continent, a fortunate termination after the same operation has repeatedly taken place. How can we account for this discrepancy? The astonishingly superior success on the Continent is not to be attributed to the climate being more favourable to capital operations—to the human system being in a state to bear them better—nor to the surgeons abroad possessing either more general scientific knowledge as to the mode of performing dangerous operations, better practical adaptation of that knowledge, or evincing greater care and anxiety about the termination, than obtains in England—but simply because the operation has been performed earlier in the labour, before exhaustion has supervened.

The fact is not to be concealed, that in different parts of Europe, and especially in Catholic countries, both has this operation many times been had recourse to, under circumstances in which a British practitioner would not have considered himself warranted in proposing it—where, indeed, there has existed sufficient available space in the pelvis to admit of the extraction of the fœtus *per vias naturales*; and also that the women, more under the influence of their clerical pastors than ours are, have readily and cheerfully submitted, from a sense of religious duty, to this dreadful expedient, that they might not deprive their children of the benefit of admission within the pale of the Christian church.

By having recourse to this means, then, comparatively early in the labour, the best chance is afforded to the patient of the preservation of her own life, as also of her infant. Nevertheless, no rightly-judging man would venture, on his own single responsibility, to urge the propriety of an operation so unusual in its necessity, so appalling in its character, and terrible in its consequences, as the extracting a fœtus *ex utero*, by an extensive abdominal incision; but before proceeding to its execution, he would naturally be desirous to consult with all those practitioners in his neighbourhood whose counsel and sanction could be obtained, and in whose opinion he confided.

These cases of extreme distortion are fortunately of rare occurrence: the operation of Cæsarean section, indeed, has not been found necessary in this extensive metropolis for very many years; and we may all hope, therefore, with some degree of confidence, that it will never fall to our lot to witness an instance in which we may think ourselves called upon to advise it. Not so, however, with the lesser degrees of diminution in the pelvic apertures: these we meet with almost daily:—certainly not so often in the open country, among the hardy agricultural peasantry; but by no means very infrequently among the inhabitants of great and crowded cities, and the population of manufacturing districts.

If, then, a case come under our notice in which there is more available space than that I have just mentioned as imperatively requiring the excision of the child from the uterus through the abdominal parietes; and yet where the pelvis is so small that we are persuaded the child's head cannot pass entire, provided it have arrived at full intra-uterine maturity;—a case, for instance, in which the conjugate diameter at the brim measures about two inches;—it would still not be right to let the patient remain very long in labour without means of relief being used: but we should perforate the head comparatively early in the process, that we might have the advantage of full unimpaired uterine energy to aid us in our extractive efforts. Do not, however, go away impressed with the idea that I recommend perforation should be had recourse to early in labour in all cases where we are likely to find it necessary afterwards. It is only in those instances where a moral certainty exists that the child cannot pass unmutilated, that we are at all authorized to use such deadly means, before the maternal system is beginning to sink.

Fortunately for the facility of teaching, it is not necessary to draw sueh a nice distinction between those cases in which it is desirable that eraniotomy should be performed early, and those absolutely requiring the same operation later in labour; beeause, inasmuch as the means used are the same under both cireumstanees, it will seldom prove of material importanee whether the operation be performed an hour sooner or an hour later. But the case is widely different when it becomes our painful duty to discriminate between a pelvis through which a head ean be extraeted after the brain has been evaeuated, and one that will not allow the passage of the child, lessened even to the utmost degree that art can aceomplish. For it would be most distressing and grievous,—after having perforated the skull and made foreible attempts to extract *per vias naturales*,—to discover that the pelvis did not afford sufficient room for the eompletion of delivery, but that the case required to be terminated by the abdominal ineision. Not only, indeed, must the infant then necessarily be born dead, but the patient would be subjected to much additional and unprofitable agony during our efforts at extraction. A diminished chance of ultimate recovery would also be offered to her, in consequence of the pain and exertion attendant on our frustrated endeavours, and the pressure, and perhaps contusion, to which the soft parts must more or less have been exposed.

I have already laid it down as a general principle, that unless there be a clear available spaee of three inehes in the eonjugate by the same measurement in the lateral diameters at the brim, we are not to expect that the child will be born without assistanee; but I must repeat here what I before ineuleated—and what I trust still dwells in your memory—that it is not merely beeause the pelvis is deformed to such an extent that we are authorized to interfere while the powers are vigorous: because the ehild may be immature; its head may be small; the bones may be less ossified, and may overlap eaeh other more than is usual; and a greater probability may therefore be afforded of a natural termination of the case. Under such eircumstanees, then, it beeomes our boundeu duty to wait till the system is suffering from exhaustion,—and that considerably,—before we have recourse to such a horrible alternative as the instruments for eraniotomy supply. On the contrary, however, if the space exeeed two inches but in a trifling degree, it is very evident that a child, sufficiently perfected in the womb to sustain independent existenee, eannot be expelled whole; and we are therefore fully justified, under this particular degree of disproportion, in interfering early; that we may save the woman from mueh present suffering—that we may avert the danger of rupture of the uterus—and remove her from the chanee both of sinking under the effects of long-continued pressure—as well as of suffering such dreadful destruction of parts as must render her a burthen to herself and her friends, for the remainder of her life.

Second variety.—In the sceond variety, where the head has entirely passed the pelvic brim—has become jammed in the eavity, and, for want of sufficient space in the outlet, eannot make its exit—the patient generally suffers extreme pain, not only from uterine contraction, but from uninterrupted pressure on the parts within the pelvis. We are to reeollect that the pelvic viseera are exceedingly nervous, very liberally supplied with blood-vessels, and soon go into a state of disease; that, although they possess a great restorative power within themselves, inflammation indueed by contusion, the result of pressure, is very likely to terminate in suppuration, or in gangrene: and when a disposition to sloughing onee commences, it is impossible to say where the destruetive process may stop. Beginning in the lining membrane of the vagina, all the vaginal eoats may take upon themselves the same action; the bladder and rcetum may be implieated, and the three cavities may be thrown into one,—than whieh it is impossible to coneeive a more miserable state of human existenee.

When, then, the head is jammed in the lower part of the pelvis, we must eminently fear eontusion, inflammation, suppuration, and sloughing. We have, also, partieularly to dread injury to the bladder: that organ may burst, or fatal inflammation may oeeur, eonsequent upon its over-distention. There is, likewise, great danger to the ehild from the eompression which the brain must suffer; but partieularly also from that pressure to whieh the funis umbiliealis is exposed, owing to the fœtal body being tightly embraeed by the uterine parietes under their contractions. It is often remarked that the ehild is born dead under this peeuliar speeies of lingering labour, especially if the head have remained long within the pelvis,—yet this may prineipally arise from pressure on the umbilieal eord, whieh, by impeding the eireulation, destroys life.

This ease, then, is one of a very dangerous and diffieult character; and it beeomes a most delicate question, how long we shall allow nature to struggle unaided, —and whether, at any particular period of time, we shall have recourse to instrumental means. Many rules have been laid

down for our guidance under this emergency. Some obstetrical practitioners of eminence tell us, that if twenty-four hours have elapsed since the rupture of the membranes; if, during that time, the patient have been suffering under strong, regular, continued pains,—the process of labour not having been interrupted or suspended during the interval,—we are always warranted in delivering by the forceps; provided we can terminate the case without injury: that, by following this simple direction, we shall be acting most wisely, and shall never be at fault. This would be a most excellent rule, because so easy of application, if all women suffered alike under labour; if the pains were always as active in one patient as in another; if each woman was able to undergo the fatigues of labour equally with another, and to bear up against the bad effects which that fatigue must produce; but this is by no means the case: we find every diversity in the suffering which a woman experiences, and every diversity in the manner in which it is sustained. In some cases the pains are very slight, though they may be continued; in others, they may entirely subside for a few hours, and then there is a cessation both of painful sensation and also of exertion. But, even if the number and intensity of the pains were equal in every respect, different women bear the fatigue of labour with very various degrees of suffering to the system: thus, one woman will suffer more in a labour of six hours' duration than another in one of twenty-four; although in both the pains may be equally frequent, intense, and powerful. We cannot, then, take time as our guide, so well as the effect produced on the constitution. There is one remark, however, with regard to time, which must not be passed over unnoticed; viz. the period during which the parts are subject to continued pressure *without intermission*; for, as just observed, the soft parts within the pelvis can, no more than other soft parts of the body, endure continued pressure for any length of time without injury, and the vessels must eventually become strangulated. If, then, the head be locked in the pelvis for a certain number of hours, even although the patient be in tolerably good spirits, and the pains be frequent and strong,—lest the soft parts should receive injury,—we must deliver; provided we can effect our object by means compatible with the life of the child. How long, then, may we allow the head to remain locked in the pelvic cavity? Dr. Hamilton believes it may remain six hours, without fear of injury to the lining structures; but I would rather limit the time to a less period; and think, after the lapse of four hours, we are warranted in delivering, provided we can do it without injury either to the mother or her offspring.

But it is necessary we should be able to distinguish a case in which the head is locked, from one where the pressure is not either constant or general. I have already mentioned that the head descends under uterine contraction, and retreats when the pain goes off: that the same is observed from the commencement of labour till the expulsion of the face over the perineum: and this, we have learned, is a valuable provision of nature; because, in the interval of pain, the soft parts are entirely relieved from pressure: but if the head be so impacted that it cannot be propelled forward while the uterus is acting, neither can it retreat in the interval of contraction; and it is by this stationary situation we know that it is locked. We ascertain, then, that the pressure has become injurious, both by local and general symptoms,—by heat, tumefaction, dryness and soreness in the external parts and vagina,—by the pulse being unusually quick, the countenance anxious, the tongue furred,—by thirst, rigors, and vomiting,—by symptoms, indeed, of general fever supervening on local injury.

So long, then, as there are good pains, the pulse is under 100, the countenance good and cheerful, the tongue and mouth moist and clean; so long as there is no vomiting, nor rigors, nor heat, swelling, and pain of the parts; no green discharge from the vagina; no tenderness of the belly; so long as the head retreats when the pain is present, and advances in its absence; while, at the same time, the finger can be passed easily up to the pelvic brim, around or by the side of the head; so long as there is any progress whatever,—even although it be most trifling,—from hour to hour; so long as there is neither flooding nor convulsions, nor any other symptom of distress in the system; we have no need to resort to the use of instruments. On the other hand, again, provided such symptoms as I have mentioned arise, it is of little practical importance whether the labour has lasted ten hours or forty; for we are assuredly warranted in delivering artificially, especially if the case can be terminated by harmless means. We shall hereafter take occasion to examine these symptoms more in detail.

Third variety.—I have considered that the third variety, where the head is partly protruded through the brim, the vertex dipping into the cavity, while the principal bulk remains above. A case in which the pelvis measures about three inches in its conjugate diameter, is of this description. But I cannot impress too forcibly or too frequently on your minds

that, although we know the size of the pelvis accurately, and have ascertained beyond a doubt that its measurement is even rather below three inches than above, we are not, on that account alone, warranted in taking an instrument in hand: for it must have occurred to every practical man frequently to have observed the head wonderfully adapt itself to the irregularity of the pelvis; so that eventually it makes its exit, under circumstances which a few hours before allowed of no expectation, and but little hope, that a natural and unaided termination would be effected. We must not, therefore, apply instruments merely because there is a small pelvis, provided it be moderately capacious. If the space, indeed, in the conjugate diameter of the pelvic brim be little more than two inches, I have already laid it down as a rule that it is better early to perforate the skull; but if it approach three inches, it is imperative on us to wait till symptoms appear indicating danger. By some we are recommended, in cases likely to be rendered difficult and protracted by the slighter degree of distortion, —embraced under the head we are now discussing,—to introduce the hand into the uterus, grasp a foot, oblige the fœtal body to revolve on its own axis, and bring the breech into the pelvis, terminating the labour by the operation of *turning*. I cannot find language sufficiently strong in which to deprecate this mode of proceeding, as a general principle. The dangers which envelop it are many and great, both to the mother and her infant, and have already been partially glanced at. I have good reason to believe, and to hope indeed, that such a means of concluding such a case is now entirely exploded from the practice of the well-informed obstetrical surgeon.

TUMORS IN THE PELVIC CAVITY.

The third cause of lingering labour is the presence of tumors in the cavity of the pelvis.

From your anatomical knowledge of the contents of the pelvic cavity, you will very justly conclude that the different organs are liable to disease, enlargement, and the formation of tumors in their structure. These tumors vary exceedingly in their nature, consistency, and size; sometimes they possess the solidity of bone itself; at others, their contents are of the most fluid character. According to their size and unyielding nature, will be the difficulty which they occasion under labour.

Exostosis.—The most solid of all the tumors that we meet with in the pelvis, is a knobby, bony growth, taking its origin from some portion of the parieties themselves;—an exostosis. But it is fortunately of very rare occurrence; indeed, so infrequent, that I have myself never met with an instance. It is generally situated at the back part, behind the rectum, and springs from the cavity of the sacrum. This kind of tumor we shall mostly be able to discriminate by its situation, its extreme hardness, the irregularity of its surface, its immobility, its knotty feel, and insensibility to pressure.

Treatment.—Our treatment of a case rendered lingering by the presence of a tumor of this description, will altogether depend upon the size of the diseased growth itself. If it be very small, it is possible that the head may pass without assistance; but if, on the contrary, it be large, occupying a considerable space—since it would be impossible to remove it by operation, so as to render the cavity more capacious—we must be guided by the common rule (provided there be the most distant probability of the child's passing), that of waiting until symptoms appear which demand delivery; and according to the magnitude of the tumor we must select our means: the forceps, long or short, if they offer a fair chance of success, should be preferred; if neither of those instruments avail us any thing, we must call in the aid of the death-inflicting perforator. It is certainly possible that an exostosis may have attained such a size as to preclude our extracting the fœtus at all through the vagina: under such a formidable case, we should be compelled to have recourse to the Cæsarean section.

Scirrhous or dropsical ovary.—An ovary, enlarged from scirrhous or dropsical disease, may impede the passage of the child's head. We shall, towards the termination of the course, become informed that the ovary is liable to diseases of various kinds, of which dropsy and scirrhus are the most frequent; that under either of these affections,—particularly dropsy,—this originally small gland enlarges astonishingly in size, so that its coats being extended into an enormous cyst, have been known to contain many gallons of fluid. We cannot be surprised, then, at the impediment offered under labour, if pregnancy and an ovarian tumor exist together. When enlarged by disease, indeed, the ovary generally rises by degrees from the pelvic into the abdominal cavity: but occasionally it becomes bound down by adhesive inflammation to the subjacent parts; and if, under such a state, the woman conceives, and carries her fœtus to the full period, her labour must necessarily be difficult. Even if it be not confined to the pelvic cavity by adhesion, it may be found under labour occupying it more or less, never having commenced its ascent into the abdomen; being prevented, perhaps, by the gravid

uterus having already taken possession of that space: or a portion of the tumor may have prolapsed, and subsided during gestation; for pregnancy by no means interferes with a continuance of such an enlargement.

The situation of this tumor is also external to the vaginal coats, and it is generally to be felt within one or other ilium. It will probably be somewhat moveable; it will neither be so hard nor so irregular as an exostosis, and will most likely possess more sensibility.

Treatment.—The treatment here must likewise depend upon the size and solidity of the tumor. If its true character be detected before the head has become much engaged in the pelvic brim, we may possibly,—provided it be not adherent,—succeed by steady pressure in pushing it up above the brim, and consequently out of the way of the head's advance. If we cannot accomplish this object, we must then, also, act upon our common principles, and wait until symptoms appear requiring our interference: for if the tumor be soft, it is very probable that the descending head will compress it into a flattened form, or squeeze a part of its contents upwards above the brim, and thus diminish its resisting power: while, on the contrary, if the disease be of a solid kind, it would be wrong to have recourse either to obstetrical or surgical instruments, until necessity compelled us. When this necessity appears, it will then become a question, whether we shall puncture or excise the tumor, or extract the child by instrumental aid. It is impossible to lay down any general rule applicable to every individual case; but we may establish it as a principle, that if the tumor possess the least degree of fluctuation evident, a puncture should be made into its substance by means of a trochar introduced through the vagina. Even if the disease be simple dropsy, we must not expect to evacuate all the fluid, because most probably the tumor will be formed of separate cysts of different sizes, possessing no communication with each other: but we may let out a part of the contained water; and if, fortunately, the cyst we puncture should be large, we shall reduce the general bulk so much as to afford a fair chance to the head to pass. Should the contents, however, be found of a cheesy, or gelatinous consistence, too thick to run through a trochar, it would then be right to make an incision into the mass also through the vagina, of half an inch or an inch in extent, with the hope of entirely evacuating the sac. If, lastly, by these means we make no impression on the size of the tumor, we must then determine whether we should extirpate it through the vagina, or deliver the child either by the forceps, or by opening the head. If there be the least chance of delivering by the forceps, that would be preferable to either of the other methods; but if the forceps fail us, we have no choice left except either dissecting out the tumor, or perforating the skull. The removal of the tumor would, no doubt, be both very difficult and hazardous on many accounts; and horrible as the alternative is, I should, in my own practice, rather destroy the child than subject the mother to such a formidable operation.

Scirrhous glands.—Another species of tumors offering an impediment to the head consists in the glands situated along the hollow of the sacrum having become affected with scirrhous enlargement.

Scirrhous glands may be detected by their situation, irregularity, and hardness—by their being very sensitive—by their forming a chain of indurated tubercles also external to the vaginal coats—and by their being more or less firmly adherent to the surrounding structures. Concerning the treatment of such unusual cases, I have nothing to add to what I have just offered for your guidance when speaking of enlarged ovaries.

Abscesses.—Abscesses will occasionally form in the glands of the pelvis during pregnancy, and in this case there would be decided fluctuation present. An abscess might be distinguished from a dropsical ovary by its situation, perhaps—its excessive tenderness on pressure—and its formation having been preceded and accompanied by symptoms of local inflammation, and indications of the suppurative action. An error in diagnosis, however, would be but of little consequence, since the treatment employed in the two cases must be essentially and positively the same. There could be no hesitation in puncturing such a tumor through the vagina, and letting out the pus. The difficulty would then be over, and the head would most probably pass.

Polypi.—Tumors, however, formed within the uterus, or growing from the internal surface of the vagina, will sometimes impede the passage of the head. These are of a polypous character; they are fleshy and solid in their structure, and grow occasionally to an amazing size. It is singular that the presence of these tumors in the uterus does not prevent conception taking place; which fact I have myself had more than one opportunity of witnessing.

One case which occurred to me in the year 1824,—and which is recorded in the second part of my father's "Practical Observations on Midwifery,"—I will give in illustration. My father had been requested to attend upon a lady in her confinement of her third or fourth

child, but was from home when the messenger arrived to summon him; I consequently went in his stead. I found the patient suffering severe pains, and using forcible bearing-down efforts, under the belief that the child was about to pass immediately. On making an examination, I instantly detected that the pelvic cavity was occupied almost entirely by a solid fleshy tumor, much larger than a goose's egg, which was pressing considerably on the perineum: the os uteri, at the brim of the pelvis, was dilated to about the diameter of a crown piece; and the membranes, unruptured, were being forcibly propelled against the upper part of the tumor with the return of each uterine contraction. I had no difficulty in determining that the tumor was of a polypous character, by its firm consistence, its shape, its situation *within* the vaginal cavity, and its attachment within the os uteri. The mouth of the womb dilated rapidly, the membranes burst speedily; and in less than an hour after my arrival, the head, under the action of powerful throes, forced the principal bulk of the tumor external to the vulva, and itself instantly followed. At the same moment my father entered the room, and, with myself, had an opportunity of examining the tumor lying forth between the thighs. Now, however, that the difficulty, as far as regarded the birth of the child, was removed, it became a question in what manner the polypus should be treated;—whether it should be taken away immediately by a knife, after having secured the vessels of the stem by placing a ligature around it; or whether it should be returned into the vagina, and a future opportunity taken of tying it, according to the commonly-adopted method. In favour of the first suggestion, it might have been urged that the tumor was at that time so completely under control, as to render the operation one of the easiest description; and against it, that the difficulty of surrounding the stem of a polypus, by means of the double canula, is but trifling; and, in the case before us, might easily be accomplished at any time. Besides,—the uterus being so eminently disposed to take upon itself inflammatory action in all cases after delivery,—there was great danger lest the double irritation of the inflicted wound and the attached ligature should excite a disease which it would be difficult to keep in check. Again, it seemed likely, that, as the adventitious growth was nourished by the same vessels which supplied the uterus, these vessels had become enlarged in a proportion somewhat equivalent to the increase in the calibre of the uterine vessels themselves. If such were the case, it was fair to infer that, as the uterine vessels shrunk after delivery, the vascularity of the polypus would also be materially diminished; and that this diminution in the bulk of the tumor would render its removal altogether less formidable. These reasons induced us to delay the operation at least until the changes consequent on delivery were accomplished, and the puerperal state had terminated. The result both justified our expectations, and confirmed the correctness of our reasoning; for my father made several vaginal examinations during the few first weeks after delivery, and satisfied himself that, as the uterus contracted, the tumor also lessened in size. After the lapse of nearly four months—no symptoms appearing in the meantime to call for earlier interference—the polypus was tied in the usual manner, and sloughed off in five days; and I here present it to you, scarcely so large as a walnut divested of its outer husk.

There can be but little difficulty in determining the nature of a case of this kind. The pear-shaped, solid, tumor will be felt more or less occupying the cavity of the vagina, attached by its pedicle either to the vaginal membrane itself, or to the uterus; or the stalk will be lost, as it were, in the cavity of the uterus, and the point of its connexion with the healthy structures will probably not be discernible; we shall be able to pass the finger all around it, to encompass its bulk and determine its shape.

Treatment.—Under the presence of such a tumor, then, how shall we act?—To a certain extent we must follow the common principles, already so frequently repeated: let us give nature a fair trial, in the hope of witnessing a termination such as I have just detailed; but if the patient begin to sink—if the pains begin to flag—if the parts become tumid, hot, or dry, shewing a disposition to inflame, then we must consider it necessary to interfere; and the question will naturally arise, whether we shall remove the tumor, or deliver the patient by instrumental means. If we can deliver easily by the forceps, let us have recourse to them, because they do no injury to the child or mother; but if delivery is impracticable through their agency, rather than perforate the skull a ligature should be put around the stem, and the tumor should be cut off below. On the removal of the tumor the child's head will probably be expelled.

Scybalæ.—There is still another adventitious tumor which may impede the passage of the child, viz. a collection of fæces in the rectum. It is not usual to meet with a case of this kind in the higher classes of society; but it is by no means uncommon among the lower orders; and I

have seen more than one instance, in which the mass contained within the rectum was so hard and so large, as for some time to obstruct the exit of the head.

It is not likely that we should confound this cause of protraction with any morbid pelvic growth; because by passing the finger into the vagina, and tracing the rectum, that gut will be felt bulging forward; but if any doubt remain, an examination of the rectum itself will generally dissipate it.

Treatment.—The obvious indication here is, to evacuate the contents of the bowels. This may be done by two or three enemata thrown up in succession, so as to liquify the fæces and dislodge them. But cases have occurred in which all attempts to inject a fluid were rendered nugatory, in consequence of the hardness and compactness of the mass. It has been advised that we should, in such an aggravated state, endeavour to empty the rectum by means of the handle of a spoon. I need not remind you, that this cause of difficulty might be entirely removed by proper attention being paid to the due evacuation of the canal during the last five or six weeks of utero-gestation.

CASES AND REMARKS,

By R. Arrowsmith, M.D., Coventry.

[Continued from p. 92.]

I.—Partial Peritonitis.

Case II.—The case which immediately follows is from Dr. Abercrombie's work on Diseases of the Stomach, &c. 2d edit. p. 178.

A lady, æt. 20, was seized with peritonitis on the 9th July, 1822, from which she was convalescent on the 12th. On that night she took aperient pills, which operated with much irritation, and the symptoms of inflammation returned, with a good deal of depression; but in a few days she was again convalescent. About the 20th, she had pain in the hepatic region, which was quite removed by local bleeding, and all her symptoms were favourable. About the 25th she had a parotid swelling, which advanced slowly to suppuration, and was of considerable size; but she would not consent to its being opened.

"On the night of the 2d of August," says Dr. A., "she went to bed in her usual health, having been in the drawing-room through the day, and without any complaint except the parotid swelling. Early in the morning of the 3d, she awoke in great distress, with cough and oppressed breathing. When I saw her (about 11 o'clock), her face was cadaverous; her breathing frightfully oppressed, with a rattling sound. The pulse was very frequent, and there was in the room an intolerable fœtor. She died about twelve.

"*Inspection.*—Betwixt the diaphragm and the upper surface of the liver there was formed a distinctly defined cavity, lined by a cyst of coagulable lymph, and containing at least a pound of thin puriform matter, of intolerable fœtor *. The right lung adhered extensively to the diaphragm, and the diaphragm was perforated by a small opening, by which the matter from the abscess had passed freely into the bronchial canals, and it was traced as far as the trunk of the

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* "En lisant avec attention les observations d'abcès du foie publiées dans les Memoires de l'Academie de Chirurgie, on reconnait qu'evidemment, dans plusieurs cas, l'abcès dont il est question n'avaient pas leur siege dans le foie, mais bien entre ce viscere et le parties contiguées, et dès lors, très-probablement, dans le peritoine."—*Chomel*, Dict. de Med. XVI. 328.

Chomel, in discussing the treatment of peritonitis, remarks, that "Une large saignée dissipe, quelquefois aussi, comme par enchantement, une peritonite partielle." I have had several opportunities of verifying this observation, of which the following is an example:—Mrs. D., æt. 40, had annually, for the last two or three years, an attack of circumscribed pain and tenderness of abdomen, with fever and depression, for which she had been treated with purgatives and local applications, and from which she had each year a tedious and imperfect recovery. In December last she had a return of the pain, which now occupied the right side of the abdomen (chiefly the inguinal region), with tenderness, fever, sickness, and depression. She had been freely purged, and had had seventeen leeches applied the evening before I saw her. The leeches produced some transient relief, but the pain and sickness afterwards recurred with severity. She was placed upright in bed, and bled *ad deliquium* (about 20 ounces of blood were lost), with immediate and complete relief. The bleeding was followed by a dose of opium and calomel, and a blister was applied to the side of the abdomen. When she first sat up, she complained of a dragging sensation in the part, but she had no return of pain or sickness, and her recovery was rapid and complete.

Partial peritonis, leading to disorganizations of, or morbid products in portions of the peritoneum, no doubt, often lead at a future period to general peritonitis. Two years ago I was present at the examination of the body of a man who had died of general peritonitis. The treatment of the case had been conducted with promptitude, energy, and judgment; but in vain. On examination a portion of omentum was found to have undergone a previous thickening and alteration of structure (from circumscribed inflammation, no doubt), and contained within a sac a grumous fluid, which had little or no resemblance to pus. This previously diseased structure had originated the general peritonitis, of which it formed the *point de départ*.

trachea. The liver was sound in its internal structure, but on its peritoneal coat there were some marks of inflammation. The intestines adhered to each other through almost their whole extent, to the omentum and to the parietes of the abdomen; so that no portion of intestine could be traced without tearing these adhesions, which were soft. Throughout this mass of disease there were, in several places, cavities of various sizes, containing purulent matter; one of these, on the right side, seemed to communicate by a small canal with the great abscess above the liver. In the posterior part of the pelvis, behind the uterus, another great abscess was discovered, containing nearly a pound of thin fœtid pus. It was formed by adhesions betwixt the intestine, the uterus, and the ovaria, so that it was completely cut off from the other parts, and remained entire after the examination of them had been concluded."

Case III.—Andral, in his *Clinique Médicale* (tom. iv. p. 664), relates the following case:—

"A printer, æt. 23, entered La Charité, far advanced in tubercular phthisis. Moreover, below the cartilaginous border of the left false ribs, the abdominal parietes were elevated by a tumor, rather oblong than round, which appeared to pass behind the ribs, and was felt to extend the breadth of four fingers beneath them. Transversely, it reached from the level of the abdominal extremity of the eleventh rib almost to the linea alba. Percussion elicited a very dull sound at the lower part of the thorax on that side where usually the presence of gas in the great *cul de sac* of the stomach renders it more resonant than elsewhere. This dulness of sound could only depend on the three following circumstances:—1st, a pleuritic effusion, which had depressed the diaphragm and thrust the spleen forward; 2dly, unusual development of the spleen itself; or, 3dly, a tumor of new formation, originating in the neighbourhood of this organ. But there was no other symptom of pleuritic effusion, nor had he ever had intermittent fever—an affection which often leaves an engorgement of the spleen. Three years before, he had experienced in that situation a very acute pain, which had obliged him to keep his bed for several days: the pain was accompanied by fever. He was once bled generally, and had leeches applied to the hypochondrium several times. The pain was called rheumatic. At the end of fifteen days it ceased, but since that period he has constantly experienced a sense of constriction and uneasiness in the left hypochondrium. He had not perceived the existence of the tumor, which, however, was tender on pressure. The decumbent position was unaffected by it. From these circumstances we conjectured that the tumor was not formed by the spleen, but that it was developed in the peritoneum; having its origin in a partial inflammation of that membrane, which three years previously had announced itself by pain in the left hypochondrium. The pulmonary affection soon proved fatal.

"*Inspection.*—On opening the abdomen, the tumor recognized during life was found in the left hypochondrium. It had at least the volume of two large oranges united. Above, it separated the spleen from the abdominal parietes, and extended below to the supra-renal capsule. It was united to the parts with which it was in contact by rather firm adhesions. It was fluctuating to the touch. The moment an incision was made, a sero-purulent, inodorous, dirty-white fluid, spirted forth. The walls of the cavity were formed of white cellulo-fibrous tissue, four or five lines thick. No other morbid appearance existed in the peritoneum*."

II.—Coma from Jaundice.

Drowsiness speedily supervening on sudden attacks of jaundice, preceded by sudden, violent, and transient pain in the epigastrium, unattended by any disturbance of the pulse, has occurred to my observation on so many occasions, that I cannot but conclude it must have been noticed by practitioners in general. I am not aware that, in connexion with these symptoms, it oftentimes proceeds to fatal coma; but I have reason to believe that it does so occasionally, and it is difficult not to conclude, that the sudden pain and its consequences arise from a mechanical obstruction in the gall ducts. In the fifth volume of the Medical Repository, a case is related which goes to corroborate some part of this view. A woman was suddenly seized with jaundice, and died the same night of coma. A calculus was found sticking in the gall duct, and the

* See also Cooke's Morgagni, vol. ii. p. 294, for cases of peritoneal abscess from injuries.

duct was ruptured. Dr. Marsh*, of Dublin, drew the attention of the profession, several years ago, to the occurrence of fatal coma, in connexion with jaundice in hepatic diseases; and Dr. Alison† remarks, that several cases, in all material respects similar to those of Dr. Marsh, had occurred to himself. In another place he observes, that these "sudden attacks of coma are not always explained by effusion within the cranium;" and that "it is uncertain whether it can be ascribed, as in the case of disease of the kidneys, to retention of matter destined to excretion." Dr. Macleod and Mr. Gilbert Burnett‡ have described two valuable cases, having a good deal of analogy with those already adverted to; and it appears to be the opinion of all these gentlemen, as well as of a more recent writer, Dr. Griffin §, of Limerick, that fatal coma in jaundice follows the *suppression* of the secretion of bile, rather than its absorption from a mechanical obstruction to the flow of it into the duodenum. Without entertaining any doubt that the suppression of the secretion of bile may prove fatal through an affection of the brain, observation has also impressed me with the belief that the absorption of bile consequent on a sudden mechanical obstruction to the flow of it through the duct, will sometimes have the same effect. How it happens that jaundice is, on many occasions, free from this serious complication, and not so in others, is a question which cannot at present be satisfactorily answered ||.

If, however, we reflect on the modifications in composition, (undefined, it is true, at present by chemical analysis,) which the biliary secretion undergoes in different states of the system, we shall cease to be surprised, probably, at the variety of the symptoms produced by its absorption; and we may reasonably conjecture that a morbid condition of the bile itself may afford one source of explanation of the supervention of fatal coma in jaundice.

That there is a foundation in nature for the opinion held in this country, (however derided that opinion may have been by some French authors), namely, that a vitiated or anormal condition of the bile is the source of very disordered states of the system, can scarcely be doubted, if we attend only to the alterations in the sensible properties of that fluid, as revealed to us in the examination of dead bodies; but we must patiently await the researches and co-operation of accomplished chemical pathologists, ere we shall be able to invest the notions in question with an exact and scientific character. Not to dwell on the observations of Stoll, Frederick Hoffman, and others*, I may be permitted to support the view now taken by the recent remarks of the sagacious and unprejudiced M. Andral. After recounting some anormal appearances of bile, and adverting to the fact that they sometimes concur with morbid alterations in the structure of the liver, and, on the other hand, are not unfrequently found in an apparently healthy condition of that organ, M. Andral proceeds:— "The knowledge of these facts appears to me to be not unimportant. They tend to elevate (so to speak) to the rank of

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* Dublin Hospital Reports, vol. iii.

† Elements of Pathology, p. 222.

‡ Lond. Med. Gaz. vol. v. page 631.

§ Dublin Medical Journal, No. xii. p. 346.—Dr. Griffin has related four cases of head affection, conjoined with jaundiced skin and conjunctiva. Two of these terminated fatally, and two recovered; and the recovery was pretty evidently due to active purgation. Dr. G. has not mentioned the existence of epigastric pain, and he has omitted to state the colour of the alvine dejections; so that whether the secretion of bile was suppressed or not in these cases, we have no sufficient evidence to shew. Morgagni recognised the *suppression* of the secretion as one cause of jaundice. He says, "The matter of the bile may accumulate in the blood, either in consequence of *some disorder of the blood itself*, or *some affection of the secretory organ*," &c.—*Cooke's Morgagni*, vol. ii. page 203.

‖ Monro, *secundus*, injected bile into the serous cavities and cellular membrane of animals, in order experimentally to ascertain its effects on the animal economy. Bile injected into the abdomen of a rabbit was fatal in two hours, without producing inflammation or jaundice. Injected into the cellular membrane of the back in a pig, it speedily produced debilitating effects, with laborious breathing, and death in four hours (Monro's Elem. of Anatomy, Edin. 1825, vol. i. page 580.) Mr. Phillips has recently prosecuted the same inquiry. "If two drachms of bile," says he, "be injected into the femoral vein of an ordinary sized dog, in a few hours he becomes jaundiced, *comatose*, and dies. If the quantity be lessened, the effects are less marked. In either case the urine and other secretions become tinged with bile." (Lond. Med. Gaz. June 1833, p. 425: On the Functions of the Liver, and Uses of the Bile.)

* Marx has collected extracts from the writings of Fred. Hoffman, Hebenstreit, Morgagni, Tronchin, and others, descriptive of the effects of vitiated or profusely-secreted bile on the alimentary canal—effects which appeared to these observers to simulate the consequences of irritant poisons internally administered.—"Die Lehre von den Giften," i. ii. 305, 307, and 400. See also Cruveilhier, in "Dict. de Med. et Chir. pratiq." viii. 322; and in particular, Portal's "Memoires sur la Nature et le Traitement de plusieurs Maladies," v. 65.

positive ideas, an opinion which, in England especially, has numerous partisans; and according to which a certain number of disorders of digestion are considered to depend on a vitiated secretion of bile." He then continues: —"On opening dead bodies we find the greatest difference in the appearance of bile in the gall bladder, which varies, with respect to colour, from a pure yellow to the deepest black, and in consistence from that of water to a very thick syrup, or even to complete solidity. There are some kinds of bile, which, placed in contact with the skin, excite a disagreeable sensation of pungent heat and pricking; and on this point we may refer to the experiments of Morgagni, who, having inserted bile collected from different bodies into the cellular tissue of various animals, found that in some cases it only acted locally as a simple foreign body, without any injurious effect from its absorption; whilst in others it produced an action eminently deleterious." — "The modifications in the qualities of the bile now adverted to, are not the only alterations of this fluid of which I have ascertained the existence in the dead body. One circumstance has especially struck me in a considerable number of *post mortem* examinations, and that is, the prodigious quantity of bile contained in the intestinal canal in certain individuals who have had during life a profuse diarrhœa. As to the liver, it presents no appreciable alteration; it is simply gorged with bile. The intestinal canal is sometimes deeply affected; the mucous membrane is inflamed, disorganized, and ulcerated: sometimes, on the contrary, it exhibits no other perceptible lesion than a slight injection of the vessels which ramify in the mucous membrane, or beneath it; so that if, in the first case, we say, with M. Broussais, that the biliary flux was consecutive to intestinal irritation, this *liaison* is by no means so clear in the second*."

I shall now detail a case in which the brain suffered from the absorption of bile, as it appears to me; and in which that absorption was determined by a sudden mechanical obstruction in the gall ducts.

Nov. 29, 1832.—Mr. L., æt. 63, had enjoyed good health during many years. For some months past he had gained flesh, having of late accustomed himself, but not immoderately, to malt liquor, in lieu of other beverage. On the evening of the 27th he was seized with a severe pain of the epigastrium, which was considered to be "spasm of the stomach," and for which he took "carminatives, opiates, and antispasmodics," with relief. Yesterday his skin was observed to be yellow, and he became drowsy. The drowsiness increased a good deal towards evening, and in the course of the night passed into decided coma. I was called to him at eight o'clock this morning, when he could be roused only with difficulty, and then imperfectly; he, however, recognized and spoke to me. He had no paralysis of the limbs; the pupils were natural; the breathing was attended by some heaving of the shoulders, but was not stertorous; the skin was yellow, the features sharp, the temperature natural; the pulse was unaccelerated, feeble, and irregular. He was free from pain of the stomach: the stools were nearly white; the urine passed involuntarily, and smelt strong. There was some muttering and tendency to delirium, for he made several rather persevering attempts to get out of bed.

After rousing him, he was enabled to swallow a large dose of calomel, and some blood was taken from the arm; but as the pulse and breathing very decidedly failed during the flow of blood, not more than six ounces were taken. The effusion of cold water, by pouring it on the head from some height, was practised, at first with the effect of rousing the sensibility; but this advantage was transient: he soon relapsed into his former state. Leeches to the temples, and a blister to the nape of the neck, were applied; subsequently carbonate of ammonia and stimuli: but no improvement took place: the pulse failed, the breathing became more laborious, and at length stertorous, and he expired during the night.

The blood drawn did not coagulate firmly. A film of serum which covered the surface was of a deep orange colour*.

As the body was not examined after death, the actual existence of gall stones was not demonstrated. I suppose them, nevertheless, to have originated the train

* Clinique Medicale, iv. 213, 217.

* See Dublin Med. Journal, No. vi. 346, for analysis of blood in jaundice.

of morbid symptoms just detailed. If this opinion is incorrect, then sudden and violent pain in the epigastrium, occurring to a person in health, quickly followed by jaundiced skin, and absence of bile in the stools, and unaccompanied by any disturbance, or at least excitement of the circulation, are not pathognomonic, as they have commonly been supposed to be, of the passage of gall stones. Gall stones may obstruct the duct, and cause jaundice, without occasioning pain, as was the case in the instance previously quoted from the Medical Repository; but can symptoms occurring in the order just enumerated, arise from any other cause? I think we have no sufficient proof of it, whilst we have abundance that they are the *ordinary* signs of sudden and transient mechanical obstruction of the ductus communis choledochus*. It is a question deserving of consideration, whether, in coma from absorption of bile, bleeding is an appropriate remedy. The case just now related was far advanced before it was practised; but in narcotism, which is a very analogous state, bleeding is injurious (if all the poison be not previously expelled), from promoting the farther absorption of the poison; and it may be presumed it would have a similar effect in the kind of case under consideration.

I have seen several cases of jaundice, illustrating the one just described, which, immediately following sudden violent and transient epigastric pain, was almost as immediately followed by drowsiness; and where, after the cessation of the pain, and the return of the due excretion of the bile, the health has been speedily re-established. Two females came under my care in 1832, in whom this succession of symptoms arose. In each there were several attacks of violent pain, the jaundice and drowsiness immediately following; and after a short time all subsided, leaving the patients, as they were before the attacks, in very good health. One of these women had, twenty years previously, an attack precisely similar, and on that occasion, as well as on the present, gall stones were found.

Another instance has very recently occurred to me, which shews the effect of absorption of bile in producing an affection of the brain. A man, æt. 43, had been ill three weeks with bad cough, dyspnœa, and copious but difficult expectoration of thick white mucus, with fever. On the 16th of February his complexion was observed to be yellow. On the 17th I saw him, when, in addition to the symptoms before named, he complained of acidity and flatulence, and frequent vomiting of food brought on by coughing. He had no pain of epigastrium; the fæces were deep yellow, and the urine orange-coloured. On the 19th he was fully jaundiced; and on that day and the following was very drowsy, the stools and urine continuing of a deep colour. A blister was applied to the chest; he was freely purged by mercurial purgatives, and took digitalis and alkalies. On the 21st the yellowness and drowsiness were much relieved, as well as the pectoral symptoms. This man had been previously healthy. In this case there was neither obstruction nor suppression of the biliary secretion. It was no doubt similar to some of the cases recorded by Andral, Abercrombie, &c. in some of which jaundice was occasioned by inflammation of the convex portion of the liver, communicated from the lungs; in others from an inflammatory affection of the mucous membrane of the stomach or duodenum*.

III.—Tremblement Metallique.

The process of gilding is carried on

* Andral, after describing the order of symptoms above enumerated, and attributing them in general to the passage of biliary calculi, asks—"Mais en est-ce là l'unique cause?" And adopting a train of reasoning which appears to me far from convincing, infers that they may sometimes arise from hepatic colic, or neuralgia of the liver. —*Clin. Médicale*, iv. 221.

* "Enfin, dans plusieurs cas d'ictère, on ne trouve aucune altération ni dans le foie, ni dans ses dépendances, ni dans les organes qui, par leur situation ou leur nature, peuvent exercer quelque influence sur l'appareil hépatique. Parmi les individus qui étaient dans ce cas, il y en avait chez lesquels la première maladie avait été l'ictère. Ils étaient devenus jaunes sans avoir éprouvé d'ailleurs aucun autre accident morbide. Cette jaunisse durait chez eux de huit jours à un mois, *lorsqu'une autre affection les entraînait tout à coup au tombeau.* Je possède trois observations de ce genre; dans la première, la mort fut le résultat d'une hémorrhagie cérébrale; dans la seconde, elle fut due à une péritonite suraiguë; et dans la troisième, la mort eut lieu subitement, sans que l'ouverture du cadavre montrât aucune lésion qui pût en rendre compte. Dans deux de ces cas, la complète décoloration des selles annonçait que pendant la vie la bile avait cessé d'arriver dans le duodénum; dans le troisième cas, les evacuations alvines avaient été constamment jaunes."—*Clin. Médicale*, iv. 220.

in this city in connexion with the watch manufacture, one of the staple trades of the place; and artisans occupied in that department, especially those who are intemperate, or inattentive to cleanliness, are sometimes affected with that weakness and involuntary jactitation of the muscles which has of late received the name of *tremblement metallique*—a designation conferred on the disease by Merat, in his *Traité de la Colique Metallique*. The late Dr. Darwall* states that artisans employed in the manipulation of mercury, or exposed to the vapours of that metal, are subject to ulcerations and other symptoms, usually ascribed to pseudo-syphilis; but I have never seen any other ill effect than the muscular tremors, nor can I learn that any other have been observed in this town.

An efficient remedy for the removal of the tremors is still a desideratum in practice. Darwall, the most recent writer on the subject, says that all means have proved equally useless in his hands; but refers to the reported successful administration of the nitrate of silver by the Chevalier Sementini. Dr. Darwall had not tried the nitrate, nor have I any experience of it; but having witnessed the beneficial effects of strychnia in some cases of paralysis, where there was reason to think the organization of the nervous centres was not considerably injured, I thought it not an improbable remedy in the muscular weakness and tremors from mercurial poison†. Every one knows that these tremors, which are sometimes irremediable, will sometimes also cease spontaneously, if the patient withdraw himself from the influence of the exciting cause. It would be wrong, therefore, to adopt the maxim, *post hoc ergo propter hoc*, with reference to any remedy in this disease, until its efficacy was established by a series of cases. In the case of Walmsley, which immediately follows, the effects of an over-dose of the alkaloid were striking and characteristic, and apparently led to speedy improvement.

Nov. 23d, 1832.— — Walmsley, æt. 31, has worked at the trade seventeen years, and was never "nervous," as the workmen express it, till now. He attributes his complaints to having worked lately in a shop which is damp and cold, and in which the draught of the chimney is not good. He has been slightly affected for six months, but the tremors have not been severe; they are now worse. On every attempt to exercise the voluntary muscles, the tremors take place. The first symptom was weakness of the fore-arms, and pain of the elbows. The pulse is rather weak; the functions all natural.

As the extract of conium had been recommended in this disease in the Gazette*, I determined to try it. He was directed to take four grains night and morning. On the 26th he had less trembling and weakness; the bowels were confined. He was ordered some common opening mixture, and to continue the conium. Nothing more was heard of him at this period.

Nov. 15th, 1833.—Walmsley applied again, having had tremors for six weeks, and taken the conium without any advantage. He had remained well since last year to the time now mentioned. After his former illness, he returned to work in a dry warm shop in an upper story, but lately has gone back to the damp colder shop on the ground floor; and to that circumstance attributes the recurrence of his complaint. He has pain of both elbows, with weakness of the fingers, and a great deal of trembling, and even jactitation, on every attempt to exercise the muscles. His head does not shake; all the functions are natural. He was directed to take a pill containing one-sixth of a grain of strychnia twice a day.

22.—Owing to some misconception, for two days he took two pills for a dose twice daily. For two hours after each dose he had violent spasms of the muscles of the extremities and body generally. I witnessed one of these attacks. He had no trismus, but the spasms were violent and general. The face was pallid; the forehead bedewed with perspiration from the exertion. The breathing was distressed, from spasm of the respiratory muscles, and a good deal of exhaustion succeeded†. The pills were altogether omitted, and to-day he is much better than before he began to take them. The pulse is weak; he re-

* Cyclop. Pract. Med. i. 152.

‡ See in particular Andral's Clinique Medicale, iv. 157; and Elliotson, in Lond. Med. Gazette, July 1833, p. 552, on the Treatment of Paralysis from the Poison of Lead, by Strychnia.

* Lond. Med. Gazette, Oct. 1832, p. 32.

† See Christison on Poisons, 2d edit. p. 753, for the physiological effects of *strychnos* on man.

sumed the use of the strychnia in the dose of a sixth of a grain twice a day.

Dec. 23d.—He has been so much better, that he did not present himself till to-day, and then in consequence of being requested to do so. He has scarcely any tremors. He continues the pills, and made the observation spontaneously, without any leading question being put to him, that he did not feel so well if he left them off.

Feb. 27th, 1834.—I met Walmsley in the street to-day; he has worked since the middle of December, and is free from tremors. The fore-arms continue weak, and he has yet pain in the elbows at times.

April 1st, 1833.—H. Sprason, æt. 39, has been employed about twenty years in gilding. The tremors came on rather more than a year since, for the first time, but in a slight degree; they are now very bad. In general, so long as he does not attempt the performance of an act of volition, the muscles are not agitated; but on attempting any motion, the tremors and jactitation occur. The head remains steady; the voice is tremulous, pretty much like that of a person during rigor*. The pulse is weak and slow; he complains of weakness, and his mind is easily agitated. He has headache and drowsiness, and his sleep is disturbed. In other respects the functions are natural. He was directed to apply leeches to the temples, a blister to the nape of the neck, and afterwards to take one-sixth of a grain of strychnia twice daily.

8th.—The headache and drowsiness are removed, and he sleeps well. The tremors continue as before. He will continue the medicine.

15th.—The tremors are sensibly less. He will continue the strychnia three times a day.

29th.—The tremors are much better. He will take one-fifth of a grain of strychnia three times a day.

May 13th.—The tremors are nearly well. A fourth of a grain to be continued twice a day.

He did not present himself again at this time.

Dec. 27th, 1833.—The tremors returned six weeks since, up to which time he had continued well since May. He has worked in the same shop with Walmsley, and like him attributes the return of his complaint to the cold damp state of the shop, and the imperfect draught of the fire. He has no headache, and the functions are all natural. A sixth of a grain of strychnia to be taken twice a day.

Jan. 3d, 1834.—The tremors are better. A fourth of a grain of the alkaloid to be taken twice a day.

10th.—Still farther improved. A third of a grain of strychnia to be taken twice a day. He returned to work three days since.

17th.—He has continued to work, and nearly well, till to-day. This morning he was suddenly seized with a paroxysm of trembling and spasms. It appears, therefore, as might have been anticipated, that the alkaloid has no power in preventing the recurrence of the complaint, if the patient be exposed to the mercurial vapours: and a circumstance mentioned by Professor Christison shews how comparatively slight an exposure to the exciting cause of the tremors may produce them even in a very aggravated form. In his work on Poisons, he instances the case of a barometer-maker (related to him by Mr. Hardinger, the celebrated mineralogist), who, from exposure to the mercurial vapours during one night only, was affected with the shaking palsy for the remainder of his life.

Sprason was again unable to work; he complained much of depression of spirits, and muscular debility; and having a large family to provide for, was unable to procure sufficient wholesome food for his support. Under these circumstances he expressed a wish to enter the Birmingham Hospital, and a ticket of admission was accordingly procured for him.

He was admitted on the 31st of January, under the care of Dr. Eccles, one of the physicians to the hospital, and lecturer on the theory and practice of medicine in the Birmingham Medical School; and I am indebted to the politeness and liberality of that gentleman for permission to annex the following statement of the treatment of Sprason's case whilst under his care. After stating that he was admitted with general tremors, and great muscular debility, Dr. Eccles, in the communication with which he has honoured me, dated March 29th, thus continues—"The treatment

* See Bateman, in Edin. Med. and Surg. Journal, vol. viii. on Tremors from Mercurial Poison.

which was adopted from his admission to March 6th, consisted of stimulants, tonics, and antispasmodics, with little or no improvement in his symptoms. He then commenced the use of strychnine, in the dose of one-eighth of a grain three times a day, which was continued for one week; but increased to double the quantity for ten days more. In the course of a few days the improvement in his disorder was sensible; and at the end of the time the tremors had disappeared, and his strength was very considerably recovered. It should be observed, that for a short part of this period electricity was employed in conjunction with the strychnine, and I think with increased benefit. The strychnine was discontinued on March 22d; and from that time the treatment has consisted in the occasional employment of electricity, and the administration of sulphate of quinine. He is now perfectly recovered."

Without affirming that the preceding cases supply conclusive evidence of the remedial efficacy of strychnia in tremors from mercurial poison, they at least afford sufficient inducement to further trials of the alkaloid, in this distressing, and frequently irremediable disease. Andral cured or relieved six cases out of nine, of palsy from the poison of lead, by strychnia; and Dr. Elliotson speaks of it as a remedy of "decided advantage" in that disease. In that species of palsy, I believe Dr. Elliotson has also found electric sparks of considerable utility.

Coventry, April 2, 1834.

ON PERFORATION

OF

STRICTURES OF THE URETHRA.

With Cases.

By R. A. Stafford,

Surgeon to the Mary-le-bone Infirmary, and formerly House Surgeon to St. Bartholomew's Hospital.

[Continued from p. 49.]

Case VI.—*Stricture in a man of 50, of seventeen or eighteen years standing, which had resisted the use of caustic and the potassa fusa, perforated.*

May, 1831.—Mr. L., æt. 50. This gentleman had suffered from stricture for at least seventeen or eighteen years. He had formerly used the common bougie; but having neglected himself, the diseased part gradually closed up, until he could make water only by drops. The stricture was situated only three inches from the orifice of the urethra; and a hardened mass, like marble, could be felt exteriorly at that part. Both the argentum nitratum and the potassa fusa had been applied from within to the stricture, but neither of these remedies had the least effect. At the present time he suffers extremely; and the urine being kept back, produces the greatest irritation and pain in the urethra. The straight lanecetted stilette was passed, the obstruction perforated, and an elastic gum catheter introduced through it into the bladder. This was changed every other day for one of a larger size. When the stricture was sufficiently dilated by these means, steel dilators were passed twice in the week, until the hardened structure composing the stricture was entirely absorbed. In a month or five weeks he left my care, perfeetly well.

Remarks.—This, as in the last case, would have required external division; but the stricture being in that part of the urethra which passes through the penis, it is extremely doubtful whether the wound made in this manner, and in a part so thinly covered, would ever have entirely healed.

Case VII.—*Three Strictures successfully perforated.*

June 1831.—Mr. C., æt. 53, gives the following history of his case:—When he was a young man, about the age of one or two and twenty, he contracted a gonorrhœa, which continued for some time, and at length ended in a gleet. From this period he observed the stream of urine gradually lessened, until it became very small. He suffered considerable pain and irritation in the urethra, and at length applied to his surgeon, who examined him, and found one stricture three inches from the orifice, and another seven inches: they gave way to dilatation by bougies, and he thought himself well. In a year or two afterwards, however, they returned. The same process was employed as before, and with the same result. They again returned, and he neglected them altogether. The urethra now became extremely irritable; the urine escaped only by drops, and he could get but little sleep: a despondeney of mind came on, and he felt he could obtain no relief, the disease having twice returned. He came to London, and placed himself under my care, rather as an experiment than thinking he should de-

rive any benefit from any treatment I might adopt. After having soothed the urethra, by taking blood from the perineum, fomenting, and administering anodynes and aperients, I examined him, and found he had an impassable stricture three inches down the canal. I passed the straight lancetted stilette, and perforated it; and two inches further on there was another stricture. This I left alone till another day, fearing that I might do an injury by doing too much at one time. I dilated the first stricture to the natural size of the canal, and then perforated the second, and discovered another in the membranous portion. This second was dilated to the natural size of the urethra. When, however, I proposed to operate on the third stricture, the patient objected. The despondency of mind had returned, and he wished to leave town, with a view of seeing how the other operations answered. He did so, and I did not hear from him for more than a year, when he called one morning, telling me that he was so satisfied with the plan of treatment neither of the strictures having returned, that he had come back to London on purpose to have the third stricture perforated. The operation was performed, and a catheter left in the bladder for a few days. Steel dilators were then introduced on the alternate days, until they passed with such facility that no one could have told that the urethra had been diseased.

Remarks.—This case is very interesting. There being three strictures, situated as they were, would have rendered the operation of their division from the perineum unadvisable. It is most probable, also, that no escharotic would have answered; besides the slowness of this method of treatment.

The return of strictures is not uncommon; indeed, by the usual treatment of this disease it generally is the case, unless bougies are constantly passed. When, however, they are divided within the urethra, they usually do not return, as in the present case, unless they are perforated by a very small instrument, and the opening made is not enlarged; they then sometimes contract to the size of the original perforation. Despondency of mind is not an infrequent symptom attendant on stricture; indeed, when the disease has been of any duration, it generally may be said to occur more or less. Such a symptom, with that of irritability of temper, cannot surprise us; at least when we take into consideration the harassing nature of the complaint.

Case VIII.—*Obliteration of nearly four inches of the Urethra successfully perforated.*

In the month of January 1832, I was requested to see a gentleman who had for several years laboured under a permanent stricture of the urethra, which at length had become totally impermeable, not only to bougies and instruments, but almost to the urine itself; and in consequence of this obstruction in the canal, ulceration had taken place behind the part, and the urine had made its escape, by fistulous passages, out at the perineum and scrotum; and through which passages alone it had passed for the last two or three years. Since this period urinary abscesses had repeatedly formed, and produced at the time considerable constitutional disturbance, rigors, pain, and feverishness. The patient, therefore, was always under the necessity of wearing poultices or cloths on the perineum, to absorb the pus and urine; and in addition to this miserable state of things, he sufffered from frequent priapism; and when semen was emitted, it of course could not be ejected out at the penis, but flowed into the fistulous passages, and thus produced the most distressing symptoms. When we examined him we found him in the following state.

A large mass, of an extremely hard structure, nearly the size of a turkey's egg, and which could be taken in the hand, occupied that part of the perineum which joins with the scrotum, so that part of the substance was in the scrotum, and part in the perineum adjoining it. Through this mass passed several fistulous passages, from whence the urine made its exit; and there were also several parts where pus had collected. On taking the substance in the hand, its connexion with the urethra could be traced, and it appeared to occupy two or three inches of the canal, the corpus spongiosum being consolidated. On examining the urethra, an impermeable stricture was present exactly two inches from the orifice, and through which not even the smallest bougie could be passed.

Mr. Kingdon and myself consulted together, and agreed that the case was of that peculiar description that none of the usual modes of treatment offered the slightest chance of relief. The application of caustic was quite out of the question, the disease occupying so great an extent of the urethra. The division of the part externally, for the same reason, could not be adopted; and the excision of the hardened mass would have destroyed so large a portion of the canal (at least four inches) that the ultimate result would have been extremely doubtful. The only treatment,

therefore, left, was the perforation of the part within the canal of the urethra; and even this operation appeared likely to be attended with uncertainty and difficulty, from the great length of urethra that was obliterated. The disease, however, being in the anterior part of the canal, and where it could be pretty well judged whether the cutting was in the right passage or not, I undertook the case. The patient consented to the operation, and fixed to have it performed a fortnight from the consultation, and requested that Mr. Copeland, of Golden-Square, might be present.

On the day appointed, Mr. Copeland, Mr. Kingdon, and myself, met; and the operation was performed in their presence. The penis was elongated with the left hand, and the straight lancetted stilette, or urethral perforator (No. 4), passed down to the stricture. When the point of the instrument rested upon the obstruction, the lancet was sharply thrust forward; and it penetrated, though not without considerable resistance, the hardened substance of the stricture. The blunt point of the instrument was then forced on into the incision made. The obstruction still existed. Another incision was made, and the blunt point was pushed on into it as before. In this manner, first making an incision, and then passing the blunt point of the instrument on into it, nearly *four inches* of the urethra was perforated. The instrument was then withdrawn, and some bleeding took place; but not more, nor even, perhaps, so much, as might have been expected from such an extent of cut surface. When the bleeding had ceased, which was in about two or three minutes, a small gum elastic catheter was passed through the perforated part into the bladder, secured, and left there. During the whole operation so little pain was given, that the patient at the time expressed his surprise. The newly-made canal was in an exact line with the natural course of the urethra; and the structure through which I cut was so hard and tough, that it required forcible incisions to penetrate it. After the operation was finished he was placed in bed, and requested to foment the perineum.

In the evening he was free from pain; he had had no rigors, which was sometimes the case when even a bougie had been passed; and the urine flowed both through and on one side of the catheter. He was ordered an opiate immediately, and a dose of castor oil the next morning. On the next day he had no pain in the urethra, he had passed a good night, and the urine flowed through and on one side of the catheter; and there were no constitutional symptoms more than would naturally occur from such an operation. The catheter was removed, and one double its size introduced.

From this time the catheter was changed to a larger size every second day; and in ten days from the operation a No. 10 catheter could be passed, and in a fortnight a No. 12. Steel dilators were now passed; and in three weeks I took leave of the patient, on condition that he would for the present pass bougies twice in the week for himself, and afterwards as often as he required them.

The fistulous passages remained open, which cannot be wondered at when we consider the great length of time they had existed, and that they formed the only channel by which the urine could be voided. The hardened mass in the perineum became gradually absorbed; but occasionally a few drops of urine passed through the fistulous passages.

On inquiring after the present state of this patient's health, (Feb. 14th, 1834, more than two years from the operation), I received the following answer:—That, although he was not quite so well as during the first six months after the operation, yet he attributed this circumstance to his own neglect, from not having persevered in introducing bougies so frequently as would be desirable. He can now, however, pass a bougie as large as a goose quill, and this he is in the habit of doing only once in six weeks. He experiences a difficulty, from this cause, to open the stricture, and afterwards the parts are generally so inflamed and swelled for about twenty-four hours, that he is obliged to pass a smaller bougie to relieve the spasm, and to assist him in making water. From the swelling and spasm, also, a few drops of the urine and semen are sometimes kept back, and give rise to the occasional formation of abscesses about the opening, which was caused by the stricture when it was in its worst state: in other respects, when the bougie has not been passed, he is very well; he makes water in a good-sized stream, and his health does not suffer.

Remarks.—The immense extent of urethra that was diseased and obliterated in this case is remarkable; and it is not the less so, that the patient was able to bear the perforation of so great a length of the canal at one operation. The pain appeared to be inconsiderable, and the bleeding trifling; both of which, no doubt, may be accounted for by the insensibility and want of vascularity of the ligamentous or cartilaginous structure which formed the disease. The hardened mass in the perineum was no

doubt formed from the urine constantly lodging in the fistulous passages, and thus producing a thickening of the parts. There were also some symptoms attending this case not common. The constant disposition to priapism can hardly be accounted for. When we consider the consolidated state of so great a length of the urethra, and of the corpus spongiosum surrounding it, we can only be surprised at this phenomenon occurring so frequently. How to explain such a symptom would be difficult, for, from the obliterated vessels of the corpus spongiosum, and the unyielding and inelastic nature of the indurated urethra, one would have imagined the contrary would have been the case; and that, when the corpora cavernosa became distended with blood, the penis would, as in chordee, have been turned downwards. It is a singular fact, that when the stricture was only touched before the operation, it frequently produced rigor: after the operation, this symptom never occurred. I have observed this in other cases; that is to say, if the stricture was only touched, rigor supervened; if, however, the bougie passed quite through it, this symptom did not happen.

From the account given by the patient, it may be observed, that, although a bougie as large as a goose quill can now be passed, yet the passage is narrowed at the perforated part. This may be accounted for by the patient, as he himself acknowledges, having neglected to pass a bougie, (doing it only once in six weeks,) as often as I requested him; and also from my having operated with so small an instrument (a No. 4.) I have observed, that when I am obliged to perforate with a very small instrument, that the stricture, if the passing of bougies be neglected, is liable to close up again to that size. I, therefore, on this account, generally enlarge the passage afterwards with a cutting instrument, which usually prevents such an occurrence. I regretted extremely that I had not the opportunity of doing it in this case; but the time this gentleman could spare to remain under my care being only three weeks, rendered it impracticable. It could be done, however, at this time, and I am convinced with considerable benefit.

The fistulous passages remaining open no doubt is owing to their passing through so diseased and indurated a structure, and their having become, which is usually the case when the urine has passed through them for a considerable time, lined by a membrane something analogous to that of the urethra. The narrowing of the stricture, likewise, keeps back a few drops of urine which lodges in them, and thus irritation arises, producing occasional abscesses. For the same reason, also, a portion of the semen is kept back, and gives rise to the same inconvenience. If the part were widened, it is more than probable that neither of these annoyances would occur.

The present state of this patient, compared with what it was previous to the operation, is even more than comparatively happy. Before, he could pass but few drops of urine through the natural channel; it mostly made its escape out at the perineum and scrotum, and got lodged in the cavities of the urinary abscesses: now, he can make water in a full stream, and the only inconveniences he suffers from are those above mentioned, and which might no doubt be obviated by treatment.

[To be continued.]

PATHOLOGY AND TREATMENT OF SCARLATINA.

To the Editor of the Medical Gazette.

Sir,

Having been led to adopt a different plan of treatment from that which is commonly relied on in scarlatina, I am induced to request your circulation of the result of my observations; and the more so that the disease which has recently spread throughout England has assumed the severe and dangerous character of the scarlatina anginosa, which prevailed about fifty years back. It appears that the scarlet fever of that period was of a nature so formidable as to excite a universal dread of its contagious influence and destructive effects. This dangerous form of scarlatina, however, as is well known, subsided into that mild form which has been appropriately named scarlatina mitior, and which, as Sydenham remarks, "is a fever so slight, as scarcely to be accounted a disease, requiring only an antiphlogistic regimen, and the absence of cold air." And for a number of years this mild

scarlatina prevailed, and has even run through whole villages, without requiring professional assistance. But it will not be doubted that the scarlatina anginosa has again made its appearance under a scarcely less formidable type than its predecessor. And it is upon this form of the disease that I now respectfully entreat the attention of my medical brethren,—under the belief that the long continuance of the milder kind, and the successful treatment of many slight cases, have thrown us off our guard, and created a prejudice in favour of doing little or nothing, whilst a severe form of scarlatina, requiring a proportionally active treatment, has been gradually taking its place.

In order to render the plan of treatment which I shall venture to recommend more easily intelligible, I shall not apologise for offering a brief description of the symptoms and progress of *scarlatina anginosa*, such as I have observed it.

The scarlatina anginosa is ushered in by pain in the limbs, prostration of strength, slight sore throat, and headache. These symptoms are followed by a burning skin and a rapid pulse, succeeded by the scarlet rash, more or less over the whole body. On or about the second night, the sore throat becomes worse; the head more affected; and on that night, or more commonly on the third, there is extreme restlessness or delirium, and there is sometimes stupor. The whole of this first stage would appear to be inflammatory in the highest degree, the danger commencing with the extreme restlessness, delirium, or stupor, or unusual short breathing. If these symptoms be not removed, the case ends fatally from the seventh to the tenth day.

The skin in this fever is hot and dry; and in many severe cases perspiration will not be produced till three weeks to a month have elapsed from the commencement of the attack. The throat and head are seriously attacked in this disease, and change of structure in vital organs, as the pharynx, trachea, brain, sometimes takes place; and such circumstance will of course be fatal. It will sometimes happen that sore throat and fever will occur in individuals exposed to the infection of scarlatina without rash on the surface of the body; such cases may be tedious and troublesome, but are seldom dangerous.

Scarlet fever sometimes becomes putrid in its character, becoming what is termed scarlatina maligna; petechiæ will appear on different parts of the body; the sore throat will be sloughy and fœtid; the lips covered with brown sordes; and the tongue dry and brown; the pulse quick and small. In such cases the termination will in general be rapidly fatal. The rash in scarlatina anginosa is sometimes general and florid, at others partial, and with little colour. The amount of rash in scarlatina anginosa does not appear to be any indication of danger: the head affection, or state of throat, or extreme difficulty of breathing, constitutes the danger.

The sudden interruption of all the secretions of the body, particularly of those of the bowels and skin; the determination of blood to the head, throat, and sometimes to the lungs; the disturbance of the brain and nervous system so quickly after the fever has commenced; the rapid circulation, the pulse often 130 in a minute—have led me to consider the scarlatina anginosa as a fever highly inflammatory.

In accordance with this view, an active depleting plan of treatment would appear to be the one indicated; and I would recommend, that as soon as this fever commences, calomel and active purgatives should be first employed; when restlessness comes on, or the slightest expression of delirium or stupor, blood should be abstracted: in adults it is of course easily obtained from the arm; in young children, from the jugular vein. Calomel and antimony in large dose should be given at night, and infusion of senna and sulphate of magnesia as an active purgative in the morning.

With infant children, calomel and jalap, or scammony and calomel, will answer the purpose. The purging should be continued actively till the feverish symptoms yield, and calomel and antimony, in small doses, as a febrifuge, should be given every four or six hours, to reduce inflammatory action. It does not appear necessary to affect the mouth by mercury, though no great evil arises if this accident occur; but the bowels should be made to act once in four, or once in six hours, until the active fever subsides.

The sore throat will require leeches for the first three to eight days, and sometimes the inflammatory affection will become indolent; and this condi-

tion of throat will receive benefit from external irritation, for which the best means is a mustard poultice. In the delirious state, leeches applied to the temples will be of use, but should not wholly be depended on. The abstraction of ten or twelve ounces of blood from the adult, between the second and the fifth day, and four to eight ounces from children, according to their age, followed immediately by active and mercurial purgatives, commonly takes off the brain affection, diminishes the severity of the sore throat, lessens the pungent heat of skin, lowers the rapid pulse, and puts the patient in a state of safety about the fifth to the eighth day.

A hot dry skin will continue sometimes long after this period, and will require sponging with tepid vinegar and water, and, after a time, the use of the warm-bath.

When symptoms of debility come on, after the active fever is subdued, small doses of ammonia will be useful; and when all heat of skin has ceased, bark may be safely employed.

Having ventured to recommend so active a treatment in scarlatina anginosa to the adoption of the medical practitioner, it may be necessary for me to state that the grounds upon which I have founded this recommendation, is the successful termination of 300 cases, during a practice of twenty years. The treatment was, of course, more or less active, according to the symptoms of inflammatory action; but in no one case where I resorted to it, had I occasion to repent the use of the lancet, but in several, where bleeding by leeches only was employed, the protracted period of fever, and the after ill consequences—as abscess in the ear, suppurating glands, long-continued debility—made me regret that I had not taken blood from a vein. In sixty severe and dangerous cases of scarlatina anginosa, in which blood was taken from a vein, delirium and stupor subsided after the bleeding; a second venesection was in no case required, and the recovery was rapid in all. To illustrate more particularly the treatment I have described, or rather, indeed, more forcibly to impress upon the public how thoroughly inflammatory this form of the scarlet fever is, I will now detail a few cases.

Case I.—A boy of seven years of age, was the eighth in succession attacked by scarlet fever, and it had been severe in most of the previous cases. On the second night he was in a highly-excited state of delirium, and could with difficulty be kept in bed. His pulse was 140; his head hot; his skin dry; his tongue parched. Eight ounces of blood were taken from his arm, and five grains of calomel given, with four of antimony. The delirium soon ceased, and in two hours he fell asleep. In the morning his head was perfectly relieved; he was sensible, and cheerful. In the night he had pulled off his bandage from the arm, and bled, before it was discovered, at least twelve ounces more. His bowels had acted freely; his throat was considerably better; he recovered rapidly; and on the fifth day was up and dressed, and wished to be allowed to walk.

Case II.—A slender boy, of thirteen years, who had outgrown his strength, was attacked by scarlatina anginosa. On the morning of the third day delirium commenced; twelve leeches were applied to his temples and throat, and he was freely purged. At night he was more restless, and became delirious. Ten ounces of blood were taken from his arm; he obtained sleep soon after; on the following morning he was sensible; his throat better: the purging was continued, and on the fifth day he was convalescent.

Case III.—A boy, nine years of age, had been treated by gentle and continued purging, and by leeches to the throat, gruel and water for diet, up to the eighth day, without any remarkable symptom, but with more or less fever during that time. On the eighth night he had a dry hot skin, a brown dry tongue, a quick small pulse, with a *slight jerk* in it; still, as I thought, denoting inflammation not thoroughly put out. He was at one time stupid, at another delirious; his throat was coated with soft lymph, which fringed the fauces and covered the larynx. The child breathed with difficulty, and instinctively tried to throw up the flakes of lymph which obstructed his breathing. He could not swallow.

Eight ounces of blood were taken from his arm; in an hour his delirium and restlessness subsided; his breathing became more easy; he slept considerably. On the following (the ninth) morning, he was sensible when he awoke; his throat had thrown off large masses of lymph; the boy's energies

seemed restored, and his fingers were continually passed down his throat, to draw away the flakes which obstructed his swallowing. He had no more fever, and recovered rapidly.

Case IV.—A plethoric child, aged three years, after active purging, on the third day became extremely hot and restless; at night stupor came on, with short breathing. The state of the throat was not unusually severe. Four ounces of blood were taken from the jugular vein. The restlessness and heat of skin quickly subsided; four grains of calomel, with four of jalap, were got down; the child was freely purged during the night, and in the morning was relieved, and out of danger.

Case V.—A slender child of four years, on the fourth night, under circumstances very similar, but with worse throat, was treated by six ounces of blood from the jugular vein, followed by a calomel and scammony purgative, with the same result.

It might, at first thought, be supposed that such active remedies must be followed by extreme debility: it will be generally (I had almost said invariably) otherwise; and if debility be the consequence of the high inflammatory action, we can scarcely doubt that the debility will be obviated, or lessened, by diminishing the degree and shortening the duration of that inflammatory action. In accordance with this general remark, I have found that all the cases which were, during the first four days, treated actively, were the soonest well, and the least affected by after ill-consequences—as glandular disturbance, abscess, &c.; and were the least weakened as to their muscular power. In fine, I have no hesitation in saying that the arrest of the inflammatory fever is the arrest of debility, and hence the recommencement of strength.

It will be seen, then, that I cannot approve of that mild antiphlogistic plan, consisting in little more than saline draughts and barley-water, too often employed in scarlatina anginosa; and that my experience leaves me no choice in recommending an active depleting plan of treatment.

It is scarcely necessary for me to observe that I have here written solely on the form of scarlatina termed anginosa, not maligna. I may add also, that, in 300 cases, I have not met with one of the latter; and this circumstance leads me to suspect that the scarlatina maligna is sometimes only the putrid termination of the anginosa, unchecked, or not sufficiently depleted, in its inflammatory stage. It is, however, perfectly possible that particular localities (as unwholesome air, or a marshy district) may give a malignant character to scarlet fever. To this form of the disease my observations are not intended to apply; but distinctly to that species of scarlatina anginosa which has been so prevalent during the year 1833. It is right also to state, that the situation of my practice has been ten miles to the north of London, in a bracing and invigorating atmosphere.

The profession will, of course, understand that I do not recommend bleeding in *all* cases of scarlatina; but in those that are severe, attended with intense heat of skin and rapid pulse, or the *slightest* brain disturbance;—and the time for bleeding between the second and the fifth day: but should it have been neglected beyond the fifth, and the patient be under stupor or delirium, or extreme short breathing, the lancet would still offer the best chance of recovery, as illustrated by Case the third. Purging from the *very commencement* of scarlet fever is indispensible; and after bleeding, the effect of a purgative relieves in a much higher degree than before: and for this opinion we have the highest ancient and modern authorities. I had the misfortune to witness a case where purgatives failed in operating upon a child for the first forty-eight hours, and no after-treatment could recover that child: it died after five days' struggle.

Some objection to the abstraction of blood may arise, at least with the public, from an apprehension that it would interrupt the continuance of the rash: bleeding and purging both promote that continuance; and if the eruption be previously checked, it will appear more fully after either.

Sponging with tepid vinegar and water is useful in relieving the pungent heat of skin, which always, more or less, attends the scarlatina anginosa; yet, where there is head affection, this should not be depended on as a *curative means*. Indeed, any benefit from the skin is little to be relied on; we not only cannot obtain perspiration during the inflammatory stage, but, in many instances, several weeks pass over before

sweating, or even a soft state of skin, can be procured: all we must look to obtain is lessened heat; and long experience has taught me that that object is best accomplished by active early depletion.

It is, no doubt, a matter of regret, that the practitioner may often find a difficulty in procuring the concurrence of friends and parents in this plan of treatment; and we have only to hope that its advantages may be as fully appreciated as those of a similarly active treatment in measles, and we do not question that experience will produce this beneficial result.—I am, sir,

Your obedient humble servant,

W. Hammond, Jun.

Whetstone, March 24, 1834.

REPLY TO MR. THURNAM'S OBSERVATIONS ON THE IRIS, &c.

To the Editor of the Medical Gazette.

Sir,

I entirely agree with your correspondent, Mr. Thurnam, as respects his proposition, of the propriety of individuals being well acquainted with the anatomy, before they presume to conjecture about the physiology, of parts. Perhaps he will not disagree with me, when I affirm that the best anatomists have not always proved the ablest physiologists, and that for a very obvious reason; viz. that the order of talent necessary for the one is not always combined with the other. It is the same with those who have prosecuted experimental physiology:—it is one thing to make experiments, and a very different one to reason correctly on the phenomena attending living dissection. I venture on these remarks, not in the spirit of speculation merely, but as bearing on the question at present before us; and for their truth I need but refer to the well-known experiments of Magendie, Bell, and Mayo; particularly of the first—the extravagance of some of whose deductions are perfectly astounding.

With respect to the ophthalmic ganglion, your correspondent thinks that it is not formed by a union of the third and fifth nerves, but that it is simply connected with them. Now whichever way we look at it we cannot escape the conclusion that it thus connects the iris with the palpebra. The fact that this ganglion is wanting, in those animals which are destitute of eyelids and a moveable iris, seems strongly to confirm the view I have taken of it,—that it connects the eyelids with the iris: the similarity of actions and structure of both are too striking to admit of a doubt that the former are the result of this anatomical relation. Your correspondent has not informed us of any purpose there could be in its connexion with these nerves, if it really be not for that I have suggested. Why else is there this union? A warm advocate of Sir Charles Bell's will not tell us that it is accident: Sir Charles says, "there is no accident in an animal body." Indeed, if the ganglion be not for the combining together of the various parts to which the nerves connected with them go, it is difficult to conceive what office they possess in the animal economy. If the ganglion in question be not formed by the third and fifth nerves, I would ask how it happens that injury or disease of either of these nerves should be attended with paralysis of the iris? The difficulty of answering this, otherwise than in the manner I have suggested, proves the impropriety of relying exclusively upon anatomy. The fact that belladonna, applied to the branches of the ophthalmic division of the fifth, also paralyses the iris, is a striking confirmation of this view. If the retina and iris had merely to be brought to sympathize with each other, what occasion for the ophthalmic ganglion to be joined with the branches of the third and fifth? It would but have required that a communication were established between the two former; and yet but one author is adduced in support of this view, and he, I believe, does not pretend to have traced filaments from the ciliary nerves to the retina, except in one or two animals, in whose eyes, be it observed, the arrangement of the ciliary nerves is often very different from that in the human eye, inasmuch as some have two ophthalmic ganglia, and others have none at all. And mark the contradiction of those who say that the ciliary nerves, supposed to be in the retina, convey the stimulus of light to the iris, when they will not allow the ciliary nerves in the iris to have the same properties;—as if the ciliary nerves, in the iris, could not be stimu-

lated by light, as well as those presumed to be in the retina!

Your correspondent has carefully abstained from entering into the various arguments I have adduced in support of my propositions. In so doing he may have acted wisely, if not justly. He has thought proper to confine himself to the anatomy of the nerves. He has not been able to disprove that there is an anatomical relation between the nerves of the iris and those of the palpebra; nor has he proved, although he has been lately dissecting these nerves, that there is such relation between the iris and retina.

"But the most objectionable part of this (my) theory is the idea of the branches of the fifth nerve, which supply the orbit and forehead, being compound nerves, ministering to *motion* as well as sensation." Why is this the most objectionable part? Because it is the least accordant with facts and probability? No: but because it does not agree with the theory of Sir Charles Bell, and others. Sir Charles Bell maintains two theories; the one, that the muscles used in respiration and expression are supplied with two sets of nerves, viz. a set for their respiratory motions, and another set for their voluntary motions: as respects the muscles of the face and forehead these nerves appear to be the fifth and portio dura. The other theory is, that the fifth nerves, coming off from different portions of the prolongation of the spinal column in the train, partake of two different properties—viz. sensation and motion; whilst it appears that branches from the former go simply to the integuments and muscles of the face, forehead, &c.; and that branches from the latter go only to the muscles of the jaw. Whether these branches can be said to pass, from their origins in the brain, to their final destination, without intercommunicating, is what I do not pretend to settle; but I believe it is considered that they do not. Certainly the experiments of Sir Charles Bell prove that the muscles of the face are paralysed by division of the branches of the fifth, as certainly as those of Magendie prove the same with respect to those of the eyelids and iris. If this theory (which is opposed to the other) cannot be made to accord with facts, the inference is, that the theory must be made to suit the facts, and not the latter distorted to agree with the former. But I have already said so much on this point that I do not think myself justified in repeating it: I have not trusted simply to the anatomy, as your correspondent has done, but have also reasoned from the physiology and pathology.

It thus appears to be proved, by reference to the experiments of Bell, Magendie, and Mayo, that injury of the third and fifth nerves paralyses the iris, and consequently the ophthalmic ganglion and ciliary nerves; and that injury of the branches of the fifth paralyses the voluntary motion of the muscles to which they go, whilst their respiratory or involuntary motions are paralysed by division of the branches of the portio dura.

Having thus, as I conceive, fairly surmounted the "insuperable objections" of your correspondent, I have nothing further to add, but leave it with your readers to decide which of these views is the most correct; and whether he is the more likely to have arrived at a proper conclusion, who has looked at the facts of anatomy, human and comparative, the deductions of physiology, and the phenomena of disease, or he who simply refers to a portion of the anatomy.

I have the honour to be, sir,

Your very obedient servant,

JOHN WALKER,

Assistant-Surgeon to the Manchester Eye Institution, &c.

Manchester, April 3, 1834.

RESPIRATION OF NEW-BORN CHILDREN.

To the Editor of the Medical Gazette.

SIR,

CONSIDERING how much attention is at present bestowed upon the science of medical jurisprudence, and the unsatisfactory nature of the evidence that the medical practitioner frequently finds himself obliged to give upon some very important questions connected with it, I presume that contributions on the subject will be acceptable to your readers. There is no greater desideratum, perhaps, in the whole science, than the means of ascertaining with certainty whether a child found dead has ever breathed. If the following appear to you to afford a sa-

tisfactory test for this purpose, I hope it will be deemed worthy of a place in your instructive journal.—I am, sir,

Your obedient servant,

JOHN C. BLOXAM.

Newport, I. W.
24th March, 1834.

The test which I propose, is founded upon the peculiar nature of respiration, and of the organs by which this function is performed. When an opening is made through the parietes of the chest, the lungs, it is well known, collapse, and expel a quantity of air from their interior. This phenomenon results from the circumstance, that the lungs are made to fill the cavity wherein they are contained, by the admission of air into their interior through the trachea; their natural or original bulk being considerably smaller than the cavity, after inspiration has been performed. The dimensions of the cavity of the chest having been enlarged by the contraction of the muscles of respiration, and the lungs being thereby drawn into a state of extension, the latter organs remain afterwards in a state of action; an effort to contract to their original bulk is constantly kept up, in consequence of the elastic nature of those tubes into which the atmosphere finds its way; and this contraction actually takes place the moment the parietes of the chest are rendered pervious to air, so as to allow the atmosphere to enter immediately the cavity of the chest itself. This elastic action of the air-tubes is a provision of great importance, as it not only prevents the blood-vessels in the lungs from suffering compression during expiration, as must have happened had they remained mere passive agents during this part of the process of respiration, but it holds these vessels in a state of expansion, whereby at the first inspiration the blood is drawn into a new channel, and is afterwards prevented from returning to its original or fœtal course.

The condition of all these parts is entirely different when the lungs have been inflated by any other means than by natural inspiration. If the air, instead of being drawn into the lungs to occupy the space created by the previous enlargement of the chest and consequent distention of the lungs, be either generated in their substance or forced into the natural tubes by any artificial means, the lungs must be forced against the parietes by which they are enclosed, so as to distend the chest, instead of making a constant effort at separation, and drawing the sides of the chest together; the blood-vessels, instead of being expanded, and thereby filled with blood, must be compressed, and their natural injection obstructed, if not entirely prevented;—a fact of some importance to the obstetrician, though not hitherto noticed. Since the lungs in this case keep up a distending pressure upon the parietes, instead of drawing them together, of course the parietes exert no influence to prevent the lungs from contracting, and an opening made through the parietes consequently cannot give rise to that contraction of the lungs, and expulsion of air, which takes place in the other case.

The foregoing facts, then, furnish a means for deciding, in every case, whether respiration has been performed. If, upon making an opening into the chest, so as to allow the pressure of the atmosphere to act upon the surface of the lungs, those organs collapse, and expel a portion of the air contained in them, such air must have been introduced by the action of natural respiration. If, on the contrary, upon making such an opening, this phenomenon does not occur, any air which the lungs may contain must have been introduced by some other means than by inspiration.

The best method of putting this test into practice is to put the head of the infant under water at the time that an incision is made through the sides of the chest. The advantages of this proceeding are,—first, that the air which is expelled from the lungs may then be seen rising through the water, concerning the occurrence of which there could not be any difference of opinion; secondly, if a child were still-born, and air blown into its lungs from the mouth of another person, there would be no expulsion of air upon the chest being opened; but when the body became cold, the air would condense, and possibly give rise to some partial contraction of the lungs. The front of the right side of the chest would be the most eligible part for making the incision.

It may be worth observing, that, as a few small bubbles of air are liable to escape from the nostrils, upon putting the head under water, the practitioner would do well to ascertain that no air remained in the nostrils before he proceeded to open the chest.

HÆMORRHAGE FROM THE BOWELS.

To the Editor of the Medical Gazette.

SIR,

I ENCLOSE you a case of serious hæmorrhage from the bowels, for insertion in the London Medical Gazette, if thought of sufficient interest.—I remain, sir,

Your obedient servant,

R. R. ROBINSON,
Surgeon to the London Dispensary.

Cooper's-Row, April 18, 1834.

Extensive and long-continued Hæmorrhage from the Rectum—Anæmia—Prolapsus Recti—Retention of Urine—Cholera symptoms—Death.

June 21st, 1829.—R. A., æt. 59, a very respectable, temperate man, enjoyed good health till last October, since when he suffered from continual bleeding from the bowels; at first he had some diarrhœa, tormina, tenesmus, and slime, but latterly pure blood, passes, which is sometimes dark, at others light coloured; sometimes clotted, at others fluid; occasionally to the amount of nearly a pint at a time, but generally only an ounce or two; it usually flows immediately after a motion. He now feels the loss of blood greatly: his appetite falls off; he is very weak, and much emaciated. Various means have been employed, among which may be mentioned venesection, leeches, blisters, opium, mercury, digitalis, antimony, iron, sulphate of copper, lead (which in the solid form produced griping, and was soon omitted), infusion of roses, catechu, alum, oak-bark, and a variety of astringent injections. The bleeding has been from time to time checked for a few weeks, but again breaks out.

℞. Plumb. Superacet. gr. i.; Acid. Acetic. distill.; Tr. Opii aa gtt. x.; Aq. Puræ ℥j.; M. fiat haustus ter die sumendus.

Aug. 20th.—The bleeding has continued more or less, with intervals of a week and a fortnight, since last report. The lead in solution seemed at first to restrain the hæmorrhage, but destroying his appetite, griping, and appearing to weaken him, it was discontinued. He has since tried logwood, without benefit, and for the last fortnight has taken the confection of roses in milk. No bleeding for a week; countenance very pale; pulse regular, vibratory, and decidedly hæmorrhagic. Tongue clean and moist: feels stonger.

Repetatur Confectio Rosarum c. Quin. Sulph. gr. i. ex Inf. Rosarum ter die.

Dec. 30th.—Freer from bleeding lately; looks and feels better. Has kept an account, and finds that from November 16th, 1828, to this day, he has lost blood 241 times.

Aug. 19th, 1830.—Pretty well till the 10th instant, since which the hæmorrhage has returned almost daily, always immediately after a motion: it comes out now in a stream, has a bright florid colour, and amounts to a pint or more at a time. Extremely reduced; countenance very exsanguine, of leaden hue; skin at other parts quite white. Tongue also very pale; a violent beating at the top of the head; pulse vibratory; no pain; a slight hæmorrhoidal fringe round the anus; mucous membrane of rectum relaxed; no internal pile or varicose vein discoverable; gut capacious. Mr. Howship was consulted, and proposed to include the external piles in a ligature, so as to produce surrounding inflammation, which he hoped would terminate in the effusion of coagulable lymph, and obliteration of the cavity of the hæmorrhoidal veins higher up in the rectum, from whence the bleeding, in his opinion, proceeds. The operation was delayed, from his extreme debility. The horizontal position, with the legs semiflexed; inject cold water into rectum every morning; infusion of roses *ad libitum*.

August 22d.—Stronger; no bleeding; a curved needle, into whose head a curved silver pin was fitted, was passed through the pile on each side of the rectum; the pins were left, and the piles tied around them with strong waxed ligatures.

23d.—Considerable pain in rectum; no bleeding; no motion; great dysuria, but bladder eventually emptied; pulse 90, vibratory.

Pil. Opii. gr. i. h. s.

24th.—Tumor ash-coloured; considerably less pain; some irritation of bladder still; no motion; thirst.

26th.—Tumors included in the ligatures sphacelated and discharging pus; pain greatly subsided; frequent and painful micturition; no bleeding; no motion; pulse vibratory, 100; some fever; pins removed.

Catap. panis, haustus aperiens statim; enema commune; light diet.

29th. — No bleeding; bowels well opened by medicine; pulse less vibratory.

Sept. 12th.—Countenance still sallow; feeble, yet stronger; piles more prominent; anus œdematous; a slight tinge of blood after each motion.

Oct. 27th.—Stronger; less beating in head; still a little bleeding.

May 5th, 1833.—Bleeding has continued, more or less at intervals, since last report: for the last several months a little blood after motion almost daily; exceedingly exhausted; cannot walk without assistance; a perfect specimen of anæmia.

Quin. sulph. gr. i.; Acid. sulph. dilut. ℥x.; Tr. Gent. co. ℥i. ter die ex aquâ.

10th.— Prolapsus of rectum the last few days, with retention of urine, requiring the introduction of the catheter twice a-day.

Rep. med. Injectio cort. quercus.

21st.—Gut not so much down; has passed his urine without the instrument the two last days; no bleeding; so feeble that he can scarcely turn in bed; slight œdema of legs, and sense of fluctuation in abdomen.

℞. Ferri muriat. m. xv. ter die.

June 28th.—Much benefited by the iron; feels stronger; no bleeding; gut entirely returned; pain in head subsided; is able to get up, and has been into his garden; has been several times purged to-day.

Tr. Catechu, ℥i.; Tr. Opii, m. v. ex aq. Cinnam. ter die.

July 5th.—Better the day before yesterday; purging returned excessively yesterday, and has continued to-day; motions passed with very great violence very frequently; have all the appearance of rice water; nothing fæcal; constant vomiting; no cramp; skin much shrivelled and cold; voice only in a whisper; has micturated freely to-day; features much altered; tongue pale and moist.

Mist. effervescens, 4ta quaque horâ.
Cataplasma sinap. epigastrio.

6th. — Refreshed by the mixture, which staid down; weaker; faint; restless; very uneasy, but in no actual pain; twelve motions (which nearly filled the chamber-pot) of green colour; they seem composed of offensive slimy mucus; no cramp; pulse 96, regular; tongue clean; skin cold; in doubt whether he has made water to-day or not.

7th.—Died at 9 p.m. yesterday.

Dissection.—Lungs unadherent, and perfectly healthy; heart large; coronary arteries thickened, but not ossified; ℥i. of serous fluid in left side of thorax; liver large and fragile; gall-bladder distended with green bile; spleen and pancreas and stomach healthy; small intestines remarkably flaccid, diaphanous, empty, and pale; a diverticulum in ileum; mucous membrane of cœcum red; descending colon contracted in the centre, but became as capacious as any part on the least pressure. The parts above, however, as far as the cœcum, distended partly by air and partly by a thin viscid yellow fluid. Rectum, examined with great care, was perfectly healthy: it is in my museum, and open to inspection. Kidneys large, congested, and somewhat hypertrophied. Bladder contracted, and quite empty.

Remarks.—This case presents some points of interest, to which I particularly wish to advert. In the first place, the long-continued hæmorrhage, in spite of every thing tried, is a peculiar feature, and one which I do not remember ever to have seen where no dysentery or organic disease would account for it. It is a good specimen of hæmorrhage from transudation, to which so much attention has been paid of late.

2dly, It is a beautiful specimen of anæmia, all the characteristics of which it clearly possessed, particularly the strong and annoying beating at the top of the head, to which Dr. Marshall Hall has more especially alluded.

3dly, It is interesting, as shewing that the operation of exciting inflammation in the neighbourhood of the veins, the source of hæmorrhage in a constitution the most unfavourable, was not without some good effect—a plan which has of late succeeded so well in the treatment of varicose veins of the leg, under Mr. Mayo.

4thly, It shews the good effects of steel in anæmia; for the constitution was materially improved by it. From being unable to turn in bed, he was able to get up and walk; he seemed, indeed, fast getting well, till the fatal diarrhœa supervened.

Lastly, The diarrhœa itself is worthy of consideration, as it is apparently an illustration of a remark made by Andral, —namely, that a metastasis may take place of fluid from the serous to the mucous membrane of the bowels, and be

attended with some of the symptoms of cholera spasmodica.

MEDICAL REFORM.

SUGGESTIONS FOR THE PARLIAMENTARY COMMITTEE.

To the Editor of the Medical Gazette.

SIR,

THE subject of medical reform has lately occupied so much of the attention of the profession, that it may appear superfluous to address you on so hackneyed a theme. At the same time, the subject is one of so much interest and importance to the profession and to the public, and the present time is so peculiarly appropriate for the consideration of all matters relating to it, that I deem it incumbent on all who have reflected on the consequences that may probably result from the report of the parliamentary committee now inquiring into the state of the profession, to offer such suggestions as may tend to simplify and diminish the labours of the committee, and to palliate or remove the grievances of the present condition of the profession. I am therefore induced to request that you will insert the following remarks in an early number of the Gazette.

It is not my intention to enter into a history of the origin of the Colleges of Physicians and Surgeons and of the Apothecaries' Company, nor to give an abstract of their by-laws: these matters have been so frequently and fully stated of late, that they must be familiar to all your readers. I shall therefore limit my remarks to the circumstances with which the members of the profession in general are most dissatisfied, and to the alterations which are best calculated to promote the respectability of the profession and the welfare of the community.

The chief matters of complaint with the great body of physicians in this country are, that they are precluded from the highest honours of the profession, and from all share in the government of the London College of Physicians, unless they have been educated at one of the English universities, where the means of acquiring medical knowledge are very imperfect; and that they cannot even become licentiates of the London College without undergoing an examination, although they may have obtained their degrees at an university where these honours cannot be acquired without a rigid examination, and the possession of ample qualifications.

To remedy these grievances, it is essential that government enforce uniformity of system in all the universities in the British dominions. If universities be permitted to form their own arrangements, some will be induced to offer greater facilities to students, and require a shorter residence, and make their examinations less rigid than others. Individuals will always be found eager to avail themselves of these advantages, and there will exist a perpetual source of jealousy and dissatisfaction amongst those physicians who have been properly educated, and have fairly earned the honours which they have received. Unless, therefore, the committee strongly recommend, and, indeed, insist on this great object, all their other measures will be futile and of no avail; but if they can prevent universities from conferring degrees, except after a residence of a certain duration, and a strict examination into the qualifications of the candidates, they will bestow a boon on the profession and the public, and very much simplify the rest of their own arrangements.

If this preliminary measure be secured, all physicians who have graduated in England, Scotland, or Ireland, should become members of the College of Physicians in London without any examination, but merely on entering their names; and this they should be required to do if they settle in England. A similar regulation should prevail in Scotland and Ireland in regard to English physicians practising in those countries.

In addition to the universities already established, it is very desirable that there should be a power in London of conferring medical degrees; not that this privilege should be granted to the London University, or to any other medical establishment exclusively, but that one university should exist in London for the purpose of giving medical degrees to those who have properly used the means of acquiring professional knowledge, which the great hospitals and able lectures in the metropolis afford.

The advantages, indeed, of instituting a medical university in London have recently been so clearly and forcibly stated in the Medical Gazette, that it is un-

neeessary to make any addition to what has beeu so ably said by the learned editor on this subjeet.

In respeet to the government of the London College of Physieians, I would propose that the council be elected for life by the members at large; and to prevent inexperieneed persons from being appointed, no one should be eligible to this station until he had been ten years a member of the College.

The medieal publie bodies have been exposed to much obloquy on account of their system of self-election of the governing power, and of their permanent retention of offiee. I am, however, of opinion, that the eouncil eannot preserve their independenee if they are liable to frequent re-eleetion and although I do not mean to defend the mode of self-eleetion, I am persuaded that the plan of universal and indiseriminate suffrage is not so pure and perfect as to be free from objeetion. To palliate, however, as far as possible, the evils of both systems, I would advise that no proxies be allowed, and that members exercise their privilege of voting only by personal attendance at the time of eleetion.

Whatever changes may be proposed in the laws relating to physicians, difficulties will oecur in regard to the existing race of practitioners. Liberality should be shewn to them, as the evils which may arise from excess of liberality will be only temporary, and will eease with the present generation.

All who were actually in practice as physicians at a eertain date should be exempted from the penalties of the new laws, as was done in regard to the apothecaries, in 1815. I think however, that those physicians who resided at an university, and passed an examination before taking their degrees, should, if they expressed sueh desire, beeome members of the London College, without further examination; but that those who have obtained their diplomas merely from testimonials, should not be allowed this privilege, without due inquiry into their qualifieations.

It ean hardly be expeeted that these proposed changes will meet with the cordial eoncurrence of the Fellows of the London College. Men are naturally very reluctant to relinquish privileges which they have long enjoyed; but all communities must yield to the force of circumstances; and it is evident, that, in the present state of publie feeling, this learned body eannot longer retain, exclusively, powers whieh were eonferred upon them, and perhaps not improperly, in former times, and under eircumstanees very different from those which prevail in the present day.

I would, however, deprecate the offensive and intemperate language which has too often been addressed to the Fellows of the College, in diseussing the subjeet of medieal reform. Such personalities must injure any eause, and are almost always unjust, as well as illiberal.

It ought to be remembered, that, both in former ages, and in the present day, the Fellows of the College have not only contributed to the advancement of medical knowledge by their scienee and their learning, but that they have enhanced the respectability of the profession, by the intercourse which their edueation at the English universities has given them with the nobles and gentry of the land; the whole profession having, in some degree, participated in those advantages.

It may be thought by some, that if the Fellows of the College lose their exclusive privileges, medical students will cease to attend the English universities. I think it would be a great misfortune to the profession, were this to happen: I have, however, no fear that such would be the result. The opportunities whieh those plaees afford of aequiring literary and classieal knowledge, the high eonnexions which may be formed, and the eclat of having been educated at Oxford or Cambridge, together with the prospeet of ultimately obtaining their Fellowships or Professorships, will always be suffieient indueements, with a eertain number of medical students, to procure degrees at those ancient and celebrated seats of learning.

It is true that extensive praetieal knowledge of medicine eannot be aequired either at Oxford or Cambridge; but this deficiency can be supplied in London or Edinburgh, where, indeed, the generality of English university students already resort for the advantages of clinieal leetures and hospital practiee.

The eireumstanees that chiefly require the attention of the Parliamentary Committee, in the departments of the surgeons and the apothecaries, are, the con-

fusion and perplexity which prevail, in consequence of there being two governing bodies over those branches; and the means that should be adopted to ensure efficient and cheap medical attendance for the lower classes of society.

A very large majority of the members of the College of Surgeons practise pharmacy; and nearly, if not quite, all the members of the Apothecaries' Company practise surgery: these practitioners should, therefore, be examined and governed by one board. The College of Surgeons ought to have possessed themselves of the power which was granted to the Apothecaries' Company in 1815. It is useless now to inquire into the cause of their apathy on that occasion: the only way to rectify the mistake is, either to deprive the Apothecaries' Company of the power of examining candidates, or to unite their examiners with the council of the College of Surgeons.

The Apothecaries' Company might retain their privileges as a trading company, but they should not be permitted to exercise any control over the education and qualifications of students, except in conjunction with the College of Surgeons.

All the members of the College of Surgeons ought to be examined in medicine, chemistry, pharmacy, and midwifery, as well as on anatomy and surgery. A few general practitioners should, therefore, be added to the council; and they might, with great propriety, be selected from the examiners of the Apothecaries' Company.

If this union be established, it will be necessary to alter the regulation requiring general practitioners to have served an apprenticeship; as surgeons can much better qualify themselves by devoting only a few months to pharmacy, and by a longer attendance at hospitals and lectures, than by a servitude, in which they seldom learn any thing, except the art of mixing medicines. If, therefore, apprenticeships be not abolished, they ought not to be compulsory.

The medical wants of all ranks can only be supplied by having two classes of members of the College of Surgeons. If equality of qualification be demanded from candidates, the required standard must either be so high as to preclude proper persons from settling in places where the compensation can never be sufficient to repay the expense of their education, or so low as to impede the progress of surgical knowledge, and diminish the respectability of the profession.

The first-class members of the College of Surgeons should be required to possess ample general, as well as professional knowledge; to have devoted a longer time to the attendance upon lectures and of hospitals, and to undergo more rigid, as well as more frequent examination, than would be demanded from the other division.

They alone should be eligible to be of the council; to be lecturers and teachers; to serve as medical officers in the army and navy; to be surgeons to prisons; and to be the attendants of lunatic asylums: governors of hospitals and dispensaries should also be recommended to appoint their surgeons exclusively from this class.

The second-class members of the College should possess sufficient professional knowledge to enable them to treat ordinary cases of disease; but a lower rate of qualification, and a shorter attendance at lectures and hospitals, should be required from them, than from those of the first class.

If a second-class member wished to join the first class, a sufficient additional attendance at lectures and hospitals, to complete the curriculum established for the first class, should be required from him; and he should undergo fresh examinations.

Parish attendances should be open to members of both classes. There is, however, no part of medical polity that requires more ample revision, or that is more worthy of the serious consideration of the committee, than that which concerns the medical attendance upon paupers; but the subject is so intimately connected with matters relating to the Poor Laws in general, that it can hardly be discussed separately; and it would exceed the limits which I have assigned to myself, to enter upon it on the present occasion.

The Council of the College of Surgeons should be appointed for life, by the members of both classes, from those of the first class, of ten years' standing. Members should be allowed to vote only by personal attendance at the time of election.

It would be invidious to make any distinction amongst the present members

of the College. They should all, therefore, be placed in the first class; but those who belong to the Apothecaries' Company only should be second-class members, unless they qualified for the higher division.

A great distinction has been made by some, between those surgeons who do not dispense medicines, and those who practise every branch; but as these matters are regulated by personal considerations, and are not necessarily connected with professional qualifications, they should be left to the choice of individuals, and not be subjects of legislative enactment.

The surgeons in country places must supply their patients with medicines, or they will get no employment. In large towns, too, they will not, at first, be able to maintain themselves by their professional labours, unless they practise pharmacy. The "fortunate youths" who have private fortunes, may settle at once as prescribing surgeons, immediate professional income not being an object of importance to them; but those who have only their talents and their industry to depend upon, must be general practitioners. All members ought, therefore, to be examined in every branch of professional knowledge, and no one should either acquire or lose privileges, by the selection which he may afterwards make in his branch of practice.

There is a by-law of the College of Surgeons which ought to be revised; I mean that which compels a member to pay a large sum for permission to withdraw his name from the list of the College. If an individual wishes to relinquish the profession altogether, there can be no good reason why he should not be allowed to do so, without paying for his resignation.

The Edinburgh physicians have lately made great efforts to obtain the privilege of settling in England as general practitioners, without rendering themselves liable to the interference of the Apothecaries' Company. I cannot conceive how these gentlemen can imagine that the respectability of their own department would be promoted by their possession of this power. If this right be conceded to them, it cannot be denied to the graduates of other universities; and the general exercise of it, I am persuaded, would soon produce jealousy and discontent amongst the members of both branches of the profession. If a physician chooses to become a general practitioner, he ought not to deem it a hardship that he be compelled to forego his title of Doctor, and submit to an examination at least in surgery, pharmacy, and midwifery,—subjects on which, it may be presumed, he was not minutely questioned when he obtained his medical degree.

In so vehemently contending for this privilege, the Edinburgh graduates ought, at any rate, to recommend liberality to their own medical corporate bodies. The first surgeon in London would not, in the present day, be permitted to practise in Edinburgh, without becoming a Fellow of the College of Surgeons in that city; a privilege he could not attain, except by passing an examination, defending a thesis, and paying a large fee.

All these circumstances shew the necessity of adopting, as nearly as possible, one general system in the medical corporate bodies of different parts of the United Kingdom; and I do not believe that much benefit will arise from any change that does not include this important object. If, however, one standard of qualification be fixed for physicians and surgeons, in England, Scotland, and Ireland, much of the existing discontent will be removed; reciprocity of interests and privileges may then prevail, and members of either of the national universities be allowed to practise in any part of the British dominions, merely on entering their names in the list of practitioners of the country in which they reside.

I do not expect that the plan, the mere outline of which I have sketched in the foregoing pages, will meet with the approbation of those who have formed Utopian schemes of perfection; I think, however, that we should consider the actual state of society, and the profession, and endeavour to achieve what is practicable, if we cannot accomplish all that is desirable; and I believe that the organization which I have proposed for each department, comprises all that is essential either for the benefit of the public, or the advantage of the profession, and with the least possible violation of the privileges of the chartered medical bodies.

In conclusion I have only to state, that I have no personal object in view; I have been a member of the College of

Surgeons thirty-four years, and do not expect that my prospects can be benefitted, or injured, by any change which may be made in the present system. A sincere regard for the welfare of my profession has alone induced me to engage in this discussion; and in the hope that the labours of the Parliamentary Committee will promote the interests of all classes of medical men, and the advantage of the public, I beg to subscribe myself,

Your obedient servant,
A Provincial Hospital Surgeon.

March 29, 1834.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'Alembert.

The Natural History of Animalcules, containing Descriptions of all the known Species of Infusoria; with Instructions for procuring them, &c. By A. Pritchard, Esq.

To Mr. Pritchard the study of the natural history of what may in some respect be called the *invisible* world, is greatly indebted: it was he who first constructed and employed the Jewelled Microscope: he has published several works illustrative of the powers of that admirable instrument; and he now comes forward the first to supply us, in our own language, with a systematic treatise on the Infusoria.

The design of his present volume is clearly stated in the preface. The author wished to reply satisfactorily to the queries addressed to him from various quarters; such as, Where a good description of animalcules could be found?—What magnifying powers are best to view them with?—What are their comparative sizes? Have such and such curious creatures been already discovered?—Where are there drawings of them to be had?—&c. Accordingly, with a view to satisfy at once his immediate applicants and the public, he has put together the results of his own practical observations; he has availed himself freely of the labours of Müller and Ehrenberg, especially in the arrangement of his materials; and with the assistance of an able artist (Mr. Cleghorn), to whom he entrusted the pictorial department, he has presented us with a performance which has hitherto been a considerable desideratum in Great Britain. Upwards of 400 species of animalcules are here described systematically, in brief and interesting language; and above 300 of them are pourtrayed in a very superior style, in their magnified aspect. In a preliminary book, Mr. Pritchard gives us much valuable information on general matters connected with his subject—such as the mode of procuring animalcules, chiefly from vegetable infusions, &c. The second book embraces the systematic view of the known genera and species. And in the third, we have Ehrenberg's arrangement of the Phytozoa, followed by our author's remarks.

Many readers, we rather suspect, will be surprised at the announcement of there being *four hundred and thirty-six* species of living creatures now ascertained, far the greater portion of which are invisible to the naked eye by reason of their minuteness. Yet so it is: though we confess we would not strongly engage for the exactitude of the number. Ehrenberg himself, we observe, is sometimes undecided, whether he is not describing as distinct species animalcules which only differ from each other, in consequence of the varieties of form during the period of growth. The Paramæcia and Kolpoda, we find, *he* considers as identical with the Monads and Cyclidia, only at a more advanced stage of their animalcular existence. The Kolpoda *nucleus*, again, is confessedly the young of the K. *cucullus*; yet they are put down as distinct species. And we cannot help noticing it, as another remarkable circumstance, that the Gonia, though commonly found in clusters, yet when viewed singly, are allowed to resemble strongly the genus Volvox. But facts such as these do not, of course escape the notice of Mr. Pritchard, who says, when speaking of the Vibrio *glutinis*, the oviparous and viviparous individuals of which are accounted by some observers as distinct species, that there is some excuse for *microscopical* mistakes of this kind. "It is but common justice to the microscopist to mention, that although labouring under such immense disadvantages in this department of nature, from the minuteness of his subject; the difficulty of preserving identical specimens

from infancy to maturity; and even if he accomplishes all this satisfactorily, all his knowledge of them being obtained by one organ only—the eye,—and that in an artificial manner,—ornithologists have committed *more* egregious errors. Thus Gmelin, and some others, have described the same individual, "the Golden Eagle" (*Aquila chrysaëtos*), in different stages of its growth, as four distinct species." We do not altogether like this reasoning, though we admit there is something plausible in it.

It were to be wished that Mr. Pritchard had gone somewhat further in his account of animalcules, and not confined himself, as he has done on the present occasion, to the Infusoria alone. We cannot but recollect how much remains to be known concerning spermatic animalcules, for example; and touching several moot points in human physiology, which require for their elucidation such instruments as those of our author, with ability to use them such as he possesses.

Many volumes, under the express title of *entertaining* knowledge, have of late been given to the public: we know few of them worthy of being compared with this book of Mr. Pritchard's, which has both novelty to recommend it, and at the same time abounds with materials for instruction and delight.

Traité d'Anatomie descriptive, rédigé d'après l'ordre adopté à la Faculté de Médecine de Paris. Par H. Cloquet. *Edition Belge; augmentée de Notes, d'un Atlas de Planches*, &c. Livraisons I. and II. Dulau.

This edition of Cloquet, which is to be completed in four livraisons, has the usual merits of the productions of the Belgian press to recommend it—cheapness and compactness. The plates are on a reduced scale, but well calculated for convenient reference. And we may add, that the notes of the editor (Dr. Meisser), which are copiously compiled from the best anatomical writers of the day, constitute a very acceptable addition to the work.

SIR ASTLEY COOPER
ON
FRACTURE OF THE NECK OF THE FEMUR.

To the Editor of the Medical Gazette.

Sir,

I find in the last number of your Gazette, in a report of Le Baron Dupuytren's lecture, that he attributes to me the opinion, that fractures of the neck of the thigh bone, within the capsular ligament, not only "never unite, but that it is impossible they should unite by bone."

It is quite true that, as a *general principle*, I believe that those fractures unite by ligament, and not by bone, as do those of the patella and olecranon. But I deny that I have ever stated the impossibility of their ossific union; on the contrary, I have given the reason why they occasionally may unite by bone.

The following are my words—"*To deny the possibility of their union, and to maintain that no exception to this general rule may take place, would be presumptuous.*"

I then proceed to state, that the cause of the deficient union is to be found in the reflected ligament of the cervix femoris being torn through, and that thus a defective nutrition of the head of the bone is produced. But I held, that if the reflected ligament remained whole, or was but slightly torn when the bone was broken, it would unite by means of its cancellated structure; but that in such a case the limb would not be shortened in the usual manner, nor would the case have the common characteristics of this accident.

In proof of the truth of these observations, I shall give the following quotation from Mr. Swan's work on the Diseases of the Nerves, page 304.

"Case.—Mrs. Powell, aged above 80 years, fell down in the afternoon of the 14th of November, 1824. I saw her soon after, and found her complaining very much of pain in the left hip. The limb could be moved in every direction, but this motion produced excessive pain. She lay on her back, with the limb extended; and nothing *was ever done*, except the application of fomentations for the first few days. I believed there was

a fracture of the neck of the thigh-bone, although the limb remained quite as long as the other, and I could neither perceive a crepitus, nor any altered appearance in its position, except a slight inclination of the toes outwards. She had more constitutional irritation than I ever observed from a similar accident. She suffered much pain in her hip, and was in consequence obliged to take an opiate; but she got very little rest. She generally had much thirst. There was the utmost difficulty in keeping her bowels open; and she had great pain and difficulty in making water. She had no appetite for common food, and for three weeks appeared so weak, that she was under the necessity of taking wine and brandy. For some time all her urine and stools were passed in bed, but not involuntarily, and only because she could not be persuaded to use proper means; in consequence, her back became very sore. Latterly she complained of pain in the abdomen, which was very tender on pressure, and made even the weight of the bed-clothes inconvenient. Her tongue became very dry and brown; and in the last twenty-four hours she was insensible. She died on the morning of the 19th of December, about five.

"Examination.—This took place at seven in the evening.

"There was some ecchymosis amongst the muscles about the injured part, and in the cellular membrane, about the sciatic and anterior crural nerves. The greatest part of the fracture of the neck of the thigh-bone, which was entirely within the capsular ligament, was firmly united.

"A section was made through the fractured part, and a faint white line was perceived in one portion of the union, but the rest appeared to be entirely bone." Mr. Swan goes on to say, "This beautifully shews the principle which Sir Astley Cooper has advocated—viz. that when the reflected ligament remains whole, and the bones are not drawn asunder, the nourishment to the head of the bone continues, and union will be produced even in the short space of five weeks, by only placing the knee over a pillow, and in other respects leaving the case to nature."

Of the contrary state of the fracture—viz. that in which the reflected ligament is torn—I may give the case sent to me by Mr. Robertson, of Sheerness, in which the patient's limb was extended for six months without any ossific union.

On the 25th of June, 1822, William Durain, aged 62, a tall, athletic convict, of a sanguine temperament, fell, with a very inconsiderable violence, across a piece of timber in the Dock-yard, his left hip coming in contact with the wood. On rising, he felt an acute pain in the region of the acetabulum, but no other inconvenience, for he walked on board to exhibit himself to the surgery-man. From finding him ranked up with the sick of the hulk, on my morning visit of the 26th, from his walking on board, and from his own account of the accident, I did not suspect any serious injury to the joint, and treated the case as one of concussion.

On the 29th, however, he complained of a very sudden and very agonizing accession of pain, which induced me to subject him to a more critical examination. No evident alteration in the size of either hip could be discerned, but a shortening of the limb was conspicuous, which was rendered more evident by making him stand upon the sound limb: extension removed this difference; but, on being freed from restraint, it again assumed its morbid shape; the knee and foot were everted, and rotation greatly increased his pain.

I removed him to the hospital, as a case of fracture within the capsule; but a continued attention for a period of six months to position, (chiefly with a view of restraining the motion of the pelvis, and of securing the limb,) made no other alteration in the symptoms than a gradual diminution of pain: a pair of crutches were given him, he was placed on the invalid list, and remained till the 26th of September, when he died from general dropsy. On dissection, the injury proved a transverse fracture of the head of the femur, within the capsular ligament; no species of union had taken place. The upper portion of the fractured bone was retained in situ, by the sound ligament, tolerably smooth upon its surface, but without any ossific deposit.

The lower portion was very irregular, with several detached pieces of bone adhering to the insertion of the capsular ligament. Between the acetabulum and the portion of bone retained in situ by the ligament, were several small oval-shaped, loose cartilaginous substances, apparently

fragments of bone; the capsular ligament partially lacerated, in a line above the trochanter major, and greatly thickened in its insertions.

I should not have given you this trouble, nor should I have taken it myself, but for the respect I bear my friend the Baron Dupuytren; for, although I have readily submitted myself to be misrepresented by many individuals, yet I should be sorry to be misunderstood by so excellent a surgeon, and so valuable a friend, as le Baron Dupuytren.

I am, sir, yours, &c. &c.

ASTLEY COOPER.

Conduit-Street, April 21, 1834.

MEDICAL GAZETTE.

Saturday, April 26, 1834.

OPPOSITION TO THE PETITION FROM THE GOWER-STREET SCHOOL.

PROPOSED METROPOLITAN UNIVERSITY.

WE beg to direct the attention of our readers to the very important documents on the above subjects, which we are enabled to lay before them in our present number; and having done this, we feel that any remarks of our own are rendered almost superfluous.

The systematic opposition which we, and we alone, among the conductors of the medical press, have made to the claims of the so-called London University; and the plan of instituting a great Metropolitan University, which was first proposed, and perseveringly dwelt upon, in our pages,—are now approved by the solemn declaration, and go forth to the world supported by the signatures, of a hundred men who have been engaged in the business of teaching the different branches of medical science; and including among them every physician and surgeon of eminence in the metropolis, devoted to such pursuits. Among these, not the least remarkable are the professors of King's College—the names of the whole medical staff of that establishment, with but one exception, being enrolled in the list: and when it is considered that the power of conferring degrees, if bestowed upon the London University, would also, as a matter of course, be given to the rival institution, it is but due to them to say, that the part which they have taken is highly creditable to their liberality, while it shews very strongly their sense of the injustice of that monopolizing spirit, which they thus come forward to oppose.

We have every reason to believe that the prayer in the subjoined petition will be granted. But it is needless for us now to speculate upon the subject, because before this may meet the eye of many readers, the matter will probably have been discussed before the Privy Council, and a decision may virtually, though not publicly, be arrived at. An act of incorporation the school in Gower-Street ought, no doubt, to have, but certainly not the privilege of conferring degrees in medicine—else will medical science have been sacrificed on the pretext of political expediency. There are clearly two ways of meeting the claims of the Dissenters—a just and an unjust; and we have little doubt of the former prevailing.

SOME particulars relative to the proceedings before the Privy Council have just reached us. The arguments were commenced on Thursday by Dr. Lushington, and Mr. Kindersley, as counsel for the London University. Sir Charles Wetherell followed, for the University of Oxford, but had only time to begin—as the hour of adjournment (four o'clock) arrived soon after his opening remarks: he resumes, however, to-day (Saturday), and will be followed either by Mr. Starkie or Mr. Bickersteth, for Cambridge. Mr. Follett supports the petition of the medical lecturers of the metropolis; and Mr. Pollock appears on behalf of the College of Surgeons.

PETITION OF THE MEDICAL TEACHERS IN LONDON,

AGAINST THE

GRANT OF EXCLUSIVE PRIVILEGES TO THE MEDICAL SCHOOL OF GOWER-STREET.

To the King's Most Excellent Majesty, in Council.

The humble Petition of the undersigned Physicians, Surgeons, and Lecturers of the Hospitals and other Medical Schools of London,

Sheweth,

That, in consequence of a renewed application to your Majesty from the Council of the London University, praying for a charter of incorporation, which would include the privilege of conferring degrees in physic—We, the undersigned Physicians, Surgeons, and Lecturers of the Hospitals and other Medical Schools of London, having very carefully and maturely considered the consequences to which such privilege would lead, are unanimously of opinion, that the granting of such power to the London University would be eminently unjust towards ourselves, would impede the progress of medical science, and would therefore be injurious to the public interest.

That the reasons which have led your Majesty's petitioners to these conclusions are detailed in a memorial presented to the Lord President of your Majesty's Most Honourable Privy Council, by Sir George Tuthill, Dr. Watson, and Mr. Lawrence, on the fourth day of March last; that, for the reasons therein set forth, your petitioners most humbly pray that no charter involving the privilege of conferring degrees in physic may be granted, either to the London University, or to any particular school of medicine; and that your Majesty will be graciously pleased to direct, that your petitioners may be heard by counsel before your Majesty's Most Honourable Privy Council, in opposition to the prayer of the London University.

And your petitioners, as in duty bound, will ever pray.

E. Roberts, M.D., Physician to St. Bartholomew's Hospital.
Clem. Hue, M.D., Physician to St. Bartholomew's Hospital, and Lecturer on Medicine and Chemistry.
P. M. Latham, M.D., Physician to St. Bartholomew's Hospital.
J. P. Vincent, Surgeon to St. Bartholomew's Hospital.
Wm. Lawrence, Surgeon to St. Bartholomew's Hospital, and Lecturer on Surgery.
Henry Earle, Surgeon to St. Bartholomew's Hospital, and Professor of Surgery to the Royal College of Surgeons.
Edward Stanley, Assistant Surgeon to St. Bartholomew's Hospital, and Lecturer on Anatomy and Physiology.
E. A. Lloyd, Assistant Surgeon to St. Bartholomew's Hospital.
F. C. Skey, Assistant Surgeon to St. Bartholomew's, and Lecturer on Anatomy at the Medical School, Aldersgate-Street.
Thos. Wormald, Demonstrator of Anatomy at St. Bartholomew's Hospital.
George L. Roupell, M.D., Lecturer on Materia Medica at St. Bartholomew's Hospital.
George Burrows, M.D., Lecturer on Forensic Medicine at St. Bartholomew's Hospital.
Frederic John Farre, M.D., Lecturer on Botany at St. Bartholomew's Hospital.
James Lowe Wheeler, F.L.S., and Professor of Botany to the Society of Apothecaries.
Alexander Tweedie, M.D., Physician to the London Fever Hospital, and Lecturer on Medicine at the Medical School, Giltspur-Street.
R. B. Todd, M.D., Lecturer on Anatomy and Physiology at the Medical School, Aldersgate-Street.
John Burne, M.D., Lecturer on the Principles and Practice of Medicine at the Medical School, Aldersgate-Street.
William Cummin, M.D., Lecturer on Forensic Medicine, Medical School, Aldersgate-Street.
Jonathan Pereira, Lecturer on Chemistry and Materia Medica at the Medical School, Aldersgate-Street.

Benjamin Travers, F.R.S., Surgeon to St. Thomas's Hospital.
Joseph Hayes, Lecturer on Botany at St. Thomas's Hospital.
Samuel Solly, Lecturer on Anatomy and Physiology at St. Thomas's Hospital.
H. S. Roots, M.D., Physician to, and Clinical Lecturer at, St. Thomas's Hospital.
H. Burton, M.D., Assistant-Physician to, and Lecturer on Materia Medica at, St. Thomas's Hospital.
Bransby Blake Cooper, Surgeon to Guy's Hospital, Lecturer on Anatomy.
Gilbert Mackmurdo, Lecturer on Anatomy at St. Thomas's Hospital.
Richard Bright, M.D., Physician to Guy's Hospital, and Lecturer on Medicine and Clinical Medicine.
Eolraro Cock, Demonstrator of Anatomy at Guy's Hospital.
F. Le Gros Clark, Demonstrator of Anatomy at St. Thomas's Hospital.
C. Aston Key, Surgeon and Lecturer at Guy's Hospital.
John Morgan, Surgeon to Guy's Hospital, and Lecturer on Surgery at that Institution.
Robert Williams, M.D., Physician to, and Lecturer on Medicine at, St. Thomas's Hospital.
John Ashburner, M.D., Lecturer on Midwifery at St. Thomas's Hospital.
W. F. Chambers, M.D., Physician to St. George's Hospital, and Lecturer on Medicine (signed for him by Dr. R. Macleod, he being disabled by an accident.)
James Blundell, M.D., Lecturer on Midwifery in the School of Guy's Hospital.
Samuel Ashwell, M.D., Lecturer on Midwifery in the School of Guy's Hospital.
Arthur Aikin, F.L.S., &c., Lecturer on Chemistry at Guy's Hospital.
John Hilton, Demonstrator of Anatomy at Guy's Hospital.
T. Callaway, Guy's Hospital.
Frederick Tyrrell, Surgeon to St. Thomas's Hospital, and Lecturer on Surgery.
B. C. Brodie, Serjeant-Surgeon to the King, Surgeon to St. George's Hospital.
Astley Cooper, Serjeant-Surgeon to the King and Consulting Surgeon to Guy's Hospital.
Cæsar Hawkins, Surgeon to St. George's Hospital, and Lecturer on Surgery.
George Gregory, M.D., Physician of the Small-Pox and Vaccination Hospital, Lecturer on Medicine.
Robert Keate, Surgeon to St. George's Hospital.
James Arthur Wilson, M.D., Physician to St. George's Hospital, and Lecturer on Anatomy.
Robert B. Walker, Assistant-Surgeon to St. George's, and Surgeon to the Lock Hospital.
James M. Arnott, Surgeon to the Middlesex Hospital.
John Ramsbotham, M.D., late Lecturer on Midwifery at the London Hospital.
Archibald Billing, M.D., Physician to the London Hospital.
Herbert Mayo, Surgeon to the Middlesex Hospital, Professor of Anatomy in King's College.
G. G. Babington, Surgeon to St. George's Hospital, and Lecturer on Surgery.
Samuel Lane, Lecturer on Anatomy, adjoining St. George's Hospital.
John Harrison, Demonstrator of Anatomy, adjoining St. George's Hospital.
Hugh Ley, M.D., Physician-Accoucheur to the Middlesex and General Lying-in Hospitals.
Edward J. Seymour, M.D., Physician to St. George's, and Lecturer on Materia Medica.
Robert Ferguson, M.D., Professor of Midwifery, King's College.
John Webster, M.D., Lecturer on Materia Medica and Therapeutics.
Thomas Watson, M.D., Physician to the Middlesex Hospital, Professor of Forensic Medicine, King's College.
Henry Davies, M.D., Lecturer on Midwifery.
Thomas Arthur Stone, Lecturer on Midwifery at St. George's Hospital.
Francis Hawkins, M.D., Physician to the Middlesex Hospital, and Professor of the Practice of Medicine in King's College.
Gilbert T. Burnett, Professor of Botany, King's College, London.
Richard Bushell, Teacher of Anatomy, Great Windmill-Street.
Geo. Jewel, M.D., Lecturer on Midwifery, &c.

Francis H. Ramsbotham, M.D., Lecturer on Midwifery, London Hospital.
James Luke, Surgeon to the London and St. Luke's Hospitals, and Lecturer on Anatomy, &c.
John Scott, Surgeon to the London Hospital, the Ophthalmic Infirmary, and Lecturer on Surgery.
Thomas Davies, M.D., Assistant Physician to the London Hospital, and Lecturer on Medicine.
Alfred Hamilton, Assistant Surgeon to the London Hospital, and Lecturer on Anatomy.
Algernon Frampton, M.D., Physician to the London Hospital.
Algernon Frampton, Jun., M.B., Assistant Physician to the London Hospital.
James A. Gordon, M.D., Physician to the London Hospital.
Frederick Cobb, M.D., Assistant Physician and Lecturer, London Hospital.
J. G. Andrews, Surgeon to the London Hospital.
Thomas Blizard Curling, Assistant Surgeon to the London Hospital.
George Pilcher, Lecturer on Anatomy and Surgery, Webb-Street, Borough.
William Blizard, Consulting Surgeon, London Hospital.
William Lynn, Surgeon to the Westminster Hospital.
Anthony Carlisle, Surgeon to the Westminster Hospital.
Anthony White, Surgeon to the Westminster Hospital.
John Bright, M.D., Physician to the Westminster Hospital.
Edward William Tuson, Assistant Surgeon to the Middlesex Hospital, Lecturer on Anatomy and Physiology, at the Little Windmill-Street School.
John Wilson, M.D., Physician to the Middlesex Hospital.
J. F. Daniell, Professor of Chemistry, in King's College, London.
Richard Partridge, Junior Professor of Anatomy in King's College, London.
John Whiting, M.D., Lecturer on Medicine, at the Medical School, Webb-Street, Southwark.
William Thomas Brande, Professor of Chemistry, Royal Institution.
George L. Tuthill, Physician to the Westminster Hospital.
George G. Sigmond, M.D., Lecturer on Medicine, Charing Cross Hospital.
John G. Smith, Lecturer on Anatomy, Hunterian Theatre, Great Windmill-Street.
J. Wyatt Crane, M.D., Lecturer on Medicine, Great Pulteney-Street.
Edward Rigby, M.D., Lecturer on Midwifery, St. Thomas's Hospital.
Robert Lee, M.D., lately Lecturer on Midwifery, Webb-Street School.
Benjamin Travers, Jun., Demonstrator of Anatomy, St. Thomas's Hospital.
Roderick Macleod, M.D., Physician to St. George's Hospital, and Lecturer on Medicine.
Joseph H. Green, Surgeon to St. Thomas's Hospital, and Professor of Surgery in King's College.
J. Hamilton Roe, Physician to the Westminster Hospital.
R. D. Grainger, Lecturer on Anatomy and Surgery, Webb-Street, Southwark.

[The subjoined is the document alluded to in the above petition.]

MEMORIAL

AGAINST

THE MONOPOLY SOUGHT FOR BY THE PROPRIETORS OF THE LONDON UNIVERSITY.

To the Most Noble the Marquis of Lansdowne, President of His Majesty's Most Honourable Privy Council.

The undersigned respectfully represent, that in consequence of a renewed application on behalf of the institution called the London University, to his Majesty's most honourable Privy Council, for a charter of incorporation, with the privilege of conferring degrees in medicine, a meeting has been held of the Physicians, Surgeons, and Lecturers of the Hospitals and Medical Schools of London, at which it was unanimously resolved, that the grant of such privilege would be unjust to themselves and injurious to the public; and the undersigned were deputed to express the objections of the meeting to your Lordship in writing, and personally, if your Lordship should be pleased to grant them the honour of an interview.

The undersigned beg leave to represent to your Lordship—

1. That the London University resembles in no respect the ancient universities of England; that it is a joint-stock association, founded and supported by money subscribed in shares, which may be bought and sold in the market like those of canals, gas-works, and other speculative undertakings; and consequently, that it is impossible to foresee into whose hands the important privilege now sought for might at any time pass, in right of purchase.

2. That several schools of medicine already exist in London, connected with its great hospitals, in which all the branches of the science are taught according to the method which reason and experience have shewn to be the most efficient—namely, by a judicious combination of instruction in the lecture-room, with study at the bed-side of the sick; in which also property to a very large amount has been invested in providing museums, libraries, and all the other requisites of professional education.

3. That the establishments alluded to have given to London its high character as a School of Medicine; in them the medical practitioners of England have acquired that professional knowledge and skill for which they are so eminently distinguished; in them have studied and taught many of the most celebrated improvers of medical science, among whom may be enumerated William and John Hunter, Heberden, Baillie, Pott, Cline, and Abernethy.

4. That the medical school which has assumed the name, and now claims the rights and privileges of a University, is superior to the previously-existing schools in no respect; while it is far inferior to them in not possessing a hospital, and thus wanting altogether the means of affording that practical instruction without which medicine and surgery cannot be learned. We are, indeed, aware that a small hospital has been begun, and that great, though hitherto unsuccessful, efforts have been made to open it for the reception of patients; but we submit, that while it is thus tacitly admitted that medicine cannot be taught without the means of clinical instruction, the building, if completed according to its present scale, would be totally insufficient for the accomplishment of its ostensible object.

5. That the present arrangement, by which the various medical schools in London have depended for their success entirely on the talents and exertions of their teachers, and on their respective advantages in other points, has excited and maintained a spirit of emulation highly advantageous to the public.

6. That to invest even the best of these schools with the exclusive power of conferring medical honours, would be to establish a most injurious monopoly. But the injustice and inexpediency of such a grant would be still more apparent if it were to be conceded to the imperfect school of medicine belonging to the London University, which at present can only be regarded as a very doubtful experiment; while it would be directly at variance with those principles on which that institution was established, and which have hitherto distinguished its founders.

7. That among other injurious effects of such a monopoly it would constitute the teachers of the favoured school judges of the proficiency and merit of their own pupils, and the sole distributors among them of the highest professional rights and distinctions—a system which would place their interest in continual opposition to their duty. It would, moreover, degrade and injure other schools, and diminish the number of persons employed in teaching; and thus it would check or destroy that honourable and useful rivalry which exists amongst different men engaged in the investigation of the same subjects, with the view of teaching them to others.

8. It is further respectfully submitted, that the pecuniary affairs of the London University having become embarrassed, it was stated in a printed report of the council to the proprietors, dated February 1833, that "at the beginning of October last the balance against the University was 2946*l.*, and that thence arose considerable difficulty at the close of the last session, as it was impossible to open the present session without obtaining some advance of money;" and that "the council passed a resolution, declaring that it was expedient to raise by loan the sum of 1500*l.* for the immediate purposes of the University. That in order to provide for this failing state of the finances, and for the future carrying on of the institution, the council have considered various plans, and that at the close of the present session, unless some of the plans suggested in the

sequel for giving support to the institution be adopted, they may be reduced to the necessity of giving notice that the institution cannot re-open on its present footing." Accordingly, before the opening of last session, the professors gave a guarantee to the council for the estimated expenditure of the year; in addition to which precaution it was deemed necessary to raise a loan of 4000*l.* by mortgage on the property of the University. Thus the documents published by the proprietors of this institution themselves, as well as the measures they have found it expedient to adopt, prove it to be very insecure, and wholly devoid of that stability which should be required of any institution soliciting such important privileges; while it is evident that the petitioners have a direct pecuniary interest in the success of their prayer, being large holders of shares in this joint-stock, which have fallen to a discount of not less than 75 per cent.

9. That in a subsequent part of the same report it is stated, that "the first *scheme* which presented itself to the council as favourable for the support of the institution, was, if possible, to obtain a charter, enabling it to grant degrees in subjects not theological." It is humbly submitted, that in the foregoing passage there is an open avowal of the motive which has induced the council of this institution to solicit the privilege in question; and that in the application now made they do not appear to have been influenced by any zeal for the promotion of literature or science, but have confessed it to be a "*scheme*" to prop the declining fortunes of the institution. It is, in fact, a "*scheme*" well calculated, as it is evidently designed, to entice medical students from the schools of practical science by the offer of academic distinctions.

10. That the undersigned have been informed, and believe it to be true, that many of the subjects of this realm, dissenting from the Established Church, have complained, and continue to complain, of their entire exclusion from the distinctions conferred at the English Universities; and that such complaint has been privately urged in favour of the Petition now before His Majesty's Council. But we humbly submit, that the relief which we venture to hope will be obtained by the Dissenters of England, ought not to be granted by such means as would necessarily inflict manifest injustice on a large body of His Majesty's subjects, disturb and degrade a profession which it is of vital importance to support, and check the progress of a science essential to the welfare of the community.

There is, however, another mode by which the Dissenters from the Established Church might, as far as academic honours are concerned, be effectually relieved, and which we humbly, but most earnestly, recommend to the attention of His Majesty's Government,—namely, the establishment of a Metropolitan University, framed in accordance with the liberal spirit of the age, and free from the difficulties of ecclesiastical distinctions. If this idea should be thought worthy of adoption, and it should be deemed expedient that degrees be conferred in London, we take the liberty of suggesting that all the advantages of such a plan would be secured, and that all the evils before pointed out would be avoided, by vesting the requisite power in some independent body, not concerned in teaching; so that all those who have been educated in any of the efficient schools should be admissible to examination for such degrees, on principles of perfect equality. In this manner a Medical University might be established in London, to which the several schools, and, among the rest, the London University, would stand in the relation of so many colleges; whilst to invest any one particular school with the exclusive privilege of conferring degrees in physic, would be the same thing as to grant a similar power to any single college in Oxford or Cambridge.

The undersigned, therefore, respectfully request of your Lordship, on behalf of the General Meeting of Physicians, Surgeons, and Lecturers, whom they represent, that the application of the London University having been laid before His Majesty's most honourable Privy Council, this statement may be presented to that right honourable body as a counter-petition; and that, if the matter should be farther entertained, they may have the opportunity of being heard by counsel in opposition to the grant of any exclusive privilege to the medical school of the London University.

George Tuthill, M.D.
Thomas Watson, M.D.
William Lawrence, Surgeon.

There has also been sent to us the PETITION of the ROYAL COLLEGE OF SURGEONS, addressed to the King in Council, to the same effect as the preceding documents, and couched in language equally strong. It is in type, and we only regret that want of room obliges us to postpone its publication for the present.

DR. ELLIOTSON AND MOSCATI.

IN acknowledging the receipt of Moscati's pamphlet, we expressed our intention not to enter into the subject of Moscati's allegations against Dr. Elliotson; but as the latter gentleman has addressed to us a letter, in which he answers what may be called the *professional* part of the pamphlet, we have felt ourselves called upon to publish it, although we had declined to insert the original charges. As, however, the answer would be unintelligible without the statement which it is intended to refute, we shall now extract that portion of Moscati's pamphlet to which it refers, and subjoin to it the answer of Dr. Elliotson.

"Towards the end of October 1832, I was desired by Dr. Elliotson to write a letter of censure on a lecture of Mr. Green, whom I had never seen, and whose name and profession were unknown to me. The printed lecture which that gentleman had sent, as a present, to Dr. Elliotson, with his compliments, was given to me. At first I declined; but the Doctor having informed me that Mr. Green concurred with a Dr. Macleod, who attacked him in the Medical Gazette, I wrote the letter, and gave it to Dr. Elliotson, who gave it to Mr. Mills, and it was published the 10th November, 1832, under the title of 'The Classical Learning displayed in the Sky-rocket Lecture.'

"However, some alterations were made to it, either by Dr. Elliotson or by Mr. Mills; for in my letter I had spoken of the supposed Parisian correspondent having seen the lecture in the Medical Gazette; but that journal being at war with Dr. Elliotson, was not to be mentioned as having its circulation in that capital. Several other words were added to my letter, of which even now I do not understand the meaning. This assertion I prove by the following sentence extracted from a letter of Mr. George J. Mills, *of his own hand-writing*:—'Dr. Elliotson has given me the letter on the lecture of Mr. Green; we have made some alterations; it will make an admirable letter.'

"As some faults had occurred in it, I went to the office of the Lancet, and spoke to a gentleman, requesting that they should be corrected. The next morning I received another letter from Mr. George J. Mills, *of his own hand-writing*, from which I extract the following passage:—'Mr. Hutchinson tells me that you say that there were many errors in the paper in the Lancet. Why, a proof was sent purposely to Dr. Elliotson, to prevent the possibility of errors. How could it be, then?'

"While I am on this subject I must also state, that when Mr. Green was lecturing at the Royal Academy of Painters, Dr. Elliotson engaged me to write critical reports of his lectures; and whenever I complained that I had nothing to do, and wanted money, 'Why,' answered he, 'it is your fault; why don't you do what I have desired you to do, and you shall have *a guinea* for each report printed in the Lancet?' But I always declined, because three days after the publication of my letter against Mr. Green, I met Mr. Elmore, of Cavendish-Street, and he told me, 'Well, I see with regret that you have been writing against Mr. Green, in the Lancet; Mr. Lynn also is angry with you.' At first I said they were wrong in thinking so; but Mr. Elmore replied, that all my friends had found me out from the style. Then I answered, 'Well, it is true.'—'And what has Mr. Green done to you?' added Elmore. 'Nothing; I don't know him.'—'And why have you done it?' replied he. Then, under the sacred promise of secresy, I told him that Elliotson had persuaded me to write the letter; and that as he was my friend, I had done it. 'It is nothing to me whether Elliotson is your true or false friend,' said he; 'but I tell you that Mr. Green is an amiable gentleman, and a clever surgeon. Do not do such a thing again, because, if I find it out, I will inform him of it.' I promised what he advised me, and have kept my word. The 18th March, 1834, having gone to the office of the Lancet to see whether the letters in my possession were truly *of the hand-writing of Mr. Mills*, and having been answered in the affirmative, I begged Mr. Mills to tell me whether Mr. Wakley or himself had ever commissioned Dr. Elliotson to engage me to write the reports in question, and he candidly said, 'No, never.'

"When I wrote the letter of Mr. Green, I had never seen either Mr. Wakley nor Mr. Mills, and did not know that Mr.

Green was a colleague of Dr. Elliotson, at St. Thomas's Hospital, nor that he was personally acquainted with the doctor; and to my great surprise, on the 11th of July, 1833, having gone with Elliotson to St. Thomas's Hospital to see a young man, an apprentice of the apothecary, who had been attacked with cholera, we went to visit a poor woman, who was to be operated upon for dropsy. While I was walking in the court of St. Thomas's Hospital, a tall gentleman, accompanied by several students, saluted me, and asked me whether I would assist at the operation. I thanked him, and demanded to whom I had the honour of speaking. 'Mr. Green,' answered he; 'I have heard you *improvise* at Dr. Elliotson's: I assure you, reader, that I felt a very strange sensation, particularly when having gone up stairs I saw Dr. Elliotson laughing with him and speaking to him in the most friendly manner!!!

"The second secret is, that Dr. Elliotson desired me to go to the first meeting of the College of Physicians of 1833, and to put in ridicule the president, and many others of his colleagues, in a report which was to appear in the Lancet. The reason why Dr. Elliotson is so much enraged against Sir Henry is, because that baronet intrigued in favour of another gentleman, and against Dr. Elliotson, when the latter aspired to a chair at King's College. I know nothing about it, because I was not in England, but only relate what Dr. Elliotson has told me. In short, Mr. Sedgewick obtained for me Sir Henry Halford's invitation, and I went to the meeting, wrote the report, and brought it myself to the chambers of Mr. Mills, by order of Dr. Elliotson; but Mr. Mills had gone out, and had left for me with his landlady the following note, of *his own handwriting*.

DEAR SIR,

I am obliged to go to St. Thomas's Hospital this morning, and am afraid I shall not be back till the afternoon. Will you, therefore, leave with my landlady the papers which Dr. Elliotson has promised you would favour me with, or at the Lancet office; and I shall receive them there the moment I arrive. I have waited writing my account of the College of Physicians meeting, to see what your admirable pen produces.

I am, my dear sir,
Yours most faithfully,
GEORGE J. MILLS.

Wednesday morning.

"As I am considered a good Latin scholar, Dr. Elliotson procured for me the Latin Essays of Sir Henry, and did all in his power to make me write a censure on them. But, thank God, I did not fulfil his wishes.

"I owe now to justice, to say that both Mr. Mills and Mr. Wakley have done all in their power to prevent me from making use of these letters; but when I exposed to them my situation, they said they were very sorry, and that they could scarcely believe that Dr. Elliotson had been the real cause of the attack of the Times.

"Now I repent of having written this, and leave the subject, disgusted with myself and with my denunciators, who have forced me to make use of so base a means for my vindication *."

DR. ELLIOTSON'S LETTER.

To the Editor of the Medical Gazette.

SIR,

As you have noticed an unhappy foreigner's abuse of me, I beg to assert, in the most positive manner, that all his statements are *entire falsehoods* or *gross misrepresentations*,—that I never attack any person openly or clandestinely, directly or indirectly,—never write, nor suggest or furnish materials to those who write, personally,—nor am conscious of ever having acted dishonourably or unkindly towards any man, though I will always defend myself vigorously.

It is altogether *false* that I suggested to him that he should go to the College of Physicians for the purpose of ridiculing any one, or even of reporting; nor did I know till now that he once furnished a report, or was even there on that particular evening, not having been present myself. His introduction of my name into a note from Mr. G. Mills *is a forgery*. He has madly, but happily, given up the original note to Dr. Moore, and many gentlemen, among whom is Sir Henry Halford, have seen it. Mr. George Mills's words are,—"Have the kindness to leave with my landlady *any papers you may have to favor me with*." He has actually printed,—"Will you, therefore, leave with my landlady the papers *which Dr. Elliotson has promised you would favour me with*."

I will not comment upon this atrocity. Sir Henry Halford has written me a very kind and handsome letter, expressive of his complete satisfaction. Before this forgery was discovered, I inquired at the Lancet office how my name had been introduced in an affair of which I was completely ignorant.

* "Mr. Moscati and John Elliotson, Esq. M.D. Cant.; or, the Revengeful Attack of the Times Explained and Refuted. By F. De Moscati." pp. 42.

Mr. G. Mills assured me he could not have done so; that, hearing this foreigner boast of being able to carry off a long speech, *verbatim*, by memory, and wishing for a faithful report, he had applied to him, but received an account of the paper so short and so vague, that three words only of it were printed.

It is *false* that I suggested to him to attack, or write at all upon, Mr. Green, or, indeed, ever employed him to write a line upon any subject whatever. Mr. Mills's letter, of which he has printed an extract, and in which Mr. M. wonders that any inaccuracies exist, since a proof was sent to Dr. Elliotson for correction, does not allude to the review of Mr. Green's lecture; but is declared by Mr. G. Mills, and was once confessed to Dr. Moore and another gentleman by the foreigner himself, to refer to a paper read by him in the Phrenological Society, and published in the reports of the society, all which pass through my hands.

In regard, however, to the review of Mr. Green's lecture, I allowed him to call at my house every morning to read the newspaper; frequently I did not see him, and my professional occupations seldom permitted me to see him long. He sometimes read portions of MSS., which he said were for the New Monthly, Athenæum, &c. &c. I perfectly recollect that he one day read the review in question, and I heard that he took it to the houses of others also. It had no heading as at present, nor were there the expressions, bats, dubs, and pures. In short, it was a perfectly inoffensive, plain piece of argument,—anything but an attack,—such a composition as neither Mr. Green, nor any man of sense, could have been displeased with, had it been written by a friend of opposite opinions. I have now read it again, and several gentlemen have read it at my request; and they all agree with me. I did not, therefore, think of attempting to prevent him from doing what he chose with his production, any more than I should if it had been written in opposition to opinions of my own. But I have a distinct recollection of having pointed out to him one sentence which I fancied objectionable, as perhaps rather rude, and of no use to his argument. I remember saying that Mr. Green was an excellent surgeon, and most estimable man, and that I hoped he would not print any thing approaching to rudeness. He promised me he would erase it; but broke his promise, and was reproved by me. All these circumstances I mentioned to many gentlemen at the time, without the slightest reserve. As he lived in St. John's Wood, I allowed him to have letters left for him in my hall, and to leave letters to be called for. At the time he wrote this review, the report of a clinical lecture was corrected by me for the Lancet,—a practice of which I never made a secret, any more than of correcting my lectures for the Gazette; and it is possible that he requested his review might be given to the printer when calling for my lecture; but I know nothing of the matter. Mr. Green has expressed to me his entire satisfaction.

Every other statement which he makes I can meet only with a positive denial. I found him an impostor *in every particular;* that he could not utter a word of truth upon the *most indifferent subject;* and the sole reason of his fury against me is, that after satisfying myself of his character, I one day mildly expostulated with him upon the impropriety of telling falsehoods. He replied, that his lies hurt no one, that he would never tell another before he came to my house again, and suddenly darted forth for good. Professor Rosetti, of King's College, expostulated once with him upon the same vice, and enumerated his various kinds of falsehoods, when he begged the Professor not to reproach him with his perpetual incorrigibility, "la mia finale impenitenza."

I will only add, that I never did him any harm, but all the good in my power; and once sent for my tailor, and clothed him from head to foot, when he was in tatters—that I did not mention his misdeeds to Dr. Fellowes till that gentleman had discovered them and dismissed him—did not originally communicate his charges to the Times, and for many months probably prevailed upon the Times not to expose him—nor did I ever speak ill of him till I found him propagating all kinds of falsehoods against me, when, in self-defence, I put every body in possession of his true character. I must trouble you with an extract of a letter written to me respecting him by a well-known physician of Paris:—"Il n'est plus Marquis que moi et vous. Si vous avez trouvé qu'il soit le plus grand menteur du monde, vous n'avez fait que constater ce que nous

avons déja reconnu à Paris. *Il ne peut jamais dire la vérité.* C'est une phénomene à étudier, qu'un homme pareil."

I trust I have said nothing more than circumstances demand: for I pity him, and he is now in confinement; and I am satisfied, from the countless number of falsehoods, their endless variety of character, their frequent want of apparent motive, and the palpable absurdity of so many of them, that he is insane.

I have the honour to remain, sir,
Your obedient servant,
JOHN ELLIOTSON.

[April 22, 1834.]

P.S. I understand he asserts that he is imprisoned at my instigation. The assertion is altogether false; and for some time I did not even know who had imprisoned him.

ROYAL INSTITUTION.

Friday, April 11.

DEFINITE ACTION OF ELECTRICITY.

DR. FARADAY this evening followed up, and brought to a close, his popular expositions of the doctrines which he has more technically and scientifically laid down in his last paper in the Philosophical Transactions. He introduced the subject of the *Definite Action of Electricity*, by first referring to the circumstances which attend the ordinary chemical action exerted in decomposing bodies; whence he passed to decompositions brought about, not by the agency of one body disuniting the components of another, but by the power of electricity disengaging the elements of the compound exposed to it,—and which elements are left at perfect liberty. But the object of the learned lecturer on this occasion, was to show that the quantities of those elements are always definite; and that a given electrical force not only always decomposes a constant quantity of a given substance, but actually disengages its elements exactly in those proportions which are equivalent in the scale of chemical action. The illustrations of this principle consisted of a series of beautiful experiments, which should be seen in order to be thoroughly understood.

On concluding the lecture, Dr. Faraday, only then apprized of the circumstance by a communication from the chair, announced that Mr. Fuller was dead: he had died that evening, without a struggle. Through respect to his memory there was no evening meeting on Friday the 18th, the day on which the funeral of this munificent patron of the institution took place.

CLINICAL OBSERVATIONS,

Delivered at the Middlesex Hospital, by

MR. ARNOTT.

LOOSE CARTILAGES IN THE KNEE-JOINT.—CHRONIC ABSCESS OF THE SHAFT OF THE FEMUR.

GENTLEMEN,—Between three and four months ago, a young woman applied at this hospital, to have a loose cartilage removed from her knee. The request was in the first instance evaded—it was subsequently acceded to. The consequences of the operation have nearly cost her her life—they *have* cost her a limb. Let us, before she leaves the hospital, review the circumstances of her case, that you may not lose any of that instruction which it so impressively conveys.

On her appearance here, the patient, Ann K., 27 years of age, and of spare make, stated that there was something in her left knee which moved about, and which, when it got under the knee-pan, gave her great pain, and prevented her walking. The substance had been first noticed three years before, on the dispersion of a swelling of the knee occasioned by a fall, and for which leeching and rest had been required. Ever since, the substance had been troublesome to her, but within the last three months much more so, attended with a greater degree of pain in the knee, and which was worst at night. The difficulty experienced in walking was now so great, that she never left the house except when compelled; and when she did so, she was invariably deprived of sleep the subsequent night, by aggravation of pain. On examining the knee, the patient after a few attempts succeeded in fixing the solid substance in a situation on the outside of the patella where it could be felt, apparently the size of a large Windsor bean. Observing a cicatrix on the inner and lower part of the thigh, just above the knee, I inquired what had occasioned it; and she replied, that when a girl she had been pushed down, and hurt her thigh; that a swelling slowly formed, broke, and after discharging matter between two and three years, a piece of bone came away, when the opening immediately healed. This last circumstance took place about eight years ago; and until the accident three years since she had suffered nothing, and enjoyed the free use of the limb.

The occurrence of loose cartilages in the cavities of joints is curious, but not very unusual. They have been most frequently

met with in the knee, but they have been found in the elbow, ankle, jaw, wrist, and shoulder-joints. There may be more than one; in the present instance there were two (for a second was discovered on her coming into the hospital), and as many as twenty have been found on dissection in one joint. They are of various forms and sizes, for the most part oblong, with rounded edges. One of these two, you observe, is about the size and shape of a peeled almond; the other is much larger, compressed, with rounded edges, and of very irregular knotted outline, not unlike a piece of flattened ginger root. One side of each is uniformly smooth, and like cartilage; the other side is less even, and is hard from the deposition of an osseous or calcareous matter, giving to this side a resemblance to the cartilages of the ribs when they are becoming ossified. A very delicate membrane, of extreme tenuity, can be raised in small portions from either surface.

In attempting to explain the origin and presence of these bodies, Mr. Hunter had a notion that they were originally formed by extravasated blood, which, becoming organized, took on the character of the parts with which it was in connexion; and he stated that they are never met, unless the subject of them has formerly or recently met with an accident. There is, however, no proof that extravasated blood ever does become organized; vessels have never been traced into it; and if these cartilaginous substances are invariably preceded by a local injury, which is no doubt generally true, this has sometimes been at so remote a period as to have escaped recollection. A more specious explanation has been given, by supposing that they are the result of coagulable lymph effused into the joint, from inflammation succeeding the injury; and this would apply to inflammatory action arising from any other cause: in the present case you heard that there had been a fall on the knee, and subsequent inflammation. But instead of being formed within the joint, they have been found outside of it, immediately beneath the synovial membrane, and, covered by this, projecting into the joint, so that the projection increasing the pedicle is elongated, and ultimately gives way, leaving them free; in this case they are probably formed without inflammation, by some action more analogous to that by which tumors generally are produced. Again, a morbid knotted growth of cartilage, from the edge of the articulating covering of the condyles of the femur, is sometimes met with; and a portion of this being broken off, produces a loose cartilage of a different kind. We lately had a man here complaining of pain in the knee-joint in certain motions of it, and which seemed to depend on a hard knob seated on the inner and lower edge of the trochlea of the femur. This kind of loose cartilage is more rarely met with than the preceding. You may see some good preparations of it in the Museum of the College of Surgeons.

It is seldom that we find these cartilaginous bodies still attached by their pedicle; for so long as this remains entire, they are less likely to get between the surfaces of the bones forming the joint; the pain attending which circumstance occasions their detection, otherwise their existence is not noticed. This pain is so acute, and the inconvenience in walking so considerable, as to induce patients to seek relief even by an operation.

Now the operation by which such substances are removed is simple, easy of execution, attended with little pain, and with scarcely any bleeding. But are these the only merits to be sought for in an operation? If, in treating of the subject of cataract, I was to inquire of you which was the most *perfect* operation—that by extraction of the lens, or that by its division and absorption, you would no doubt answer the former, because by it the opaque substance is removed at once and for ever from the eye: but if I was to ask you which was the *safest*, you would say that by division; and why? principally because it is much less subject to be followed by inflammation, and on this ground it is more generally preferable. Now although we have no such choice of operation in the case of loose substances in a joint, we have still to inquire if the proceeding above alluded to for their removal is *safe*. In answering this question, you have to consider what are the consequences which ensue from wounds of joints, punctured, incised, or lacerated; and experience will suggest to you that they are always serious. Even in the smallest joints they are often followed by destruction of its function; they sometimes entail the loss of a limb, and of life itself. Such results are mainly owing to that susceptibility to inflame upon injury which is common to all synovial membranes, and which is of greater importance when occurring in the knee, from the great extent of this membrane, and the variety of parts which it covers, including, as it does, the crucial ligaments and semi-lunar cartilages, as well as the cartilaginous extremities of the bones. Once the inflammation is set up in it by a penetrating wound, these other parts readily participate, and a constitutional disturbance, of the most violent character, ensues; so that not only destruction of the joint, but of life, may result. But I need

not enlarge upon this subject, gentlemen. You have lately seen an instance, under the care of one of my colleagues, of all but a fatal termination, from a small and neglected wound of the knee by a scythe.

It is the knowledge, then, of these circumstances which renders us never anxious to recommend the removal of cartilaginous bodies from the knee by operation. When this young woman applied, I therefore thought it right to apprise her, that though the operation was effectual, and frequently succeeded, yet that being attended with considerable risk and danger, I advised her not to have it done, but, after giving the limb rest, and applying cold lotion for a few days, to have a bandage applied, with a view to confine the loose cartilage in a fixed position, whereby it could not get into the joint. This is a method which ought invariably to be tried. For three weeks she did persevere in this, but the bandage failed in the object proposed, and she returned at the end of that time, requesting to be taken into the house, as she was kept "in a constant state of worry," and was unable to attend to her children, and wished to have the operation performed. She was admitted, and an impartial statement of the risk attending the operation having been again made to her, she persisted in her resolution, to take the chance of it.

Our first duty, then, was to take every precaution which experience has suggested as most likely to ensure the success of the operation. Ten days were allowed to elapse before it was done. She was kept on low diet; had aperient medicine; cold lotion was applied to the knee; and the two preceding days she was kept in bed, with a straight splint applied to the back part of the limb, that she might be accustomed to the position in which it was to be maintained for some days afterwards. The operation itself was performed in the ward, and in bed, so that no unnecessary motion might be occasioned. The two cartilages having been brought to the outside of the patella, were encircled and held there, by a silver tongue-scraper. The integuments being drawn aside, an incision was made upon the largest body; and when this was exposed, it projected, and was laid hold of by a tenaculum. A little delay was occasioned by its irregular shape, a knob on one of its extremities being caught and detained, requiring a further enlargement of the wound in the capsule; when it was extracted, the smaller one readily followed. The wound in the integuments was immediately accurately closed, and ice was applied to the joint by way of precaution.

For two days every thing promised success; the wound in the integuments had united, with the exception of a single point. There was no pain or swelling. Fifty hours after the operation, the pulse having become frequent, the knee was again examined, and a slight puffiness being observed on each side of the ligament of the patella, twenty-four leeches were immediately applied. As the swelling increased, they were renewed in a few hours, and again and again; so that in the course of four days one hundred and fifty had been applied, besides blood taken by cupping from the lower part of the thigh. In the intervals of the application of the leeches, ice was kept day and night on the knee, but in vain. The inflammation proceeded, the knee became distended, the wound re-opened; first a sort of sero-purulent matter, or rather synovial fluid and pus mixed together, and then pure pus, were discharged in large quantities, and that in eight days after the operation. The pain in the joint now became most acute, and it was evident that the cartilages were becoming affected; a large caustic issue, therefore, was made on the integuments covering the inside of the joint, and in a few days another on the outside, the ice still being continued. During this time the most violent constitutional disturbance had taken place, so as to endanger the safety of the patient. Our objects now were to prevent an immediately fatal issue, and to favour anchylosis, as the joint was evidently destroyed: but abscesses formed around the knee, one at its upper and inner part, near the cicatrix already mentioned as being at the lower part of the thigh; another formed on the front of the leg, immediately below the tubercle of the tibia, having followed the course of the flexor tendons from the ham. The patient complained of additional pain in the joint, and a grating in it, whenever she made the slightest alteration in the position of her body, although a splint was attached to the limb to keep this steady. She became much emaciated, with constant night perspirations: she had diarrhœa also, and the skin over the sacrum had given way. To save her life, then, we were obliged, two months after the operation, to amputate the limb, which I took off a little higher than usual, on account of the abscess at the lower part of the thigh. On examination, the joint was found filled with purulent matter; the synovial membrane, where it remained, highly vascular, thickened, and pulpy; the cartilaginous coverings of the condyles of the femur, and of the head of the tibia, extensively absorbed, exposing the bone (of its natural hardness); the semi-lunar cartilages almost entirely removed, and the crucial ligaments partially destroyed.

Here, then, gentlemen, notwithstanding

that every precaution was taken to ensure the success of the operation, you have witnessed its failure; you have likewise witnessed the access of that inflammation which we endeavoured to guard against;—and how uncontrolable this proved under the most active treatment. This ought to impress upon your minds the absolute necessity of first trying, in every case, the efficacy of a bandage or knee-cap, and of dissuading patients from the operation. When you *are* compelled to resort to it, bear in mind that it is but justice to them and to yourselves that they should be made fully aware of the risk, and that they themselves, therefore, must bear the responsibility. With regard to our unfortunate patient, she suffered so severely, so constantly, and was so unequal to attend to her family, that I believe the result was, *to her*, not a matter of regret; and this leads me to another and very important fact in the case.

You will recollect, that, in examining the knee, when Ann K. first applied for admission, a cicatrix being observed on the inner and lower part of the thigh, she stated, in answer to the question of what had occasioned this, that a piece of bone had come out here more than eight years previously. Desirous to ascertain whence this had been derived, I examined the corresponding part of the femur after its removal, and was first of all struck with the great thickening of the periosteum, covering the inner side of that bone, as you see here. At one spot, three inches above its lower extremity, three or four drops of a dark-coloured sanies were found beneath the periosteum: the bone, however, with which it was in contact, was not carious, nor did it present any excavation or breach of surface; it was simply rough. At the distance of more than an inch from this there was embedded in the thickened periosteum, in a sort of cyst, a particle of detached yellow bone, the size of one of the ossicula auditus. The amputated portion of femur, which was not enlarged, was now sawn through perpendicularly, in its whole length, when an abscess was discovered in its centre, containing about a drachm and a half, or two drachms, of very thick, yellow, purulent matter. This was contained in a cavity of irregular form, about two inches and a quarter in length, the lower end of which was four inches from the articulating end of the bone: the cavity was lined by a rose-red, soft, thick membrane, and had no communication whatever with the external surface of the bone. The bone around the cavity was of a whiter colour, and, as you will observe, is much more solid than natural, from the deposition of osseous matter into its cancellated structure, which cancellated structure, indeed, might be said not to exist. This solid state of the bone extended as high up as where it was sawed through in amputating, an inch and a half higher than the upper end of the abscess, so that here there was no medullary cavity. Below the abscess, about half-way between it and the end of the femur, in the middle of the cancellous structure, another detached yellow osseous particle, about the size of the incus, was imbedded in thickened medullary membrane; there was, however, no pus or other fluid here. The diameter of the femur was not increased; it was not enlarged; and its external surface presented no appearance of disease, with the exception that it was less smooth, and the thickened periosteum adhered to it, perhaps, a little less firmly than when in a healthy state.

How long could this abscess in the bone have existed? I can have no hesitation in replying, that, considering its situation, and the state of the bone around it, it must have done so for some time—months before the operation. Such a change as was here observed in the cancellous structure is the result of slow deposition from chronic inflammation.

The kind of abscess in the centre of the bone here met with has not been much noticed; and you may imagine, by the almost accidental manner in which it was in this case discovered, how easily it may be overlooked, when not accompanied by external swelling or enlargement of the bone. You will, however, find a most interesting account of three cases of chronic abscess of the tibia of analogous character, by Mr. Brodie, in the 17th volume of the Medico-Chirurgical Transactions. In two of these cases the disease had been in existence or progress for twelve years, and in the other for eighteen; all were attended with constant aching, and with occasional exacerbations of most excruciating pain; in all there was enlargement of the bone. In our case, I can hardly doubt that the abscess of the femur had some connexion in its origin with that disease, which had apparently terminated eight years previously with the exit of a portion of bone. I am also disposed to attribute the increased sufferings, for the three months preceding the patient's application here, and more especially the pain, which was worst at night, to this chronic abscess of the femur. Entertaining these opinions, it is impossible but that I should attribute the unfavourable issue of the operation for extracting the loose cartilages in a considerable degree to this serious complication,—a complication which must have rendered the joint in its vicinity much more susceptible of inflammation, and the inflammation, when excited, more violent and intractable.

PROJECTED MEDICAL SCHOOL AT YORK.

To the Editor of the Medical Gazette.

Sir,

The object of provincial medical schools is to advance the respectability of the profession; and, so long as this is the end aimed at, (and it can be proved that they are advantageous), so long is it the *duty* of every member of the profession to use his influence in effecting their establishment. I am not an advocate for exclusive provincial education,—for it must be allowed that the London schools possess superior privileges, compared with those in the country,—yet, nevertheless, the local schools have had their share in contributing to the general improvement which has been accomplished in the profession of late,—a proposition, in my opinion, which no man can well dispute; and if their number be extended, and the "signs of the times" augur aright, I aver that their influcnee will be augmented, and luminaries will arise among them, whose names will add honour to their country and the science they cultivate.

You, Mr. Editor, must be aware,—you know well the state of the profession,—that the Manchester, Liverpool, and Birmingham schools are using their influence in raising the character of the profession; and it is to be regretted that similar institutions are not more general in the provinces; I trust they soon will be.

In thus anticipating, I am glad to communicate something (scarcely amounting to positive information) relative to the pending prospects of a school where it has long been called for. The city to which I allude is already eminently distinguished for its readiness in promulgating science and literature. Its connexion with the Scientific Association, &c. &c. renders it well qualified for still greater improvement; and if we for a moment recal its former greatness, its former pre-eminence, and consider its *present* condition, we shall be ready to exclaim, why is it that no medical school stands within its renowned walls? Why is it that *no* lectures connected with medicine are given either at its hospital or dispensary? Surely it is time the spirit of innovation visited *Old Ebor*, or the rust which has lain on her so long will become impenetrable by age, and put at defiance the energies of modern reformation. The spirit of the times calls for improvement; the situation and prospects of York—once the grand emporium of all that was great—call imperatively for improvement; and I trust—I, who live not five miles from the walls of the city—that such an effort will be made as shall end in the organization of a medical school.

You, sir, will recollect, that last year you communicated to the profession, that the medical men, and also their pupils, made some effort in this cause. At that time things promised well—so far as promises extend—but the object was never attained; and the spirit which at first animated the friends of the school appears now to have fallen into the Tartarean gulph. The genius of improvement surely never had so unfelicitous an adventure.

But for the data which prove *ad demonstrationem* that a school has been attempted: more than twelve months ago the medical men, as a body, held several meetings, and after some time had elapsed a committee was formed. The result of their meetings was, that a school of medicine is wanted. I cannot learn any thing further, save that one or two of their number endeavoured to find some rooms which would be fit for anatomical purposes. These were never found; at least not recognized by the committee. In consequence of this desideratum, some of them proposed erecting a convenient building; others, that the room at the Hospital, if it could be procured, would be more advantageous. In such a state the "school affairs" remained until a few months ago, when another general meeting was called (said to be the last), at which a resolution was carried, to the end that the medical men as a body would have no connexion with the proposed school, but that the affair be left open for any members of the profession who may feel interested in it.

Such is the present state of the pending school; and if I have not given a full account of the proceedings, I give myself the credit that what I have advanced is correct, and that I have made no *ex parte* reflections. I conclude, with the prospect of again addressing you on this subject.

I am, sir,
Your obedient servant,
Machaon.

York, April 1834.

HOSPITALS OF PARIS.

THE LATE RIOTS.

The wounds received in the late serious riots were not so numerous as severe. Nineteen cases were taken into the Hôtel Dieu; one woman among them, who had received a bayonet-wound; of the rest, six were soldiers. Amputation at the shoulder-joint, in consequence of a grievous gun-

shot wound of the upper part of the humerus, was performed in one case, by M. Sanson. Two patients died soon after entering the hospital; one from fracture of the pelvis by a ball,—the other, from having been shot through the chest. The Hôpital Saint Louis received fifteen wounded, and the Val-de-Grâce eight. It is supposed that the entire number of the wounded in this *affaire* of the 14th of April (as it is called) did not much exceed one hundred.

ST. PANCRAS MEDICAL ASSOCIATION.

We are glad to perceive that an association for carrying into effect the benevolent principle of the Self-supporting Dispensaries, has at length been formed in London, in the parish of St. Pancras. The account of the arrangements which has been sent to us seems as complete as can be wished. It is proposed—"1, To place within the reach of the industrious artizan of small income the means of obtaining medical aid, without subjecting him to the necessity of seeking humiliating charity, or destroying his honest pride by exposing him to the demoralising influence of the poor laws; and, 2, To afford gratuitous relief to persons of real desert, who may be unable to procure by their own efforts the advantages just mentioned." However desirable it may be in the establishing these dispensaries, to keep them as much as possible strictly *self-supporting*, and for the benefit of a *free class* of patients alone, we cannot, perhaps, well object in the present circumstances to the admission of a *pauper* class; but we trust that ultimately, and chiefly by the force of good example, all the members will be of one class—the independent and the free.

NEW MEDICAL WORKS.

The Anatomy of the Human Eye. By John Dalrymple, Assistant-Surgeon to the Ophthalmic Infirmary. 8vo. 12s. bds.

The Second Fasciculus of Anatomical Drawings, selected from the Collection of Morbid Anatomy in the Army Medical Museum at Chatham. Folio. 21s.

Catalogue of Preparations, &c. in Morbid, Natural, and Comparative Anatomy, contained in the Museum of the Army Medical Department, Fort Pitt, Chatham. 8vo. 5s. bds.

JUST READY.

An Inquiry into the Principles and Practice of Medicine, founded on Original Philosophical Investigations. By G. Calvert Holland, M.D., Physician to the Sheffield General Infirmary. Vol. I. 8vo.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *April* 22, 1834.

Abscess	1	Hooping-Cough	12
Age and Debility	61	Inflammation	35
Apoplexy	9	Bowels & Stomach	9
Asthma	24	Brain	4
Cancer	1	Lungs and Pleura	12
Childbirth	5	Influenza	2
Consumption	106	Insanity	3
Convulsions	43	Jaundice	2
Croup	6	Liver, diseased	2
Dentition or Teething	9	Measles	7
Dropsy	26	Miscarriage	1
Dropsy on the Brain	20	Mortification	4
Dropsy on the Chest	1	Paralysis	4
Erysipelas	3	Small-Pox	2
Fever	11	Spasms	5
Fever, Scarlet	8	Stone and Gravel	2
Fever, Typhus	2	Thrush	2
Gout	1	Unknown Causes	7
Heart, diseased	1		
Hernia	1	Stillborn	20

Increase of Burials, as compared with the preceding week . . . 28

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

April, 1834.	THERMOMETER.		BAROMETER.	
Thursday . 10	from 27	to 47	30·24	to 30·21
Friday . . 11	26	47	30·14	30·02
Saturday . 12	30	44	29·93	29·88
Sunday . . 13	33	47	29·90	29·99
Monday . . 14	24	51	30·07	30·19
Tuesday . . 15	27	53	30·27	30·33
Wednesday 16	29	55	30·37	30.39

Prevailing wind, N.E.

Except the 10th and 12th, clear; flakes of snow at times on the 10th; hail frequent during the 12th, a rather heavy shower about 3 P.M. accompanied by thunder.

Rain fallen, ·05 of an inch.

Thursday . 17	from 33	to 57	30·40	to 30·44
Friday. . . 18	35	59	30·40	30·47
Saturday . 19	39	51	30·34	30·29
Sunday . . 20	31	55	30·23	30·18
Monday . . 21	34	58	30·14	30·11
Tuesday . 22	39	60	30·09	30·08
Wednesday 23	35	53	30·11	30·16

Wind variable, N.E. prevailing.

The 23d cloudy, otherwise generally clear; a shower of rain at half-past 12 on the 23d, not measurable.

CHARLES HENRY ADAMS.

ERRATA IN LAST NUMBER.—Page 95, line 20, *for* pairs, *read* paces; line 28, *for* have, *read* had.

W. WILSON, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MAY 3, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XXX.

DIFFICULT LABOUR.

RIGIDITY OF THE OS UTERI.

A fourth cause of lingering labour consists in rigidity of the soft parts through which the child must pass: and this is a subject of great interest, because of its frequent occurrence, the excessive pain generally attendant upon the case, and the difficulty that is often experienced in overcoming the resistance occasioned.

I have already said, that rigidity may exist in the os uteri, the vagina, or the perineum, separately; but we usually observe that when one part is affected, all the structures partake more or less of the unhealthy condition; so that even after the os uteri is fully distended, much delay and distress is experienced in the dilatation of the vagina and external parts.

Women who are bearing their first child, —especially if they be approaching the middle period of life,—those who possess a strong constitution engrafted on a vigorous and rigid fibre, are the most likely to suffer from this cause of protraction. To add to the distress, it is very usual, when extraordinary rigidity exists, for the membranes to break early in the labour; and this unfortunate occurrence much aggravates both the pain endured, and the tediousness of the dilating process.

Cases rendered lingering by preternatural rigidity are, in my opinion, the most perplexing—and perhaps I might add the most unmanageable—of all head presentations; they are frequently followed by inflammation of the uterus or vagina, by abscess, and sloughing. As these states deserve a distinct consideration, we shall first give our attention to rigidity of the os uteri.

When a woman has borne a number of children, the uterine mouth usually dilates very readily, and therefore we may expect to meet with this cause of difficulty more frequently in primary labours; but this is by no means universally the case. I once witnessed a very uncommon exception to this general rule. I was requested to take charge of an unmarried woman, pregnant with her first child, who was in as comfortable circumstances as her situation would admit of. Under labour the os uteri opened with no difficulty, and the child was born naturally in four or five hours from the time I was summoned. She again became pregnant, but it was under very different circumstances; and her mind was much more disturbed than on the first occasion. On the accession of labour the membranes broke early; the pains soon became exceedingly violent; the head was urged powerfully against the undilated and rigid os uteri; irregular muscular spasms supervened; and at the end of about fifty hours from the rupture of the membranes,—when the os uteri had not acquired a diameter larger than that of a shilling,—in the acmé of a strong pain,—while I was instituting an examination with the greatest possible care, I felt the os uteri split on the right side, and I traced the rent considerably upwards through the cervix. At the same moment the head passed into the vagina, and was expelled by a continuance of the same contraction. During the process of this labour, I bled the patient to syncope three different times, and exhibited opium freely, my mind being impressed with a dread of the very accident which occurred. It is an instructive case, because it proves, that although an os uteri has relaxed and

dilated readily in a first labour, it may on after occasions possess a high degree of unnatural rigidity,—and that too independently of the existence of any discoverable disease in the organ itself. It proves, also, that the much vaunted power both of bleeding and opium will not always avail in removing rigidity. The poor creature died on the fourth day after delivery, of uterine inflammation.

To another case I was called, in consultation, in which the os uteri had entirely sloughed off, in consequence of the strong pressure to which it had been exposed for a great length of time. This fact, sufficiently evident before delivery, I had an opportunity of ascertaining beyond a doubt by dissection. There existed, also, a slightly distorted pelvis.

A case occurred to Mr. Scott, of Norwich, in which the os uteri and a considerable portion of the cervix was torn off by the force of the uterine contractions, and came away in an entire state, presenting the appearance of a circular fleshy substance, having a central aperture. The patient eventually recovered, after hovering for a long time on the brink of destruction. These two last accidents occurred under first labours.

DISEASE IN THE OS UTERI CAUSING RIGIDITY.

Sometimes the os uteri is rigid from disease, particularly schirro-cancer, and cauliflower excrescence—states which do not prevent conception, but must give rise to more or less difficulty under labour.

It has happened to me to see two instances of labour in which the mouth and neck of the womb were extensively affected with cancerous ulceration; and one in which a cauliflower excreseence of two years' growth, and of large size, was attached to the same organ. The two first cases occurred in patients of the Royal Maternity Charity, and I had been attending both for some weeks before labour. One of these women (in whom the whole disc of the os uteri was destroyed by malignant ulceration, the vagina being extensively affected also,) went into labour rather prematurely; the process was so rapid, that the child was born before the midwife could arrive. The woman died in the second week after her delivery, and dissection proved the disease to have acquired the aggravated extent which I have just described. In the other case the os uteri was but partially destroyed; the remainder was thickly studded with scirrhous tubercles. The patient was worn down to the lowest ebb of life, and,—to lull the acuteness of her sufferings,—had been in the habit for many weeks of taking two ounces of laudanum daily. I was summoned by the midwife soon after the commencement of labour, and on my arrival I found that death had just taken place. There were very strong reasons to believe the child was not alive, and I thought it useless to extract it then. On opening the body the next day, the os uteri was found dilated to about the diameter of half a crown, partially ulcerated, the principal portion thickened, and exceedingly indurated. The membranes had ruptured; and although so short a time had elapsed from the commencement of uterine contractions, the patient had evidently sunk exhausted.

I shall briefly detail the history of the case of cauliflower excrescence, because it also possesses some points of interest. The lady had had one living child seven years before the time I speak of; and my father had been attending her for nearly two years for the uterine affection, during which she had once miscarried. She beeame pregnant a second time while labouring under the disease; abortion was threatened, but was with care averted. The membranes broke early on the morning of Sunday, May 26th, 1828, and uterine action came on at noon, when my father was called, and found the os uteri would just admit the tip of the finger. The pains continued strong all day, with scarcely any increase in dilatation; and at night an opiate was given, which procured an intermission of suffering, but no sleep. The process of dilatation went on very slowly through Monday, the pains continuing regular and powerful. I saw her for the first time at half-past eight on that evening. The os uteri was then dilated to the size of a crown; and from its whole disc a fungous tumor sprung, which filled a large portion of the vagina: the cervix was exceedingly indurated all round; the pains were very strong, and the vertex was being forcibly protruded with each return of uterine action, partially through the undilated and unyielding opening. Still the constitution had suffered but in a slight degree from the protraction of the labour, although so intensely painful. It was considered that it would be premature to adopt any means of delivery just then; and it was arranged that I should remain up with her during the night. Very little alteration was perceptible till half-past three in the morning, when, under the influence of a violent contraction, she suddenly screamed out that the child was passing. Being in the room at the moment, I instantly made an examination, and found the head had escaped through the os uteri, and was occupying the pelvis: in about half an hour it was expelled. The child was alive, and is so, I believe, still.

The placenta gave no trouble. From the rapidity with which the child's head passed through the os uteri; the violent shriek, and the rending sensation by which it was accompanied; I have little doubt that a laceration of the organ occurred; although, owing to the confusion of parts consequent on the presence of the spongy tumor, I did not detect any breach of substance; nor, indeed, was I anxious to disturb the tender structures by making a prolonged and very minute examination. For a fortnight she continued in imminent hazard; but at the end of a month was able to leave her room. I was in constant and almost daily attendance on this lady for fourteen months after her delivery, when she sunk, worn to a skeleton by pain, hæmorrhage, and serous discharges. So profuse was the exudation of that peculiar serous discharge, so eminently characteristic of cauliflower excrescence of the os uteri, that for some time before her death she was compelled to use three dozen napkins in the four and twenty hours, each of which was perfectly saturated with moisture.

When the os uteri is diseased, we shall mostly find it irregular, knotty, and very painful to the touch. Symptoms indicating diseased structure will probably have existed prior to labour; and—from the history of the previous sufferings alone—there can be little difficulty in ascertaining the nature of the case. If it be not dilated more than to the size of a shilling; if we find it very thick, tuberculated, and in part ulcerated; if it be very tender; and if there have been previous symptoms of uterine affection,—such as acute pains, occasional and irregular eruptions of blood, constant or very frequent, sanious, fœtid, acrid, or serous discharges from the vagina,—we can have no hesitation in pronouncing that the os uteri is in an unhealthy condition.

Treatment.—Under organic disease of the mouth of the womb, it is very possible that a natural termination may occur, as in two of the cases I have related. It would therefore be proper to delay the application of any means of relief, so long as is compatible with the patient's immediate welfare; moderating, at the same time, excessive action by opiates taken into the stomach, or exhibited per anum. Mostly, the patient's system will have been too much depressed, by the wasting nature of the disease, to allow of the abstraction of blood; nor, indeed, could we expect bleeding to be followed by relaxation of the organ, when structurally affected. Nevertheless, we must affix a limit to our passive treatment; for, as in more ordinary cases, a period may perhaps arrive, beyond which we cannot trust to nature. Should we observe, then, incipient symptoms of exhaustion; should the pains begin to flag; and should an increased quickness of pulse, a more anxiously-dejected countenance, or distressing attacks of vomiting, indicate impending danger, it would necessarily become an anxious question what means should be adopted in order to afford relief. Delivery offers the only chance of preserving the patient from immediate death. But, under the undilated state of the os uteri which I am supposing, it would be impossible to apply the forceps, or use any other means compatible at the same time with the child's existence and with the continuity of the mother's structures. We have, therefore, the choice only of either delivering by instruments, which must necessarily destroy the infant,—provided it be at the moment living,—performing the Cæsarean section, or dividing the diseased part to a sufficient extent to permit the child to pass. I presume no well-judging man would in such a case propose the abdominal incision; and we should, therefore, be driven to the alternative of either perforating the head, or making a division of the mouth and neck of the womb itself. Considering, then, that the woman labours under a disease which must terminate in death,—and that, probably, at no very distant period;—that the os uteri would most likely be lacerated in our attempts at extraction; that the incision would not necessarily be followed by fatal consequences,—whilst at the same time, after perforation of the head, the child must certainly be born lifeless,—I should prefer operating on the os uteri, unless, indeed, there were present the most unequivocal signs of the child's death: and I should even hope for the patient's survival for some time, being cheered by the result of the case last detailed, in which I have not the slightest doubt that a laceration occurred.

COMMON RIGIDITY.

The rigidity usually met with, however, is independent of diseased structure, and is known by the os uteri being hard, firm, and but in a slight degree painful—by its resisting the dilating powers of the membranes, or fœtal head—and by our not being able to make any impression on its edge by the finger. It is not likely that we can mistake a case of rigidity of the os uteri, for one in a more natural state. We must recollect, however, that rigidity of the soft parts may co-exist with a deformity of pelvis; and that each of these causes may, at one and the same time, tend in some degree to induce a retardation of the process of labour.

Treatment.—Under this simple rigidity of the os uteri, it is our duty, if possible, to

produce relaxation in the organ; and we are possessed of some means which have been esteemed highly efficacious. Observing how supple and distensible the os uteri becomes, and how readily it usually dilates under hæmorrhage, bleeding has very generally been adopted to effect this object. This is certainly a powerful, but by no means entirely a safe agent. From personal observation, I cannot help thinking that depletion has been much abused in lingering labours; and I fear it has been resorted to often unnecessarily. The great objection that attaches to bleeding at the commencement of labour, is, that after the child is born, there must necessarily be a certain quantity of blood lost. We are in perfect ignorance how much that may amount to; and it would be wanton to take blood from the arm without grave occasion, when the few ounces we may voluntarily abstract,—had it been preserved in the woman's system,—might have turned the vacillating beam of life in her favour, and snatched her from impending death. This, however, is but a remote, though probable danger; and it becomes a question, whether we ought to take into account a remote probability, when weighed against a state of actual and existing difficulty. My father has always used the lancet with caution under the first stage of labour, in consequence of the risk of flooding afterwards; and I, in conformity with his views, adopting his sentiments, and very much revering his opinion, seldom direct bleeding in the first stage of labour, for the purpose, simply, of overcoming rigidity. There are 2,300 women, in one charity, annually delivered under my immediate superintendence; these patients' labours are not of more than an average length, and there are actually fewer deaths among them than we meet with in the higher circles. Of this number I scarcely bleed one under labour, unless there be evidences of undue determination of blood to the brain, or symptoms of congestion or inflammation of some other viscus. At the same time it is right I should inform you, that my father, although he is adverse to indiscriminate bleeding in all cases of rigidity of the os uteri, considers it occasionally useful to soften and relax that organ. Dr. Dewees, of Philadelphia, considers it certain and never failing in its effects. Dr. Blundell, Professor Burns, and other eminent practitioners, strongly recommend it; and Dr. Hamilton goes so far as to assure us, that he can always relax the os uteri, at whatever time he likes, by bleeding; and that he never allows the first stage to continue longer than twelve hours, so completely has he the process under his control; he states, also, that he never has had a patient in labour more than twenty-four hours, except where disproportion existed, since he began this practice. There is certainly no doubt that the robust constitutions of the northern females bear depletion better than the comparatively weak systems of this metropolis. This observation also holds good in regard to the country: and if I were engaged amongst a race of peasants, I should, no doubt, more frequently have recourse to bleeding than I can dare to do in the situation where I am accidentally placed; having under my charge persons whose bodies are too frequently either broken down by early disease, shattered by an irregular course of life, or weakened by the want of proper nourishment, exercise, and air; and whose minds are rendered anxious, fidgetty, and fretful, by the toilsome, the feverish, the harassing, and often uncertain nature of their occupations.

With regard to bleeding, then, as a means of relaxing the os uteri, I look upon it as powerful, but not devoid of danger: to do good it must be carried far enough to make an impression on the general system; for it is idle to expect advantage will be derived from it, unless syncope, or at any rate a degree of faintness, be produced. But there are some constitutions which bear the loss of blood so ill, as to preclude the use of the lancet entirely; and yet in such we may possibly meet with preternatural rigidity. Even the warmest advocates for the depleting system acknowledge this to be the case; and other measures have consequently been had recourse to with the same view.

As second in importance rank opiate enemata. Opium, whether exhibited by the mouth, or in injection, — provided it be used in sufficient quantity, — will suspend uterine action, as well as relieve muscular spasm. If the contractions then are not powerful, it would be wrong to administer it. Opium is found of incalculable advantage in removing false pains, and is eminently useful during the first stage of labour, in those cases where the membranes have ruptured early — where the uterus is acting strongly and powerfully— where it is urging the head of the child against its undilated mouth; causing excessive agony; inducing irritability, fever, and nervous excitement; and producing no effect equivalent to the suffering endured. In such a case, if sleep can be produced, an opportunity of recovery is afforded to the system; and the woman gains strength to enable her to bear up against the fatigue necessarily attendant on such great exertion. Besides, during the time of inaction a favourable change may have taken place in the os uteri, predisposing it to dilate more kindly when the pains return.

On both these accounts, then, opiate enemata are useful when the pains are violent and irritating, and not producing advantage equivalent to the suffering they bring with them;—they procure rest and freedom from pain for a certain period; and in the interval of action they afford an opportunity to the os uteri to take on itself a more kind and favourable state. I am perfectly persuaded that opium has no positively relaxing power under labour, and that its virtues are entirely centred in its capability of moderating excessive action. The danger of opiates exhibited under labour is, that the uterine contractions may be so entirely removed through their agency, as never again to be established: and thus the case may be converted into one requiring the use of instruments—perhaps even of a destructive kind.

An infusion of tobacco, in enema, has also been suggested in rigidity: and Dr. Dewees has related a case in which two clysters were injected, an interval of an hour and a half intervening between their administration; but the alarming symptoms which supervened, prove the danger attendant on their use. This herb is very useful in reducing irregular spasm, and relaxing muscular fibre; but independently of its dangerous character, it is of no avail under labour, because it does not take away the rigidity of the os uteri: nor, indeed, should we, *à priori*, expect such an effect from its application; because the difficulty experienced does not arise from accidental spasm, or irregular fibrous contraction; but depends upon an originally firm, hard, rigid, and unyielding condition.

Common domestic enemata are most useful and valuable assistants under all cases of rigidity, both of the os uteri and vagina. They are serviceable by clearing the bowels, by acting as an internal fomentation, and also by amusing the patient's mind. By having recourse to such harmless means, we give her reason to think that she is not neglected, but that all is being done for her relief which art can accomplish: and thus both hope and confidence are inspired, and time is also gained for a full and fair trial of nature's powers; which negative virtue, indeed, is of equal, or perhaps greater advantage, than any of the more positively useful attributes of these applications.

Substances have been applied to the mouth of the womb itself, with a view to relax it: and belladonna has been recommended for this purpose; in France, by Chaussier, Velpeau, and La Chapelle; and in London by Dr. Conquest; but this practice has not met with the general sanction of the profession in this country. The extract of this herb was mixed with lard, in the proportion of a drachm to an ounce, and a small quantity rubbed on the os uteri, by means of the finger.

The knowledge of the extraordinary powers which this drug possesses in causing a dilatation of the iris, led to its employment, for the object of enlarging the aperture of the uterus: but there is certainly no similarity in the structure and office of the two organs; and no analogy can be drawn between their functions. It is not likely that this means will produce the relaxation we require; and if no good results from its use, it must be injurious;—not in consequence of the poisonous quality resident in the drug itself, but from the friction which is necessary for its efficient application. The mucus that naturally lubricates the part must be wiped away, and this irritation must predispose the tender organ to take upon itself inflammatory action.

It is the custom, also, in France, to inject mucilaginous fluids, as recommended by Gardien, and warm oil, into the vagina, for the purpose of softening the os uteri, and giving an extra degree of lubrication. I do not see the slightest objection to this practice, and in some instances it may be desirable and beneficial. Two or three syringes full might be thrown up once in every hour.

The warm bath has been recommended, and a trial made of its effect, by Dr. Smith, in America; but it is inconvenient in its use; it is not generally at hand; it tends to weaken the system, and is of no service in relaxing the part: it cannot, however, do much injury, unless indeed it may produce hæmorrhage, and perhaps, in some very rare instances, it might be of benefit, especially if there be preternatural heat and dryness of skin. External warmth applied to the vulva, as I shall presently recommend, in rigidity of the vagina and perineum, will occasionally be desirable; but it cannot have much effect on the os uteri.

Under a state of preternatural rigidity of the os uteri, it not unfrequently happens that, without any apparent cause, and independently of any means being used, sudden relaxation takes place; and from that time the labour progresses with much greater rapidity. This favorable alteration in the condition of the organ is generally accompanied by sickness; and I always hail an attack of vomiting under such circumstances as the harbinger of a fortunate change. I have before mentioned, that emetics had been recommended for the purpose of facilitating the dilatation of the uterine mouth, under the erroneous idea that the vomiting was the cause of the softening observed. Reverius, indeed, two centuries ago, remarked upon

this practice; and Lowder said, "he had often known spontaneous vomiting do good, but had seldom found benefit from the exhibition of emetics, though he had frequently seen them used." Only a few years since, an empirical practitioner of midwifery in Paris professed to render all kinds of tedious labour easy by the induction of vomiting; and, in spite of the authorities just mentioned, as well as many others, he boasted this as a novel practice. The most disgusting substances in nature have been advised at different times, to expedite parturition. Thus Hartman tells us, "Apud pauperes vidi sæpè partum difficilem solvi haustu urinæ mariti. Sic stercus equinum in vino expressum, et percolatum subitò fœtum, et secundas expellit." A midwife also, named Sarah Stone, who published some cases in 1737, gives several instances in which women in labour were made to drink their husbands' urine. Dr. Merriman well remarks, "If such horrible messes were ever serviceable, it was probably by inducing nausea and vomiting." Perhaps the effect on the mind, arising from the confidence with which they were advised, might also have had some influence.

Under rigidity of the os uteri, the forceps can never be available. Unless this organ, indeed, is perfectly dilated, or most easily dilatable, neither the long nor the short forceps can be used. I am not prepared to assert, that in some rare instances of rigidity the head may not require to be opened: it is seldom, however, that such an extreme case exists; for in time the organ usually gives way. Under such circumstances, indeed, we should wait until the last moment, consistently with the woman's life, before we think of destroying the child for the purpose of delivery.

Generally, in cases where the os uteri is rigid, it is found high, at the brim of the pelvis, or in its natural situation; but at other times the head of the child has descended into the pelvic cavity, covered by the thin expanded cervix; and the mouth of the womb is comparatively low, looking back towards the coccyx or sacrum. Such a case may be the occasion of much error and disappointment, unless it be clearly detected: for, on passing the finger for the purpose of making an examination, the tumor caused by the head will be distinctly felt occupying the pelvic cavity; and if we were careless in our examination, or were not acquainted with the possibility of the occurrence, we might suppose that the child would be born immediately. If we form an opinion to that effect, however, in such a case, we shall be greatly deceived; for many hours of wearying pain must be experienced before the os uteri will dilate in a sufficient degree to allow the transit of the head. The sensation communicated to the finger by the tumor itself will sufficiently indicate the nature of the case. Instead of feeling the denuded hairy scalp, we detect a smooth, polished surface; sensible,—perhaps acutely so,—to the touch; neither suture nor fontanelle will be distinguishable; and, on carrying the finger back towards the centre of the cavity of the sacrum, we shall find the os uteri opened perhaps not more than to the size of a sixpence or shilling; and through its orifice the head will be clearly perceptible. I have been sent for in more than one instance of this description, under the belief that the parts were sufficiently prepared for the immediate passage of the child; and that the head was sufficiently low to be extracted by the forceps. If attempts were made to apply that instrument in such a case, the blades either would not pass up at all, or if they were introduced, it must be by main force; a laceration of the cervix uteri would be necessarily occasioned, and the destruction of the patient would almost inevitably follow.

RIGIDITY OF THE VAGINA AND PERINEUM.

The vagina and perineum are sometimes so rigid as to prevent the exit of the child; with this there often exists also rigidity of the sacro-ischiatic and coccygeal ligaments, which adds much to the difficulty of the case.

This state much more usually occurs with first than subsequent children; in fact, simple rigidity of the vagina and perineum, when the patient has borne a a family, is very rare. Sometimes, rigidity of these organs singly may be the cause of delay; but it is much more frequently combined with the same condition of the os uteri.

There is little difficulty in detecting the existence of rigidity in the vagina and perineum; we may ascertain it by the firmness, dryness, narrowness, and want of distensibility, which characterize the state. The rigidity will sometimes exist to such an extent, that two fingers cannot be passed without difficulty up to the os uteri; and yet, even under this aggravated condition, the parts will most probably, in process of time, become relaxed, moistened, softened, and distensible; they will eventually dilate, and the case may be naturally terminated. Under this state, if the os uteri be widely open, and the pains be strong, great pressure will be exerted on the parts within the pelvis, and all the injurious effects of contusion and strangulated vessels may be eminently dreaded.

Treatment.—Here, also, it is our duty to endeavour to relax the rigid structures; with this intention, bleeding has been had

recourse to, as liberally and almost as universally as under rigidity of the os uteri itself; but bleeding certainly does not possess the same power in this as in the case just now under consideration. I am inclined to limit the use of the lancet to those instances where the rigidity is combined with heat, tumefaction, unusual tenderness and unnatural dryness,—symptoms which denote that injurious pressure has taken place, and that inflammatory action has commenced. Opiate injections have also been generally adopted; but they seem neither of so much avail as in rigidity of the os uteri, nor indeed are they so much called for; because there is not so much distressing pain experienced as when the head is pressing strongly against the hard, undilated, os uteri: but if the pains are exceedingly violent, opiate enemata may be desirable. Simple domestic clysters are also of essential service, and may be used in any case.

Warm fomentations are sometimes of great advantage. Flannels may be dipped in hot water, or a decoction of poppy-heads, and applied to the labia externa and perineum. They may be continued, with little intermission, for four or six hours at a time. The warmth is grateful to the patient, and the relaxing influence has sometimes appeared evident. Another means of applying warmth externally is by desiring the patient to sit over the steam of warm water, provided she can maintain the sedentary position without pain: one principal use, however, of all these means is to gain time, so as to allow the natural powers a full and fair trial, and at the same time to convince the woman that our mind is directed towards affording her relief. Warm oil might be injected into the vagina, if the parts were dry, and harsh and hot; and if the head were not lying too low to prevent the introduction of the fluid: but, generally, this will not be practicable; and lard will be found a more easy and useful application. The external parts may be lubricated by a little occasionally smeared over them; and a small portion may be carried as high as possible within the vagina, and allowed to melt there. I have often found this cooling application very grateful to the patient; and have fancied that, at the same time, it has tended to produce relaxation. I must, however, caution you strongly against unnecessary meddlesome interference: all *rubbing* must be avoided; and if this lubrication is used at all, it must be in the tenderest and gentlest manner; for much more injury will accrue from denuding the parts of their natural mucus than good from the artificial moisture which the unctuous substance affords.

It is scarcely necessary that I should repeat the cautions I before gave you respecting the exhibition of ergot of rye in cases of lingering labour connected with rigidity; but I will relate a case, illustrative of its powers and its dangers:—Late one evening, in the beginning of the year 1829, after a very fatiguing day, I received a message from a midwife, requesting my attendance on a patient in labour of her tenth child. I was informed that the membranes had been ruptured more than twenty-four hours,—that the breech was in the pelvis,—that the uterus had acted very feebly from the commencement of the labour, but particularly so since the discharge of the waters, and that the whole cause of delay seemed to be an insufficiency of pains. I directed an intelligent pupil, at that time resident in my house, to accompany the messenger,—to take with him some ergot, and to exhibit it, if he thought the case fitted for its use. He gave half a drachm infused immediately, and another dose of equal strength half an hour after. Ten minutes had scarcely elapsed from the administration of the second quantity, when the uterus began to act most powerfully; in ten minutes more the child was born,—wholly without artificial assistance,—and the placenta passed quickly, with very slight discharge. He returned quite delighted with the powers of the drug. Early in the morning, however, I received a second summons, stating that the patient had experienced violent pains all night, had lost a large quantity of blood, and appeared very ill. On my arrival I found her recovering from a state of faintness, and complaining of acute suffering at the lower part of the person. A copious discharge of blood had flowed from the vagina, as was evidenced by the appearance of the room; for a large quantity had soaked through the bed, and lay in a pool upon the floor. On placing my hand on the uterus, I found it exceedingly well contracted, hard, and by no means tender; and it was plain that the discharge had not proceeded from that organ. Examining further, I discovered that the right labium was very much distended, and painful on pressure being applied. There was, indeed, a longitudinal laceration just within, extending the whole length of the labium; and the cellular structure of the part was filled with a very firm coagulum. On the removal of the clot, an oozing of arterial blood was perceptible, which, however, was restrained by the use of pressure and other means. An opiate procured sleep. In a few days healthy granulations made their appearance; in little more than a fortnight the cavity was quite

filled up, and a cicatrix of about two inches in length shewed the situation and extent of the injury.

More recently, I was called, in consultation, to a case in which the uterus had ruptured after the exhibition of a dose of ergot. The accident, however, might have occurred had the medicine not been given; there was a slightly distorted pelvis.— Such are the dangers likely to arise from the administration of this drug, in cases unfitted for its use.

It is a very common practice with the attendants in lingering labour to urge the patient frequently to take stimulants, under the idea that her strength must be very much exhausted, and that some extraordinary means are required to sustain her. No custom can be more injudicious. I have already hinted at the dangers which must spring from its adoption; but it is particularly to be deprecated in cases where rigidity is the cause of delay: for, by increasing the power of the uterine contractions, stimulants would have the effect of forcing the head strongly against structures unprepared to admit it, and,—independently of inducing fever and premature exhaustion,—might occasion laceration of the organs which refuse to yield. For the same reasons, all voluntary efforts on the part of the patient must be restrained as much as possible; and—if from ignorance or obstinacy her officious friends persist in urging her to call those powers which are under her control, to the assistance of the uterine energies,—the injurious tendency of this advice must be candidly and plainly pointed out.

When the head presses on the perineum, the extended structures must be supported constantly and anxiously, lest they should rupture: the more rigid the parts are, indeed, the greater must be our assiduity; and, as I before told you, you will find this occasionally a most irksome duty.

CICATRIX IN THE VAGINA.

A cicatrix in the vagina, the result of sloughing under previous protracted labour, will occasionally be found to impede delivery. When the healing process is established in the ulcer, which is left on the separation of the slough, a puckering of the vaginal membrane takes place; the surface is diminished in extent, and consequently the diameter of the canal is lessened. In porportion to the extent of the slough, in general, will the difficulty be. The history of the case will be in itself almost sufficient to enable us to judge of the nature of the impediment. We shall find that the patient will have suffered one or more lingering, and probably instrumental labours; that symptoms of inflammation of the vagina occurred after one of the deliveries, and that her convalescence was protracted. On making an examination, we shall detect, at some portion of the vaginal surface, a fibrous unyielding band, preventing the passage of the head. The edge of this band may be as thin as paper, or it may run up for a quarter or half an inch in length, narrowing the canal to that extent longitudinally.

It is very possible that nature unaided will overcome the difficulty offered by a cicatrix in the vagina; and it would, therefore, become our duty to wait a moderate time, that we may give her an opportunity of surmounting the impediment. Either relaxation may occur to such a degree as to allow the child to pass, or the fibrous band may lacerate under the strength of the contractile powers. Should the desirable softening, however, not take place,—rather than run the risk of extensive contusions, by the continued residence of the head in the pelvic cavity—rather than have recourse to forcible attempts to deliver by the forceps,—it would be right to enlarge the passage artificially. Four slight incisions may be made into the edge of the constricted part: one towards each sacro-iliac symphysis, and one behind each groin, avoiding particularly the neck of the bladder, the rectum, and the large uterine arteries which run up from below, one on each side the vagina. If four incisions be made, the least snip that can be formed will usually be sufficient; for it is more than probable that the aperture will be widened by laceration: and I am inclined to think this would be preferable to making an extensive cut; because of the danger we incur of wounding, not only the rectum or bladder, but also some of the large vessels with which the vagina is so liberally supplied. It would afterwards become a subject of consideration, whether the case should be left to the natural powers, or whether instrumental means should be resorted to to terminate the labour. The answer to such a question must entirely depend upon the peculiar circumstances attendant on each ease. After delivery, when the healing process begins to be established, care must be taken that a narrowing of the canal does not again occur; and this would be best prevented by the introduction of a piece of sponge, dipped in oil, to act as a tent, and preserve the vaginal parietes distended.

Notwithstanding the high authority of Dr. Dewees, I should by no means trust implicitly to the abstraction of blood for the purpose of procuring relaxation of the cicatrized and constricted membrane. The American practitioners, however, are in

the habit of carrying depletion, with this intent, to a degree which we seldom hear of in England. Dr. Dewees has given three cases, in which he attributes the relaxation entirely to this means. In one of these instances, however, between 65 and 70 ounces were drawn at two bleedings; and another of his patients lost "upwards of two quarts" of blood at one operation, through the agency of the laneet.

UNRUPTURED HYMEN.

Impregnation has occasionally been effected although the hymen has never been broken; and if this membrane remained entire till the period of labour, it would form a greater or less impediment to the passage of the child. I have been consulted in one case of this description, and another has come under my father's personal observation. Such a case of protraction must of course be met with in a first labour; and by this circumstance it could be discriminated from a cicatrix, the result of previous sloughing. Its situation would be just at the vaginal entrance, and its form would also assist us in determining its nature. The aperture must be dilated, if possible, by mechanical means; and if that cannot be effected, the case must be treated in every respect upon the principles just laid down for the management of a cicatrix.

OBLIQUITY OF THE OS UTERI.

The last cause of delay attributable to the mother is obliquity of the os uteri; and this has been much insisted on by the French writers. It is certainly true, that when women have borne a large family the abdominal muscles become relaxed, lose their tone, and cease to afford that support which the gravid uterus ought to derive from them: the abdomen consequently becomes pendulous; the axis of the uterus, in respect to the person, is changed; its fundus is thrown forwards, and its mouth is directed too much backwards against the sacrum. The upper part of the uterus has also been observed to fall to the right or left side, and the mouth to be turned towards the opposite ilium. Under such circumstances, we are recommended to place the patient either on her back, or on the right or the left side, as circumstances may require, in order to admit of the body and fundus of the uterus gravitating in the proper direction. We are also instructed to draw the uterine mouth more into the centre of the pelvis, by the fingers hooked within it.

As far as the change of posture is concerned, I can have no objection to the treatment; and I would moreover endeavour to restrain the uterus in the necessary situation, by a bandage girt with moderate pressure round the person: but I am decidedly opposed to any forcible attempts being made to pull the os uteri into a more convenient situation; lest it should be bruised or excited to inflammatory action, by the irritation necessarily attendant on our endeavours: and I have at best very little faith in obliquity of the os uteri producing serious protraction, unless indeed there be present also more or less rigidity.

LUCHTMANS ON THE QUESTION OF VENOUS ABSOPTION.

To the Editor of the Medical Gazette.

Sir,

If the following translation of some observations on venous absorption, selected from a Latin dissertation, by Dr. Luchtmans, on the distinction between healthy and diseased absorption, be worthy of a place in the Medical Gazette, I shall feel obliged by its insertion.

Your obedient servant,

C. J. B. Aldis, M.A., M.B.

13, Old Burlington-Street,
April 15, 1834.

Do the veins absorb?

Having clearly proved that portions of the body are absorbed, it has not been so evidently established by what means this action may be accomplished. Mention is made by Hippocrates * and Galen † of absorption and inhalation, which they believed were effected in the human body through the agency of the arteries and veins. But when Harvey, in the seventeenth century, had fully illustrated the circulation of the blood, from which it was plain that the blood was conveyed by the arteries from the centre to the periphery of the body, the power of the arterial absorption was deservedly rejected ‡. Not long after the discovery of the circulation of the blood, Casparus Asellius (anno 1622) found vessels filled with a whitish fluid in the mesentery of a dog, and on the surface of the intestines, which he named lacteals and lacteal veins. Already some idea of these had occurred in Herophilus and Erasistratus (300 years before the birth of Christ); and even in the year 1556, Bartholomæus Eustachius saw the thoracic duct in a horse, but mistook it

* Epidem. L. vi.

† Comment. in Hippocr. Epidem. L. vi.

‡ P. J. Van Maanen, Diss. de Absorpt. solidorum, p. 15.

for a vein, and called it the white thoracic vein; therefore, many physiologists have assigned the function of absorption to these vessels, discovered by Asellius in the first instance; after that Olaus Rudbeck (anno 1650) more fully demonstrated the thoracic duct, both its origin and continuation from the lacteal veins.

When diligent investigation taught that the lacteal vessels, in appearance at least, differed somewhat from the lymphatics, but that this kind of vessels was commonly dispersed throughout the body, physiologists began to doubt the seat of absorption and its apparatus, moreover, to divide into opposite opinions. For some regarded venous absorption to be proved by the testimony of the ancients; others thought that this action was to be attributed only to the lymphatic vessels.

Glisson, Bilsius, and Swammerdam * are among those who supported venous absorption, who, having tied the mesenteric veins in a live animal, and after some time having opened them, saw the blood contained in them mixed with white striæ, and imagined that this matter was absorbed from the intestines by the veins. But some explain these striæ to consist of coagulable lymph; others of chyle not well mixed with the blood, and more or less dispersed through the whole body; while Hewson and Rudolph determine that that chyle was received from the arteries.

It was the opinion of Haller that veins, with the power of absorption, arose with small open mouths from the cavities of the body, and the cellular tissue, &c., as appears from various passages in his Elements of Physiology †. He also derives jaundice from the bile absorbed through the sanguiferous veins‡. Others, as Rosen, Waller, Meckel, Lobstein, and Sir Astley Cooper, have observed, that mercury injected into the lymphatic vessels, passed into the branches of the vena portæ. Tiedemann and Fohmann have found this anastomosis to exist in the glands.

K. Boerhaave (anno 1730) injected water into the stomach and intestines of a dead dog, and after continual pressure for several hours, writes that it entered the mesenteric and gastric veins, and propelled the blood *. But many persons, and among others Dr. Van De Sande†, contend, in order to render this argument unavailing, that the water penetrated through the organic pores, or through a rupture. J. F. Meckel filled the vesiculæ seminales with waxy matter, having tied the excretory ducts, and observed this matter penetrate the minute branches of the hypogastric vein. So, also, water injected into the bladder of a male subject passed into the same vein ‡. Neither Cruikshank nor Dr. Oudeman succeeded in this experiment; wherefore they suppose the injected matter passed into the veins through ruptured vessels §.

Moreover, others have alledged the deficiency of lymphatics in some parts, as in the placenta, which, however, appears doubtful; and that blood effused into the cells of the corpora cavernosa penis is taken up by veins ||. They also adduce the capacity of the venous system, which far exceeds the arterial, and the small dimensions of the thoracic duct, as if it were not sufficient to receive and propel all the moisture which is contained in the lacteal and lymphatic vessels ¶. But even the slower motion of the venous blood corresponds with the greater capacity of the veins.

Here may be mentioned the arguments derived from tying or wounding the thoracic duct, such as Haller ** and Flandrin †† bring forward, the lesion of which animals have survived a long time, seeming to prove that other channels exist besides the thoracic duct, by which chyle passes into the blood. Nevertheless, it is evident that these arguments are equally incapable of proving venous absorption, since a double thoracic duct has often been discovered.

Hence it appears that the arguments produced by many of the ancients to demonstrate venous absorption, have not placed this subject beyond all doubt;

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* Vid. Oudeman, Diss. de venarum imprimis meseraicarum fabrica et actione. Gronin, 1792, p. 90.

† Tom. i. p. 151.; tom. vii. p. 47; ubi dicit; "Facilis etiam et patula via ab iisdem (meseraicis) venis est in intestinum," et Tom. vii. p. 89.

‡ Sömmering, de morbis vasorum absorbentium, p. 122.

* De Perspir. dicta Hippocr. Leidæ, 1738.

† Diss. de Venis lacteis, illarumque agendi modo. Gronin. 1784, p. 66.

‡ Experimenta et observ. de finibus vasorum, Berot. 1772, p. 49.

§ Oudeman, i. c. p. 28.

|| Schreger, Fragm. Anat. et Physiol.; fasciculus I. Leipzig, 1793.

¶ Oudeman, i. c. p. 115.

** El. Physiol. tom. vii. p. 68.

†† L'Esprit des Journ. Oct. 1791, p. 375, seq.

neither do the arguments taken from the incubation of eggs prove more,—in which veins are present before arteries and lymphatic vessels: for a conclusion on so imperfect a state is scarcely tenable with respect to the human body.

Flandrin was the first who at length studied to recal the learning of the ancients on venous absorption; being urged by the observation, that the blood of the mesenteric veins in a horse had a peculiar aromatic odour, and analogous in taste to urine, which the blood in the remaining veins in the body did not possess. He injected into another horse half a pound of assafœtida, dissolved in honey, and smelt it in the venous blood, but not in the chyliferous vessels *. Magendie having succeeded him, instituted the following experiments. He separated in such a manner a part of the small intestine of a dog, which had been previously well fed, that it adhered to the body only by one artery and vein: one extremity of this portion being carefully tied, he injected the poison upas, and prevented its effusion by a ligature. Death followed as soon as if the poison had been introduced into the sound part of the intestine; appearing to him a proof that this poison was absorbed by the small branches of the remaining vein. Since, however, the slender twigs of nerves probably penetrate the coats of the artery, and follow their course, doubts can be started whether these poisonous effects cannot be explained by an affection of the nerves themselves.

He separated the thigh of a dog from the rest of the body, previously stupified by opium, so that only the crural artery and vein remained, having removed the cellular coat of these vessels, lest lymphatic vessels might accidentally pass through this. He then injected two grains of the same poison into the foot of this side; the symptoms appeared as soon, and as powerfully, as when the poison is applied to a sound foot†.

The experiments were repeated with the same result by Lawrence and Coates. It is, indeed, supposed that the poison was placed in contact with the blood itself, and passed into the circulation by a way made in this direction; but Emmert, in experiments performed upon frogs, introduced the poison between the skin and muscles, and the same result ensued. It would, however, be difficult to show clearly that the application was made without injury to the smaller vessels.

Fodera filled a portion of the small intestine of a live animal with a solution of prussiate of potass: having tied the intestine on both sides, he saw, after he had placed the intestine in a solution of sulphate of iron, the lymphatic vessels and mesenteric veins tinged with a bluish colour *. My most esteemed instructor performed this experiment, after the interval of a year, and could only detect absorption of the prussiate of potass in the lymphatic vessels, but not in the veins. The prussiate of potass contained in the portion of the intestine was not changed in colour after half an hour, so that the sulphate of iron had not penetrated the walls of the intestine†.

According to Tiedemann and Gmelin, colouring, odoriferous, metallic, and saline matters, are, in the first place, absorbed by the mesenteric veins; the chyle especially, by the lacteal vessels. White striæ were also observed in the blood of the vena porta by these celebrated men, which they explain from the anastomosis of the lacteal vessels with the sanguiferous veins. Lawrence and Coates detected a solution of prussiate of potass both in the chyle and in the blood, although more abundantly in the blood‡; the contrary was observed by the Society of Philadelphia§.

At length, Mayer injected a solution of prussiate of potass into the trachea of a rabbit; he found this sooner in the blood than in the chyle; and in the left ventricle of the heart before the right: the same thing occurred, although the thoracic duct had been tied; which, indeed, seems to prove that this solution was absorbed by veins from the bronchial extremities, but not by lymphatic vessels. The same person observed in a man who died from pulmonary affection, the veins of the small intestines, at least the minuter branches running on the surface of the small intestine, filled with a grey matter, similar to chyle, which were visible to the naked eye even at the margin of the valves of Kerkring. The greater trunks contained blood: the lymphatic vessels were empty, which difference he attributed to the venous

* Magendie, Physiol. tom. ii. p. 263.
† Ibid. tom. i. p. 25, sqq.

* Magendie, Jour. de Phys. tom. iii. p. 80.
† J. Koker, Diss. de subtiliori membranarum serosarum fabrica, p. 61.
‡ Lund. l. c. p. 65.
§ Ibid, p. 64.

system living longer than the lymphatic, or from the right side of the heart dying later than the left.

There is certainly a great difference between the manner in which functions are performed, during health, in the living body, and that in which they are exercised, and after vivisections have been instituted, by which, in addition to the other injuries, the nervous system, at least in many, is violently disturbed. Besides, the substances introduced or applied, in order to illustrate this or that physiological question, in the greatest degree differ from the nourishing matter naturally swallowed. Although, therefore, it would be very hazardous to apply rashly to the human body all opinions which depend upon experiments, nevertheless, we think it can be deduced from the experiments by Mayer and Tiedemann, that their opinion is not devoid of all appearance of truth,—that the power of absorption exists both in the lymphatic vessels, the mesenteric and pulmonary veins.

Another question is added, which has lately been agitated by physiologists, —whether another passage of the lymphatic vessels into the veins exists, besides the thoracic duct alone; either the insertion of several into the left subclavian vein, or occasionally into the right; whether, therefore, the power of absorption is peculiar to veins,—whether they convey what the lymphatic vessels pour into the veins by anastamosis.

Already anatomists have observed, while injecting the lymphatic glands, the veins sometimes filled with quicksilver*. It happened to J. F. Meckel, who saw mercury had penetrated the vena cava, after he had injected a lumbar gland†, which Hewson, Cruikshank, Mascagni, &c., attributed to ruptured vessels. Falconar‡ and J. F. Meckel, jun.§, have observed the same thing. G. Vrolsk relates that he perceived such a communication in the phoca vitulina. Beclard affirms he often saw the passage of mercury both into the lymphatic vessels and the veins of glands, which, however, he did not attribute to the rupture of vessels‖. V. Fohmann, who scientifically investigated this branch of minute anatomy, found the same in man; moreover, in horses, cows, cats, &c. From many of the glands in dogs the mercury passed only into veins. The lymphatic vessels of the small intestine in the phoca marina, appear to terminate only in the mesenteric veins; the same obtained in the bronchial gland.

He also observed in fish, as in the *torpedo marmorata*, and the *esox lucius*, an abundant anastomosis between the lymphatic vessels and veins in the neighbourhood of the heart, and on the surface of the organs subservient to digestion.

Many observations worthy to be read, on this subject, which have produced a similar result, may be found in Lauth*. Even the greater trunks of the lymphatic vessels, according to Lippi, are inserted in man into the renal vein, cava, and vena portæ. Outside the glands he has even represented, in plates, a horse and goose, having this complicated anastomosis; but Fohmann has proved this author to have mistaken sanguiferous vessels proceeding from glands, for lymphatic.

Hence, indeed, it seems to be proved that a communication exists between the lymphatic vessels and veins. Granting this to be true, many arguments alleged in favour of venous absorption would be invalidated; for then, to adduce an example, the chyle, already in the lymphatic glands, might be mixed with the blood in greater or less quantity, by the assistance of the veins.

It remains for us to notice, that even in our time physiologists might be cited, who strenuously deny both absorption of veins, and their anastomosis with lymphatic vessels, among whom Th. Soemmering and Rudolphi are particularly to be mentioned; for their injections do not show any passage of mercury from lymphatics into veins, neither in the dog, dolphin, birds, amphibious animals, nor even in fish; but if mercury should have passed into the veins, they suppose it attributable to ruptured vessels. The lymphatics of the small intestine in the phoca marina extend to a congeries of glands, which is commonly called pancreas Asellii, the lymphatic vessels proceeding from which converge into a duct of marked diameter, to be inserted into the thoracic duct itself;—an observation

* Haller, o. l, tom. l, p. 177.
† Nov. Exper. et Observ. p. 7.
‡ Experimental Inquiries: London, 1777; p. 46, *nota*.
§ Lindner, Spec. Med. de Lymphaticorum Systemate, p. 78.
‖ Anatomie Générale, 476.

* Rudolphi, l. c. page 256.

opposed to Fohmann. Rudolphi especially made the following objections to venous absorption:—1. The difference of structure between the lymphaties and veins. 2. Fluids injected into the cavities of dead animals are taken up by the lymphaties, but not by veins;—which argument does not affect the experiments instituted in living animals by Tiedemann and Meyer. 3. All odoriferous matters penetrate organs, but are not found in lymphatie vessels, because their nature is changed in the glands. Tiedemann found not only odoriferous, but also colouring matters, in the blood of the portal veins. 4. He deduees that some substances are found in the blood, but not in the ehyle, both from peeuliar causes moderating the absorption of these within the vessels, and from the chemical analysis of an organic substance not as yet sufficiently perfeet*. But if chemieal analysis could deteet these substances in the blood, I do not see why they eould not be diseovered in chyle, whieh is as yet imperfect blood, and more simple. 5. According to his opinion, matter analogous to ehyle existing in the blood, was not absorbed at first by the mesenterie veins, but was changed into genuine blood during sanguification†.

My valued instructor performed the following experiment before many pupils, in order to establish whether the passage of quieksilver from the lymphatic vessels and glands into the veins, was to be ascribed to ruptured vessels:—He carefully sought for the lymphatic vessels of the left fore paw in a well-fed dog, whieh had been quiekly killed by prussie aeid, and injected with mereury a little vessel near the carpus; and pursuing the eourse of the lymphatic vessels to the axilla, there we saw a network of lymphatie vessels, conspicuous on aecount of the eomplieated anastomosis, with distinctly turgid valves, filled with mereury. All these lymphatie vessels entered the axillary glands. One larger gland, situated near the insertion of the eephalic vein into the axillary vein, was entirely filled with the quicksilver, without any rupture of the vessels or effusion of quicksilver, whieh could not be so easily deteeted in the other glands or lymphatic vessels. We were unable to diseover, by the most accurate examination, any other lymphatic vessels going out from the other side of the glands; but a vein filled with mereury went out, terminating in the subcutaneous vein of the neek, which, like the jugular vein, contained mereury. A lymphatic vessel going out from another smaller gland, partially filled with mereury, appeared to extend to a greater gland. He injected a little mercury into a lymphatie vessel of the other foot, and separated it from the body, with the gland and veins which proceeded from the other side of the gland, and terminated in greater veins. We could not detect any lymphatic vessel going out. The parts being arranged on a table, he introduced a tube into a lymphatie vessel, towards the gland, when, after moderate pressure, we saw by the aid of the eolumn of mercury, all the lymphatic vessels of the gland in nodules—not only filled, but the mereury soon began to flow from the veins. Upon inereasing the pressure, this vein was turgid with mercury, when a ligature was passed round it. We eould not deteet in this experiment any rupture of vessels, after the most accurate examination. At length, having opened the thoraeie eavity, we saw the thoracie duct turgid with transparent ehyle, without a globule of mercury; whieh proves that mercury, in the former experiment, did not pass from the thoracic duct into the veins.

Should any one wish to establish that the mercury passed from the lymphatic vessels of the gland by ruptured vessels, after sueh slight pressure, it would be necessary, after the inereased pressure, that the mercury should be effused from this rupture not only into the vein, but also into the cellular tissue surrounding the gland, so as to be extravasated; whieh was by no means the ease.

Afterwards, an opportunity offering itself through the kindness of the veterinary professor, Numan, my instruetor endeavoured to repeat these experiments in a eolt. He filled with mereury the lymphatic vessels in the groin, at the os humeri, and in the mesentery; but after the mercury had entered the glands, an effusion of mereury always oeeurred from ruptured vessels. After employing less pressure, the glands, indeed, were filled with mereury; but the mercury neither passed from sound, nor from injured and ruptured glands, into other vessels, so that neither the lymphatic

* Rudolphi, i. c. page 256.
† Ib. i. c. page 245.

vessel going out, nor the adjacent vein, was filled. Perhaps the parts were more or less putrifying, from the increased temperature of the atmosphere, although no putrid odour was evident in the body. If the passage of the mercury from the glands into the veins was the result of rupture of the vessels, I do not see why we no where observed mercury passing from these glands into veins, but into the cellular tissue between the lymphatic vessels of the gland, which was expanded into large vesicles.

THE FŒTAL GENITAL ORGANS CONSIDERED IN REFERENCE TO HERMAPHRODISM.

To the Editor of the Medical Gazette.

SIR,

SHOULD you deem the following observations on the genital organs of the fœtus, worthy of a place in your exeellent journal, their insertion will much oblige,

Your obedient servant,

A. F. MOIR.

Arundel, March 29, 1834.

Looking over a few fœtal preparations, varying from the second to the sixth month, my attention was easually direeted to the eonformation of their genital organs. The appearances (by a cursory examination) were at first believed to be the result of malformation. On a more minute and eomparative view, we were, however, satisfied that it must be considered as the normal development of those organs in the earlier stage of organization. The subject, as far as I am aware, has not hitherto been investigated; I would therefore eaution your readers neither roundly to eondemn, or implicitly believe, the following statements, until they have examined the subject for themselves, whieh ean be easily and satisfactorily accomplished by any one having access to a series of such preparations.

In a fœtus between the second and third month, examined by a mieroseope of a medium power, the rudimentary penis eould be distinetly observed inelined downwards; when elevated, its lower surface was seen cleft in the course of the urethra, with a similar cleft or fissure, bordered by a slight projecting fold of integument, extending from its base, and in the mesial line, to near the margin of the anus.

In two or three others more advaneed, a similar eonformation of parts could be distinetly seen with the naked eye. The cleft in the urethral surfaee of the penis was continued in the direetion of the bladder, further than we could deteet even by the aid of our mieroscope; but as yet, not having disseeted the parts, we are unprepared to state whether or not it may be continued as far as the orifiee of the bladder. The *scrotal* cleft, or, from the now increased development, more properly *fossa*, was of a triangular shape, its base being towards the penis, and the marginal projeeting folds of integument meeting, and slightly united at its apex, whieh (excepting the penis) might have readily been mistaken for the embryo organs of the female. In one fœtus in particular, between the third, or about the fourth month, as nearly as eould be ascertained, the parts were so distinetly seen, that had it been in the absence of further, or eomparative illustration, we should have had no hesitation in considering it as the usual anormal condition, tending to, and explanatory of, the nature of the anomalies occasionally occurring in these parts. The penis was eonsiderably developed, covered superiorly by a continuanee of the common integument of the abdomen, and inclined downwards: when elevated, a similar condition of the parts, as already described, in a progressive state of development, was distinetly observed. This preparation is valuable, inasmueh as it must invalidate any objection that might be urged against us, in eonfounding the penis and clitoris; a mistake whieh ean be easily avoided by eonsidering the locality of the two, and *that* in reference to the parts in their immediate vicinity.

From the above sketch of the organization and development of the fœtal genital organs, studiously endeavoured to be faithfully described, and as concisely as consistent with perspicuity, we deduce the following inferenees:—That prior to, or between the fourth or fifth month, (for we cannot be precise as to the exact period,) the penis consists of the corpus cavernosum, or union of the eorpora cavernosa, covered superiorly by the eommon integument of the abdomen; and inferiorly, a cleft or fissure (terms

vague, but suffieiently expressive) is observed in the eourse afterwards to be occupied by the urethra, (the formation of which, with other peculiarities, we have not yet examined); that a similar cleft extends from the base of the penis, in the mesial line, to near the margin of the anus, bordered by projecting folds of integument, which gradually inerease, and by their union form the scrotum; that, by the retarded organization of either of these structures, we can readily aecount for the various speeies of genital malformation, known by the general name of *hermaphrodism*, and other *irregularities* oeeasionally met in these organs. For the present, however, we forbear to generalize further, well knowing that the importance of the subject preeludes all speculation; and that it is only by a patient and minute examination of a series of fœtal preparations that the *truth* can be elieited.

Indeed, the ehief object we have in view in soliciting the insertion of the above remarks in your excellent journal, is the desire that others, whose opportunities are more extended than our own, may co-operate with us in an investigation at onee so interesting and eurious; otherwise eommon prudence should render us cautious in communicating observations so crude and meagerly *jumbled together*.

FUNCTIONS OF THE FIFTH PAIR OF NERVES.

To the Editor of the Medical Gazette.

Sir,

I may be allowed to put Mr. Walker right upon an important faet bearing upon his theory of the motions of the iris. There is no foundation for the opinion which he maintains—that the ophthalmie branches of the fifth pair of nerves have the power of bestowing motion on the parts which he describes; neither ean he derive any support, for this part of his theory, from any thing that Sir Charles Bell has written on the nerves of the orbit. In the very first communieation which that gentleman made to the Royal Society, on the Nervous System*, he related experiments to prove that when these ophthalmic branches emerge upon the forehead, they are simply nerves of *sensation;* and when he spoke of the fifth pair being a motor nerve, he carefully restrieted its power to the *muscles engaged in mastication.* The title which he bestowed upon the whole nerve, in that original paper, sufficiently proves that he did not believe the ophthalmic branches to possess any influence over the muscles; for he designated it the "nerve of sensation and masfieation."

Mr. John Shaw's experiments* upon the fifth pair (at which I assisted), were the first to demonstrate that the third division alone, of the three great trunks which compose this nerve, possessed any power over the muscles.

Although Mr. Thurnam† drew his attention to the subjeet, Mr. Walker has omitted to refer to the funetions of the roots of the spinal nerves, and to the correspondence between them and the roots of the fifth pair; yet if he will only examine how decisively the experiments made upon the one set of these nerves corroborate those made upon the other, he will, I think, be led to modify his theory; and to eonelude, with Sir Charles Bell, that the fifth pair is only a nerve of motion, as it is connected with the organ of mastieation.

I am, sir,
Your obedient servant,
Alexander Shaw.

April 26, 1834.

* 1821.

ON THE ABSURD SCHEMES OF CERTAIN MEDICAL REFORMERS.

To the Editor of the Medical Gazette.

Sir,

Though now residing in the country, removed from the "*Fumum et opes strepitumque Romæ*," and daily occupied in the exereise of my profession, I oeeasionally amuse myself as I ride along the lonely roads, or in the subsequent seelusion of my study, by perusing the periodieal publieations which fall in my way; and cannot forbear smiling at the "fantastie tricks" which are constantly played in the present day by the

* See Med. and Phys. Journal, Oct. 1832; and Sir C. Bell on the Nervous System, 4to. pp. 62 and 104.

† Medical Gazette, March 29.

different aspirants to notoriety in medicine. It diverts me to observe that the bubble of reform, in which so many victims have been swamped in the political world, has extended to medicine; and to watch the little splashings which so many *medici* and *mediculi* are making in the spray, instead of venturing into the depths of the ocean, in which their little life-bark has by some fatality been launched. Reform and universal knowledge are the cry; and, deafened by the shouts of the former, and dazzled by the blaze of the latter, the thoughtless multitude press on, without being sensible of the "*vox et præterea nihil*" of the one, or the *ignis fatuus* of the other. The temple of Hygeia seems indeed to be converted into a tower of Babel, wherein all the workmen are jabbering at once, and where no one understands what his co-operative would fain convey, even should he enjoy the enviable distinction of clearly comprehending his own meaning.

But to be serious. The grand object appears to be the annihilation of all grade in the medical profession—to make every member of it a "servant of all-work"—to disregard all the decomposition consequent on the *fiat mistura* of what will not incorporate—and thus convert what has hitherto, on the average, worked well, into what seems calculated only to disagree.

The physician, surgeon, and apothecary, are to be henceforth a "*Tria juncta in uno*"—trinity is to be unity, and unity trinity (if I may be permitted to use such an illustration without any intention of impiety); and the boundaries of medical rank are to be trampled on and destroyed! But let me understand it correctly: is the physician to imbrue his hands in blood, and the surgeon lay aside the bloody knife and stain the paper on which he seeks to write? Is Sir Henry Halford to lithotomize, whilst Sir Astley Cooper prescribes for a fever? or is Sir Henry first to prescribe, then operate on the patient, and afterwards prepare the medicine for which he has just written the formula? —or what is this "One Faculty" mania which rages? "I pause for a reply." It surely cannot be so: I hope and trust I am mistaken; or I do not hesitate to aver my opinion, that what little dignity the medical profession may at present enjoy (and, God knows, it is little enough!) will henceforth be annihilated: the title of physician will be lost in that of general practitioner; and the science of medicine, which, when viewed in its proper light (I mean that of extracting from the animal, mineral, and vegetable world the remedies intended by God to relieve the "various ills that flesh is heir to"), is entitled to a proud ascendancy over all others, will sink into obscurity, and become as degraded as it is on the Continent, and most parts of the habitable globe. Let me not, in giving vent to these sentiments, be subject to the imputation of casting any disrespect on the surgeon or the apothecary: such is very far from my intention—

> "For Brutus is an honourable man—
> So are they all, all honourable men."

But I appeal to any of your readers, whether those who practise physic on many parts of the Continent hold that high rank in general society which those who follow this high calling have a right to claim? And I will ask what more probable *rationale* can be given than the universality which I here oppose? I may appear old-fashioned, or be falsely designated aristocratic and illiberal in my ideas; but, sir, if my interpretation of the prevailing wish be correct, I maintain that such a republican form of medical government will neither tend to the advantage of society nor to the respectability of the profession. Distinctions reign in every other profession; nor in medicine can they easily or advantageously be discarded. How could our armies or our fleets be commanded if the admiral is to keep watch whilst the midshipman slumbers in his cabin, or the general to drill the men whilst the ensign presides at the court-martial? How are causes to be conducted, or trials carried on, if the attorney is to sit on the woolsack, whilst the Chancellor is drawing up a brief; or the Lord Chief Justice to make out the depositions, examine the witnesses, and then pass sentence on the prisoners? No, sir: such system may be suited to the hurricane taste of the times—may be popular in theory, but can never be efficient in practice.

> "Order is heaven's first law—and this confess'd,
> Some are, and must be, greater than the rest."

The field is open—the surgeon is at liberty to lay aside the knife, and the

apothecary to quit the counter, and proceed to the title of M.D.; and if they dislike the department in which they began, and think proper to advance, I would have them do so. But I would have the surgeon lay down his knife (not hold it in his mouth), ere he take up the pen; and the apothecary break all his bottles on the day he takes his degree, and convert his shop into a consultation-room, before he signs his initials to a single prescription.

That the code of medical law requires revision and correction, I am free to admit, and especially in that department which is held by general practitioners. I would have them remunerated for their skill and valuable services, *not* by the number of saline draughts they can succeed in pouring down their unfortunate patients' throats, but by a regular and legal charge to be subsequently agreed on. I would fain elevate them to a far higher station than they hold at present.

Surgeons are no longer barbers, and apothecaries have long ceased to be the pitiful objects which Shakspeare represents them in his Romeo and Juliet; they are not starvelings, whose "poverty consents" to commit crime for "forty ducats!" but men of much scientific attainment, of high principle, of professional acumen, and gentleman-like address, and who should fill their coffers not by the profit on the drugs they sell, like the ordinary tradesman, but by a proper estimate of their professional assistance, to which it is high time the public should be awakened.

"The law allows it, and the court awards it."

Thus, sir, I would have a distinct line drawn between the tradesman, who retails the medicines he does not prescribe, and the gentleman who supplies the medicines which his judgment directs. In contracting to attend the poor of a parish, the apothecary has no interest in sending in more medicine than the cases actually require; and if the patients die, he has the satisfaction of feeling that the death is not attributable to the "*nimia medici diligentia!*" He receives the remuneration which he has expressed himself satisfied to receive, without the trouble and humiliation of making out the items; and by so doing, the patient is spared the sudorific shock of perusing several pages of the extract from a huge ledger, or being startled by "the ghosts of pleasures past!" Thus, although they may be considered by some as subalterns in the profession, they are at liberty to purchase their promotion, and they mess with their brother officers: they are branches of the same tree, and not unfrequently bear much more of the "*aurea poma*" than the higher ones. Nevertheless, I would preserve distinction, and have each member of each department in physic so devote himself to its respective duties, as to render himself perfectly master of them. An eminent lecturer of the present day, a warm advocate for the *universal*, the "one faculty" system, advances in its support the absurdity which would strike any one who, on taking his watch to a watchmaker to be repaired, should be told that he must take it to another, because the wheel that was out of order did not belong to his province of the trade; but I would prove the fallacy of his argument, by reminding him that, as a watch can only be repaired by manual operation, its disorders are all included in chirurgery!—and, in opposition to his metaphor, I would quote one which surely carries some point in it, viz. that, in order to attain perfection in the business, there are no less than twenty-five workmen employed on every pin that is sent into the market!

The said eminent lecturer is undoubtedly a man of great attainments, and gigantic grasp of mind; he may probably be master of the three departments, viz. physic, surgery, and pharmacy; but where one Ducrow can ride three horses at a time, how many thousands are there who fail in their attempt to manage even one!

I will not, however, occupy more of your valuable pages. I have expressed my opinion on the grand subject of Medical Reform, and shall be most happy to see the tree of medical knowledge ably pruned, which will surely be more conducive to its utility, fruitfulness, and beauty, than lopping off all its branches, and converting it into a pollard.—I am, sir,

Your most obedient servant,

J. C. Badeley, M.D.

Chelmsford,
April 23, 1824.

MEDICAL GAZETTE.

Saturday, May 3, 1834.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

CONDUCT OF THE GOWER-STREET SPECULATORS.

THE first two days' proceedings before the Privy Council, in the matter of the petition of the Gower-Street school, have been given in the public prints, so that there remains little or nothing in the shape of news for us to tell. The further hearing of the arguments was adjourned from Saturday to this day, when Mr. Bickersteth is to be heard for Cambridge; after which the counsel of the other counter-petitioners, one for each party, will be heard, in the order mentioned in our last.

One advantage which has already resulted from this judicial inquiry is, that it has obliged the Gower-Street people, proprietors and "Senate," one and all, to throw off the mask. They are now at length forced to lay aside that trickery and hypocrisy with which they have hitherto covered much of their real design. At the commencement of the present session, it created some surprise—perhaps some pleasure—to hear the lecturer who was deputed to give the opening address, disclaiming, as it were, on the part of himself and brethren, any purpose of seeking for exclusive privileges, but professing a willingness to enter into a fair competition with the other schools of the metropolis. All this seemed wondrous fair, and served the purpose of disarming of their suspicions, not a few of the hearers. We have further reason to know that similar intimations of singleness of purpose were sedulously propagated by the friends of the school; and when we, who were not so easily to be deceived, publicly stated the fact of the real intention of the Senate to procure, if possible, a monopoly, to set them beyond the need of competition with the other metropolitan schools, their parliamentary patron and solicitor (Mr. Tooke) stepped forward as their champion, and directly denied the truth of our assertion; but we let it pass, knowing that the truth would prevail ere long. Nor was this all. At the very time that the shareholders, through the advocacy of Mr. R. Taylor, their printer, were managing to get up a petition from the city in favour of their speculation—no longer disguising the "scheme" of securing the privilege of dealing in degrees—the teachers, calling themselves the Faculty, were busy, we understand, in still disavowing any wish to possess such a privilege: nay, some of them have publicly stated that it was not expedient that the school should possess any such exclusive right.—But a trick of still greater duplicity seems to have been stooped to since. One of the medical societies of the metropolis having announced its intention of coming to a vote regarding the impropriety of the school calling itself the London University possessing the monopoly to which it laid claim—of conferring degrees in London—a member, who had evidently been prepared for the occasion, came forward before the debate began, and endeavoured to stifle further proceedings, by informing the meeting that he had had it from the best authority—some of the "professors" themselves—that they were entirely opposed to the project of procuring power to confer degrees, and that, in fact, that scheme was all at an end; so that a discussion on the subject would be quite idle and superfluous. The bait, however, did not take; for whatever the lecturers, *quasi* professors, may choose to say, the Senate, it was well-known, persevered in their demand; and the attempt to gag the Society was completely frustrated.

After a series of petty manœuvrings of this kind, the mask, as we have said, has now been fairly removed; it could be no longer worn. The Privy Council resolved to hear the arguments which were to be urged for and against the petition publicly; so that there was no more room for underhand doings; and the first piece of open dealing has been the publication of an "Address from the *Senate* to the *Council*, in support of the application for a charter."

We confess we are not quite masters of the exact import of the terms "Senate" and "Council," as applied to the managers of the school in Gower-Street; but we presume the former includes the body of lecturers of that establishment—the real executive of the concern, as the public some time ago were given to understand; if it does not, then we can only say, that any thing more egregiously uncandid or jesuitical than the conduct of the said lecturers, we cannot well conceive, in affecting to decline, on their own part, the attainment of exclusive privileges, while they enter no public protest—but allow the money-changing proprietary, under the denomination of a "Senate," to make every effort to procure them. If the Professors *be* opposed to the petition of the Council, let them come forward publicly, openly, and boldly, as their compeers at King's College have done, and then the question will be at once set at rest. The Privy Council, we are satisfied, will not grant the prayer of the "Senate," as it is; but they could not for a moment have entertained it, had the professors memorialized in any shape against it.

The "Address," is chiefly remarkable as coming forth at the present moment, when it serves both for instruction to counsel, and as a sort of declaration to be laid before the public. This, indeed, is implied in its opening paragraph, and accordingly we find that the substance of the manifesto has been converted into the body of the arguments used by Dr. Lushington.

Among the most prominent of the grounds on which the managers appeal to the public, in behalf of a charter of monopoly, we observe that they put forward the *number* of their "Professors;" though they take good care not to mention how many of the said professors have *classes*, or how many of them have utterly failed in getting a single pupil. This, however, accounts for the unwearied efforts made by the party, not only on every resignation of an unfortunate "professor," but whenever a new name for a *chair* could be invented, to cajole the public by advertisements, under the imposing head of "University of London,"—inviting candidates for the empty honour to come and offer themselves. One of the most amusing things of this sort has been the recent announcement of vacancies for a "Professor" and a "Lecturer" in the "Arts of Design." It must be confessed, that here the Gower-Street managers have the good people of Cambridge and Oxford at a disadvantage: there are certainly no such chairs in either of the ancient seats of learning; and no doubt the Gower-Street ones will be ably filled,—for if other candidates should fail to offer, the council or senate have only to elect any two members of their own body, who have most of them already displayed much talent as adepts in the "arts of design."

A considerable portion of the "Address" is occupied with a reply to the supposed objection that religion is not taught in Gower-Street. The truth is, that it is not so much an objection which is offered by the public on this ground—the public does not *want* them to teach any thing; so that great part of their attempted reply is uncalled for;—but there is no small wonder felt by many reasonable persons in society,

that an establishment, setting itself up for a University, should have the presumption to exclude religion, as a forbidden or hateful thing, from its class rooms. For a complete exposure, however, of the proceedings relative to this point, we must refer our readers to the remarks of Sir Charles Wetherell, whose speech, delivered on Saturday, was perhaps one of the most learned, emphatic, keenly-cutting, and at the same time entertaining, specimens of eloquence ever delivered before a Privy Council.

With regard to medical matters, and the power sought of conferring medical degrees, it may be observed that the "Senate" are not very prolix on this subject in their present brochure. They appear to have shifted their ground a good deal of late, in their mode of canvassing for this privilege. They are willing to conciliate to the utmost. They dwell chiefly on the advantages of having an admitting body in the metropolis—a medical university; but they hold, consistently enough, however ludicrously, that it ought to be established in Gower-Street. They even offer to allow "assessors"—medical men from without the walls—to be appointed to co-operate with the teachers within; thus making up a *quorum* for carrying the "scheme" into effect.

Further, they propose to give their degrees *cheap*—inasmuch as "abuses have arisen in *other Universities* from the payment of fees for diplomas." Does it not occur to the senate that the cheapness of the article for sale often attracts only a certain class of customers; and that it may be possible the public shall understand the lure of fixing low prices on the wares in the Gower-Street University?

But the most ludicrous passage in the pamphlet, perhaps, is that in which the worthy senate gravely inform the public that "if any other establishment shall seek the same privilege of conferring degrees," the "University of London" *will not oppose it!* We are sure the public will duly appreciate this liberal intimation: the generous, patronizing spirit which breathes through the announcement, surely merits that the privilege should be possessed by those who will use it so nobly.

Here we close our remarks on a production which some of our readers may probably think we have treated much too largely. We are rather disposed to think so ourselves; but we should recollect that the "Senate" had not the opportunity of knowing, when drawing up their address, that it had been so ably and completely answered by anticipation in the memorial and petition of the great body of the medical lecturers of London, given in our last. Perhaps the perusal of these documents may induce our senators once more to resume the pen, unless indeed the Privy Council shall have previously rendered that task superfluous.

PARLIAMENTARY COMMITTEE ON THE STATE OF THE PROFESSION.

Among the witnesses examined this week have been Dr. Yelloly, Dr. Frampton, Mr. Guthrie, President of the College of Surgeons, and Mr. Clift. Some very curious facts were elicited in the evidence given by the latter gentleman.

ERRATUM OF A CONTEMPORARY.

The Lancet of last week quotes our account of Fisher's case in such a manner as to shew that the Editor of that publication feels he had been misled by his informant. There is, however, rather an important mistake in his edition of what appeared in the Gazette. We stated, that "as there was reason to suppose that the calculus was small, it was decided that it should (if found practicable, without using force), be withdrawn through the urethra," &c. But the Lancet has altered this into—"As there was *no* reason to suppose that the calculus was small," &c.—an interpolation which alters the meaning not a little, and which, however suspicious, we must in charity presume to have been undesigned.

CLINICAL LECTURE
ON
AMPUTATIONS.
Delivered at the Middlesex Hospital,
By Sir Charles Bell,
March 15, 1834.

I was about to give you some remarks upon injuries of the great joints; but I remember that I promised, in the beginning of the season, to pay particular attention to all operations that might eventually be performed; and you have seen such a variety of amputations of late, and two flap operations yesterday, and one again early this morning, that I fancy I am bound, by my promise, to speak of *amputations.*

The first reflection which I think must have arisen in your own minds, is the very remarkable difference in the success of the operation of amputation, according to the previous condition of the sufferer,—if the patient has been reduced by long illness, you see him rally in a surprising manner; but the event is very different when a man in high health is overtaken with a severe accident that requires amputation. You have, I am afraid, seen that the operations performed on young persons in florid health have not been successful in that measure that you have seen in poor, languid, exhausted, hectic creatures. Is it, then, that you operate in a condition of the patient's mind little prepared for so severe a shock? Are we to seek the cause of this in the constitution of the bodily frame, or in the condition of the mind?

Now, there may be a great deal, in common cases, in the preparation of the mind; but I have seen a man come into this hospital, petitioning the surgeon to amputate, on account of distortion, and die. I remember very well a little conceited tailor coming in here to have his leg amputated, because it was crooked: his mind, we must presume, then, was prepared. I was against the operation—that I must say for myself; but the operation was performed, and the patient died: and such occurrences make me suspect that we are to seek for the cause more in the state of the constitution than in the condition of the mind; though I am ready to allow that there must be a great deal favourable in the calm and resigned condition of the mind. For example, when you take the practice of our navy surgeons, and when they are speaking of the time of amputations,—these are circumstances to be taken into consideration. Brave men, in going into action, are prepared for wounds. In matter of fact, when wounded, they descend to be operated upon, while yet the animation of the fight prevails; and in the work of Mr. Copland Hutchison you will find that these men have the resolution and presence of mind to tie their handkerchief or garter around the limb, and with a shattered leg descend from the rigging, and hop down to the cockpit! These are men who, under operation, will join in the shout, when they hear their comrades on deck cheering after a successful broadside: and we must not lose sight of the fact that many of them desire to lose a limb, because it is a provision for life. What a different prospect is before the poor lad on whom it was necessary to operate this morning! It is lamentable to think, that, during the operation, he was whispering to his nurse, "Well, I must have a wooden leg, and go a-begging." That is a very different condition of mind, and a very different condition of body, too, you say; and, therefore, I fear that we may be apt to be misled by the authority of these naval reporters, inasmuch as they are operating upon young men in health, but in a condition of mind not merely prepared, but some of them positively wishing for the operation.

As to the time of operating—what is the meaning of early amputation? If you take again our authorities, our navy and army surgeons, you find a very great difference,—some of them speaking of half an hour as an early period, and some of them speaking of six or seven hours as the early period. Now it appears to me that the early period is when the operation is performed very early; that is, when the man is still animated, as it were, with the passion and excitement of the actual engagement,—when the knife follows the injury so immediately, that there is no second shock.

If this be the first period for amputation, it is pretty certain that, in domestic and hospital practice, we can never profit by it. You are not present: you do not stand with your consultant and assistant, and all appliances, ready to operate; and if you were, the alarm of the sufferers, and the outcry of the friends, preclude this early operation.

There is another point that you must consider, in looking critically into authors on this question. You find some, especially the French authors, eloquent on the subject. Some of our army surgeons have taken up the same view; and in my brother's works you find a very eloquent and animated description of it. I allude to the weakness that arises from commotion, the tremor, the nervous lowness, the sickness, and the sensation of death, which comes upon a man severely wounded. This undoubtedly does come upon a man when wounded in the belly; but on

many occasions, when I have conversed with the wounded, I have not found any account of this tremor or nervous weakness, from commotion; and in the pamphlet in my hand, which is the last record we have of the navy practice, by Mr. Hutchison, (and which, consisting but only of a few pages, is yet invaluable,) I find the condition alluded to in one instance only. That there is such a condition, be assured, although it is very possible that their number may be multiplied by authors, giving an air of originality to what they have learned at second hand.

In order to make the different testimonies correspond, I fancy you will have to take into consideration the loss of blood in many instances. There is a case which you have all seen repeatedly,—which we unfortunately see very often in the course of a season; and I have from time to time, in former days, taken it as an illustration of what I meant;—I refer to the condition of a man who has attempted suicide, by cutting his throat. When the patient is brought to the hospital, there is depression of mind and loss of blood; and the depression of mind and the loss of blood reduce the man to the very condition, so far as I can understand it, that these authors have described to be the effect of severe gun-shot wounds; so that I cannot help suspecting that when the picture is drawn of the nervous influence upon a man who is shot, they have not sufficiently distinguished the cases where there is loss of blood.

We say currently, and with perfect correctness, that if an artery be opened with a lancet, it bleeds the person to death; if it be cut with a bit of glass, it bleeds the person to death; but if the limb be torn off, the artery does not bleed; and if the artery be cut by a ball, it does not bleed. Now, that is perfectly correct; there is no hesitation about the fact; yet I have a notion that when a round shot comes through the sides of a ship, for example, and tears off the limb near the groin,—that this is not such an injury to the artery as to cause the immediate stopping of blood; that, in fact, before the man is brought down to the cockpit under the observation of the surgeon, he has lost an enormous quantity of blood; and that the great artery does not bleed in some measure, because of the loss of that blood; then the condition of the patient is accounted for: there is the fear of death,—there is the shock to the whole nervous system,—and there is loss of blood; and these three combined are enough to explain the lowness, tremor, and condition almost of approaching death.

With regard to the effect, then, of early or delayed amputation, I have had this pamphlet put into my hand by my excellent friend. I have not had time to read it again; but I have just lighted upon this paragraph, and it brings me to reflect on things long since gone by. It is a list of amputations performed on board His Majesty's ship Leander. The remarks are these:—

"Amputation of both thighs, very high up: died in a few hours.

"Amputation of both thighs, very high up: ditto ditto.

"Amputation of hip-joint: died shortly after.

"Amputation of left thigh: convalescent.

"Ditto - ditto - ditto.

"Ditto - ditto - ditto.

"Amputation of the right thigh: convalescent.

"Ditto - died of phthisis, and the stump cicatrised.

"Amputation of the left thigh: fever twenty days after amputation," &c. &c.

Now this brings us to reflect on a most important circumstance,—a circumstance that is necessary to explain my conduct this very morning, with reference to the amputation in that young man. In amputation there is a shock to the system, to the powers of life, by the lopping off of a portion of the body, and there seems to be a shock proportionate to the mass that is severed; and there must also be a shock proportionate to the extent of the wound. You may naturally enough say, is there a shock to the system from amputation, and none from the limb torn off by cannon-shot? The question would be very natural; but my meaning in using nearly the same words is different. The shock to the powers of life which results from too severe an operation, is very different from that attributed to the effect of wounds. No doubt if a thoughtless surgeon should keep his patient too long upon the table, poking for a stone in the bladder, or working on a disease of the rectum, the patient becomes sick, and vomits, falls low, and never rallies. But the impression I now speak of is of a different kind, and occurs at a different period: he goes to bed in comfort, and promises to do well, but dies on the fifth or sixth day,—sinking you know not how. The extent of the wound must be very great in a hip-joint operation. The extent of the wound is very great when both thighs are taken off, and the shock is greater when the thigh is removed nearer the groin than just above the knee; and although we may lose sight of this great principle when we see one case at a time, yet it is impossible to look upon the wards of an hospital after a naval engagement, in which a great many men have been wounded, and have suffered amputation,

as I have seen in Haslar hospital, or after an engagement on land, as I had abundance of opportunity of witnessing in the hospitals of Brussels after Waterloo, without perceiving in these cases this great question decided,—that the deaths are in proportion to the mass cut off, and to the surface of the wound exposed. This corresponds exactly with our practice here at home—I may say with your own knowledge.

The sole object I have in these observations is just to carry your minds back, to bring you to reflect upon these cases, and make them the elements of your future reasoning. Now you have seen a man with his limb shattered, in whom amputation of the thigh has been performed; and you have seen that after a fair promise of success he dies. You have repeatedly witnessed this occurrence. Well, then, here comes in this young man, on whom I operated this morning, a healthy, robust, country lad. A heavy waggon has gone over his foot, crushed the tarsus, crushed the ankle-joint, and fractured all the bones of the foot. At first I thought of making a flap from the sole of his foot, and taking away the shattered bones, but upon putting the finger into the wound, it is found that the os calcis is fractured, and that the ankle-joint is torn. You see by a man now lying in Clayton's ward what is the effect of attempting to save a limb where the ankle-joint is at all open: that man must suffer amputation after all. The attempt of saving the heel, to rest upon, in this lad's case, would have been followed by suppuration in the ankle-joint and a tedious caries of the bones. It was necessary, then, to take off the foot; and the question came to be, shall we perform the operation just below the knee, or low down? You see that the matter stands thus: when you have got a young man in a certain condition of life, who is to lose his foot, you perform the amputation with a flap above the ankle-joint; and he may have an artificial foot so adjusted that he retains the motion of the knee-joint. A footman, a great favourite of his master, who was very desirous to keep him in his service, had an accident of this description. In that case I performed the flap operation above the ankle-joint; he got a case, into which he put his limb; he had the motion of the joint, and could walk decently into the parlour, and stand at the sideboard. He thus retained his place, which he could not have done had he rested on the knee, and had a stump sticking out behind.

But although this lad be healthy and be young, it was not for that reason that I amputated low down. There is a great brawny calf of a leg; and if I had amputated through the mass of muscle, I conceive that the danger to life would have been much greater than in amputating a little above the ankle. You remember I stated to you that in some of these cases I preferred completing the amputation which the accident had in part performed, extending the incision, and dividing the bones, and thus avoiding the second shock altogether; for I am sure that in some cases it is a good practice. On reflection, I feel satisfied that of the many amputations I performed at Brussels on shattered limbs, more would have survived if I had opened the great abscesses in the leg, cut across the remaining soft parts, and cut off the spiculæ of bone. In those cases it appears to me that the profession has been too anxious for a clean wound. Here I could not do that, but, acting on the same principle, I made the operation as low down as I could, taking care to save muscle to cover the extremity of the bone. That is the reason why I performed this amputation as you saw; and I hope it may turn out better than those operations that we have had performed, on account of severe accidents, higher up, where the great mass of the limb was cut through. At present there is no indication by symptoms that the constitution has felt the injury.

With regard, then, to amputation generally, there is one rule which answers for them all, and which is so simple, so obvious, that you would wonder that it had ever been omitted in performing an operation. It is simply to save integument to cover the muscle, and to save muscle to cover the bone, and to keep the periosteum hanging over the bone, and never to scrape it or expose it. If these three things are done, it requires ingenuity to make a bad amputation. If the integument freely cover the muscle, and the bone be sunk in the muscle, the amputation is well performed. Performing the operation on the thigh is the simplest operation of all to the surgeon, though the most severe to the patient; and it ought to be so done, that when you have cut across the bone, that is the last view you should have of it: you should never see it again; but if you see that the bone is sticking out, that is bad; you should, and you must so manage it, that when you have made a section of the bone, you say good by to it; you never see its face again; it is sunk among the muscles.

Now how is this to be done? By the simplest mode possible. First, as to the external incision. If the incision go round the thigh, it may be a good amputation; but it will not make a very neat stump. You will find that when you bring the integuments together, they are puckered; they are irregular: that is a matter of

no great consequence; but yet it is easy to avoid it. If you take the long amputating-knife, and, instead of going round at once, incline it with a little semi-circular sweep (first on the one side, and then, without raising the knife, on the other), then you have two flaps, and you have made an incision that will come nicely together over the face of the stump. Do not separate the integument from the fascia, nor the muscle; that is quite wrong; and it is never done unless there has been a great deal of previous inflammation and hardening of the parts, in which case you are obliged to fold up the skin. You are never to separate the cellular texture which is under the skin from the muscles by dissection: all that your assistant does is to pull up the skin. You have now the integument retracted, about an inch above the line of your first incision, and here you are to put down your knife again. You are next to make the circular incision through the muscle down to the bone, and round it. An attempt has been made—and I am sorry to say it has the support of no mean name—to make the incision at three times: first an incision through the integuments, then through the superficial muscles, and then through the deep muscles,—the superficial being cut further down, and the deep-seated further up. Now what has the surgeon done? He has contrived to make an irregular surface, in which both arteries and nerves may be partially divided—that is, cut above where they are divided. Go through the muscles directly down to the bone. All that is required is, to allow the patient to draw up his leg, which he will do naturally, if the assistant dresser who holds the leg will let him; and as he draws up his leg, the integuments and the muscles are drawn from the bone, and the bone projects to the utmost extent. Assist this a little with the split cloth, cutting the fibres attached to the bone as it protrudes. You now make your saw move horizontally; and one effect of this is, you do not break it, which you are very apt to do by the unsteadiness of the assistant who is supporting it in the horizontal posture. The moment you have cut across the bone, bring down the limb to the horizontal posture, or nearly so, and then you find the muscle lapping over the bone, and the integuments a little projecting from the muscle. That I call a good amputation; every essential circumstance is secured.

Now you have seen another mode of operating adroitly performed,—and by and by I trust we shall be able to say, successfully performed,—which is the flap operation; you have seen this done by both my colleagues in one day. You grasp the mass of muscles and integuments upon one side of the thigh bone, pass the long double-edged knife close to the bone; and having made this ugly stab quite through the thigh, the knife is carried downwards and outwards (that is, in a direction from the centre); and having thus made a flap of muscle and integument, the soft parts on the other side are grasped, and the knife passed again close to the bone, and a second flap made. The advantage, not a small one, is, that the operation may be done with great rapidity. The essential points are certainly gained here; first, a rapid operation, and, secondly, the muscles and integuments can be freely laid back, and the bone exposed; and you are secure of muscles and integuments enough to cover the bone. I do not see any further advantage in it, however, unless it be this (and I am willing to allow what I do not comprehend), it really does appear that there is less pain. We are jealous here, as it were, of our own observation; and we must wait and notice whether this be a fact, because it is a great matter, indeed, to the patient: the less pain, the less shock.

[Sir Charles Bell now went into a description of the mode of amputating with the flap low in the leg; afterwards he proceeded—]

I beg you to recollect that, in order to insure the third point of an operation—the burying the bone in the muscle—you have a difficulty in all amputations of the leg. You sometimes see the surgeon put through the split cloth between the tibia and fibula, and then twisting it over, attempt to draw back the muscles and integuments; but he does not succeed, and a little consideration will shew you why: it is because of the inter-osseous ligament; and therefore you put in your knife close upon the edge of the tibia and fibula, and you cut the ligament. As you cut the ligament, you find that the cloth goes up—that the bone projects more and more: without you understand this, you will have the bones projecting in the stump. Further, you so manage it as to cut through the tibia and fibula together. If the tibia be cut through in a high operation on the leg, it belongs to the regular operation, that the surgeon again puts on his saw obliquely, so as to take off the projecting angle of that bone, which else is apt to project, and irritate the thin integument.

[The subject was resumed on the 22d March.]

On leaving you the last day, you will, I hope, consider that it was natural in me to ask myself, "Have I omitted any thing in addressing these gentlemen?" Now I found I had, and a point by much the most important in the operation of amputation. I said that an amputation of the thigh was well performed when the bone was quite

buried, and when, on its healing, it looked like the caricature of a fat child's face. For the reverse picture, look to the case of the lad with diseased hip, under my care, who lost his leg some time ago, and who exhibits now the most perfect specimen of the conical stump. There the thigh tapers to a point where the bone projects, covered only with a tender film of vascular integument.

But it may happen, when all is done well, and the stump is of the most approved form, that the patient has pains in the extremity, and that the stump may be as susceptible to the variations of the atmosphere as a barometer, and very easily affected by the state of the general health. He may have shooting pains in the stump, darting like a stroke of electricity, and with such violence as to make him faint. He has got rid of one formidable disease, to become subject to a different and a worse malady.

What is there then in the mode of amputation that has laid the foundation of all that misery? That is the point of which I omitted to speak; but you now anticipate what I have to say. See that the nerves are cut short; for if the nerves are permitted to hang out, and to attach themselves to the cicatrix, your patient is in danger of this formidable occurrence. This is the reason why, when you cut the integuments, you draw them up, and lay the edge of the knife again on the fascia, higher by an inch at least than where you cut the integuments. This we are able to do, by retracting the integuments. Between the skin and the fascia there is a loose cellular texture, and under that the cutaneous nerves run close upon the fascia; so that if you retract the skin properly, and place your knife at the edge of the retracted skin, you divide the cutaneous nerves at least an inch and a half, or more, shorter than the integuments, and the latter completely covers and envelopes their extremities. This is a very essential part of the operation to be attended to, and is too often neglected.

Then, again, you sometimes hear the poor patient say, "Is that foot not off yet?" What is the meaning of that question, when the bones are sawn through, and the leg is removed? It is, that when you are taking up the arteries, you touch a divided end of the nerve, and it appears to the patient as though the sensation was in the foot. Take care that this be the last time he shall feel the foot; for if you include the nerve in the ligature of the artery, you lay the foundation for the condition I have described; your ligature hangs upon the end of the nerve, and the nerve expands beyond it, like a button in the loop. By and by you desire to get it away; and that is a difficult and very painful operation—much more difficult than that of amputation, and nearly as painful. As you would avoid that awkwardness, then, be careful, in taking up the vessels, that you see the end of the nerve free from the loop you are about to draw. If you have any hesitation, draw out the nerve; it is the least elastic part of all which you divide in amputation; and if it has suffered inflammation, it is particularly apt to project; which, if it do, fix the tenaculum in the neurilema, draw it out, and cut it across with a very sharp scalpel; it is a pang which will prevent much and long suffering.

If you have made a note in your memories of what I said on amputations, when I last addressed you, add these few observations to it.

DR. RIGBY'S

MIDWIFERY HOSPITAL REPORTS.

GENERAL LYING-IN HOSPITAL.

[Continued from p. 112.]

DYSTOCEA PELVICA.

Of *dystocea pelvica* several cases have occurred: four out-patients required the aid of the forceps; in two in-patients the perforator was employed, and artificial premature labour was induced in two others. Of the forceps cases, I select two, which are interesting—the one because a large living child was born where the pelvis was evidently considerably deformed, the other where the forceps were required to be introduced *above* the brim of the pelvis.

"12th Jan., 1833.—Margaret Dudman, æt. 28. First child; delicately formed; very juvenile in her appearance; pains became strong at one o'clock yesterday morning, when the liq. amnii came away; since which, till this evening, the pains have been lingering; and, as the patient was becoming exhausted, I was sent for. Upon examination, I found the os uteri all but fully dilated, a small portion of the anterior lip still remaining: the head had entered a considerable way through the superior aperture, but was evidently much elongated and compressed for want of room in the antero-posterior diameter; felt a fontanelle behind the left foramen ovale, but the swelling of the head rendered it indistinct. As there were no pains, and the head so far advanced, I applied the forceps, passing them up where there was most room; viz. in the transverse diameter. I had some difficulty in doing it; for, whilst introducing the second, the first blade slipped into the hollow of the sacrum, and I was obliged to begin again. The first extracting effort which I made

convinced me that the head was *very* tightly impacted, for it was nearly immoveable, and I felt as if I was pulling against the whole mass of the patient's body: it advanced however somewhat; and finding, both from examination, and from the distance between the handles of the forceps, that it was very large, my pupil, Mr. Wray, who assisted, tied a piece of tape tightly round the knobs of the handles, so as to keep up a constant pressure upon the head, and thus induce it to elongate still further. This was again tightened after a time, and the head advanced gradually towards the perineum: when the perineum became distended, I waited some time between each extractive effort, in order to give it every opportunity of dilating, and an immense head was extracted, the face turning to the mother's right thigh. One blade had passed obliquely over the right eye to the root of the nose; the other, under the left ear to the under-jaw; the shoulders followed with considerable difficulty, and a laceration of the perineum was produced. The child appeared dead; but, after a little while, gave a slight gasp. I immediately poured some spirit on its breast, and applied a hot flannel, moistened with spirit, over the chest and abdomen. After a time it again gasped. The cord not pulsating, I divided it, and squeezed out some drops of blood from it, and then put on the ligature. Another gasp came: I inflated the lungs through an elastic catheter, and it made two attempts in succession to inspire. It was now put into a warm bath, and the hot spirit flannels applied to its chest; a little ammonia was added, and occasionally applied to the nose, and a drop of spirit to the lips. The efforts to breathe recurred more frequently, and at length, in the course of an hour, the breathing became distinct and regular, although the child was still insensible when I left. Whilst this was going forwards, hæmorrhage suddenly appeared from the patient. A dose of ergot had been given when the head was born, and was now repeated. The placenta was situated on the anterior wall of the uterus; the cord broke, and I was obliged to introduce my hand to extract it. I threw up some cold water per vaginam, and with cold, externally applied, the uterus contracted hard; the sacrum was reached very easily with the finger; the antero-posterior diameter of the pelvis was, at the utmost, three inches and a half. Both mother and child did well."

It is exceedingly difficult to know, in cases of this sort, exactly the limits where we can no longer use the forceps, and where the perforation becomes necessary; nor do I know of any very distinct rule which will serve as our guide. So many circumstances must be taken into consideration, both with respect to the child as well as the mother, that it is impossible to ascertain this point, without having accurately examined the whole history of the case, and carefully weighed the importance of every symptom which may present itself. The readiness with which some children's heads yield, in comparison to others, is well known; but to form an opinion as to this point, before applying our instruments, is far from being so easy as may be imagined, and demands that tact and experience which can only be obtained by long and constant observation.

In the above case, as in many others, where the head has been closely impacted, I have repeatedly found the benefit of keeping a constant pressure upon it, by a strong tape tied round the handles of the forceps, which I have tightened in proportion as the head appeared to yield. By following this plan, and by being careful not to hurry the labour, the head has at last begun to move, in cases where at first it had resisted all justifiable attempts to bring it down; and where, to all appearance, I had no resource but in perforating. In the case I have just described, a laceration of the perineum was produced, although not to any serious extent; and, from the care which I took in supporting it, when the head was passing, I am inclined to suspect that the greater portion of it was produced by the shoulders. I have repeatedly seen cases where the perineum has been supposed safe, and where, I believe, that, with the exception of the frænulum perinei, no laceration had taken place, that, when the shoulders were born, a considerable rupture was found. The shoulders are apt to pass out rather unawares, perhaps when the practitioner is engaged in ascertaining whether the cord be round the child's neck: at this moment the shoulders are very liable to convert a slight tear into a considerable laceration. The difficulty with which the respiration of the child was established, illustrates some very interesting points of practice, which are of much importance. The asphyxia neonatorum demands very different treatment, according to the state of the circulation. In the present case, the symptoms of plethora were very distinct, and justified the extraction of a little blood from the cord. I know of no method so successful in rousing the torpid action of the heart, as applying a hot flannel, moistened with spirit, to the child's breast: a little liquor ammoniæ sprinkled upon it, assists the rubefacient effect, and accelerates the passage of the blood in the capillary circulation.

The ergot of rye I have given in compa-

ratively few cases before labour was completed, having very much confined my use of it to where there was inertia uteri after the birth of the child, nor has it failed my expectations. For some time I used the tincture, on account of the convenience of the form for instant exhibition; but I cannot say that I ever once saw any distinct effects produced by it, and have long since relinquished it for the use of the powder, which I give in a little cold water. Many discrepant opinions have been entertained respecting the ergot of rye; but where it has been procured good, well protected from the air and light, and kept dry, I have seldom known it fail. As a means of preventing hæmorrhage after labour, I consider it very valuable; and shall, in several cases of this report, have occasion to bear repeated and favourable testimony of its power in exciting uterine contraction. In the present case the placenta was detached with some difficulty, owing to firm adhesion and unusually weak cord; and having been left for some minutes whilst engaged about the child, the uterus relaxed, and hæmorrhage appeared: I did not, therefore, wait for the action of the second dose, but immediately threw up some cold water into the uterus; and thus having caused full contraction, effectually prevented any further loss.

"Aug. 19, 1833.—Mrs. Worsley, æt. 36; out-patient; a stout, elderly-looking, red-faced woman, mother of three or four children. Pains came on yesterday evening; membranes ruptured at midnight, the head partially entered the brim of the pelvis, but not having advanced further, in spite of strong pains, by two o'clock this afternoon, I was sent for. The midwife, by her own account, had introduced her hand repeatedly to turn the head, and other improper manipulations. The patient was very hot; but as the pulse was slow and soft, the passages moist, and the os uteri fully dilated, I did not bleed her. I applied Naegelè's forceps with tolerable ease, although there was some little difficulty in passing the brim of the pelvis. At first, my attempts to extract the head appeared to produce no effect, but, by persevering gently, and constantly moving the handles in a circular direction, it at length gradually began to move, and then came down quickly. An immense child was born. There was merely a trace of pulsation, which quickly disappeared, in spite of my attempts to rouse the heart. The blades had pressed very tightly, leaving a large ecchymosis, which filled up the fenestræ. A slight hæmorrhage followed, for the room was very hot; but she did well."

I have given this case chiefly with a view to show the practicability of applying the forceps, where the greater portion of the head is still above the superior aperture of the pelvis. It is an operation, I believe, which is rarely performed in this country: indeed, by far the majority of English midwifery forceps are not adapted to it; it has been also opposed by the high authority of Hamilton, Burns, and Merriman; and a similar opinion has been entertained by the celebrated Carus, of Dresden. "The true case," says Dr. Hamilton, "is where the bulky part of the head has passed the brim; and so far advanced, that the soft parts of the woman begin to protrude."—"In almost every case," says Mr. Burns, "where the forceps are beneficial, the head has so far entered the pelvis as to have the ear corresponding to the inner surface of the pubes, and the cranial bones touching the perineum."—"The forceps," says Dr. Merriman, "should never be employed till the ear of the child has been within reach of the finger for at least six hours, hæmorrhage and convulsions excepted." I am far from wishing to invalidate such rules as these, because they are of the utmost utility in preventing those improper attempts at giving assistance at an early period of labour, which would so often lead to injurious results: still, however, there are cases where these rules cannot be considered to apply; where there is such a want of proportion between the head and pelvis, that the expulsory powers either cannot make the head enter into the superior aperture at all, or in so slight a degree, as to give no reason to expect that it will pass into the cavity of the pelvis. The cranial integuments are felt puffy and swelled, from the obstructed circulation in the scalp, shewing that in all probability the child is alive, or was so very recently: is this a case for perforation? Knowing the extraordinary powers which the child's head possesses of elongating and shaping itself to the passages through which it has to pass, and the great degree of pressure which the fœtal brain will bear without injury, are we not justified in trying not only the effects of extracting, but also of compressing force? In labours of this sort, where the obstruction is chiefly at the brim of the pelvis, the passage of the child is rarely difficult throughout; and I do not hesitate to own, that in several cases where I have applied the forceps and could not stir the head, I have been on the point of deciding to perforate, when, having made one more trial, a sudden movement of the head has taken place, the difficulty has been passed, and the delivery completed with comparative ease.

I mention this to impress upon the minds of the junior members of the profession how seriously important it is, not

only to make an accurate examination as to the proportions between the head and pelvis, but also, as far as lies in our power, to form some idea as to the softness and yielding nature of the cranial bones. Above all, in cases of this doubtful sort, a minute and familiar acquaintance with the symptoms of the child's death is of the utmost value; for the practitioner who delays to perforate the head of a dead child, and thus prolongs the sufferings of his patient, in a labour where at any rate the forceps must be applied, is just as culpable as he who performs the operation without sufficient evidence of the necessity for so doing.

I now come to those cases of dystocea pelvica, where the deformity of the pelvis was so considerable as to require perforation.

"July 12th, 1833.—————— æt. 24, out-patient, a muscular, red-haired, florid, Irishwoman; married exactly nine months and eight days. Labour pains came on early this morning. At four in the afternoon the head had made but little progress, resting on the superior aperture, and feeling much swelled; could not ascertain the position. As the pulse was full and hard, the patient hot, and face flushed, I bled her to ℥ xxiv., producing only slight faintness. In ten minutes after, the pains, which had become very inefficient, returned with much energy; the os uteri, which previously had not been completely dilated, was now open to its full extent, and the edge, which hitherto had been thin, became soft and thick; a large portion of the head had entered the cavity of the pelvis; and as all this had taken place so rapidly after the bleeding, and the pains were active, I was in hopes that nature would complete the labour, and therefore left her with the midwife. At six P.M. I was again called; the head had made no further progress, and the pains were again failing; she was becoming restless, vomited her drink, &c. I found there was plenty of room in the inferior aperture, but towards the brim the head was fast wedged. I tried to pass up a blade of the forceps, but it would not go beyond the upper portion of the pelvic cavity; and a large portion of the head was still above the brim. I therefore perforated; the head was unusually hard; I threw up a stream of warm water, and a considerable quantity of brain followed. The head collapsed very slowly; and in spite of considerable efforts with the blunt hook and fingers, it did not pass the perineum till near nine o'clock; the nates passed through the pelvis with considerable difficulty; the placenta followed easily, and no hæmorrhage. After labour I examined the state of the pelvis, and could not only reach the promontory of the sacrum, but at least an inch of the sacrum, beneath the promontory, seemed to be convex, or at least straight, towards the pelvic cavity, instead of being concave. The patient did well.

In perforating the head, I follow a plan which was recommended to me by my excellent friend, Professor Naegelè, of Heidelberg, and which I have found of great service in facilitating the collapse of the cranial bones—viz. by introducing an elastic tube into the perforation, and throwing up a powerful stream of warm water, to dislodge the brain from its attachments; the substance of the brain is thus entirely broken down, and comes away very readily; the cranial bones collapse, and the head descends with great ease. In the case of a very deformed young woman, whom I delivered by the perforator, only two or three hours before the commencement of the year, the practice of which I am now recording, and where the projection of the sacrum more resembled a second fœtal head than any thing else I can compare it to*, the advantage of this practice was very striking; the whole brain came away almost immediately, the bones collapsed, and the head descended with much greater ease than in many cases where the deformity was by no means so considerable.

[To be continued.]

ROYAL COLLEGE OF SURGEONS.

(Communicated by order of the President.)

To the King's Most Excellent Majesty.

The Petition of the Royal College of Surgeons in London,

Sheweth,

That by charters, granted by your Majesty's Royal Predecessors, this College was incorporated for the advancement of surgery, and for the examination of surgeons, with the power of granting them a diploma attesting their ability.

That your petitioners have expended large sums of money in the reception and display, in the preservation and augmentation of the Hunterian Museum, and in rendering it accessible and useful to the public, so as to advance to the utmost of their power those great scientific and national objects, which were contemplated by your Majesty's Government, and by Parliament, in intrusting the museum to their

* The antero-posterior diameter, by measurement after delivery, was barely two inches and three-quarters.

care; also, in making and constantly increasing a collection of books, not only in all branches of medical science, but in various auxiliary departments of knowledge, and in opening it freely to the scientific public, as well as to their own members.

That the museum and library are attended with a great annual expenditure, entirely defrayed from the funds of the College; and that they could not be maintained in their present state of public usefulness and efficiency, if those funds should experience any material diminution.

That, in fulfilling the important duties intrusted to them by Royal Charter, your petitioners have constantly endeavoured to improve the education of surgeons, to advance the healing art, and to uphold the scientific character of the country. They have the satisfaction of believing that these efforts have been successful; they do not hesitate to affirm, that the members of this Royal College have contributed very largely to the great improvements which surgery has received in modern times; and that, whether we regard the intelligence and skill of the body generally, or the knowledge and public services of individuals, English surgeons are not inferior to those of any country.

The diploma of the College is held in such high estimation by the public, that nearly all who enter the surgical profession consider it indispensible to their success; and its possession is required, almost invariably, as a condition of eligibility to public surgical appointments of all kinds.

Your petitioners having heard that certain persons, acting on behalf of an institution called the London University, have applied to your Majesty for a charter of incorporation, with the power of conferring medical degrees, humbly submit to your Majesty, that the grant of such a power to that institution, while it would infringe the chartered rights of your petitioners, would be injurious and unjust to the other medical schools of the metropolis, which your petitioners are bound to protect to the utmost of their ability, from a long experience of their efficiency as instruments of professional education, and that it would consequently be highly disadvantageous to the public.

Your petitioners,—having heard that the liberation of a large class of your Majesty's subjects, who dissent from the Established Church, from the inconveniences under which they labour, in consequence of their exclusion from the English universities, is alleged as a reason for granting to the London University the power of conferring degrees,—beg leave to represent to your Majesty that no religious distinction is observed in conferring the diploma of this college.

Your petitioners humbly submit to your Majesty's gracious consideration, that the institution which is called the London University resembles the ancient and venerated universities of England only in name; that it is, in fact, a joint-stock association, established by the subscription of money in shares, which may be bought and sold in the share market. When first opened, it was under the general direction of a council, and the immediate superintendence of a resident head, called the warden; and it thus presented to public view the outward semblance of academic arrangement and discipline. These forms have been materially changed, and the office of warden has been altogether abolished. The institution, having spent all the money originally subscribed, has already fallen into pecuniary embarrassments; so that, according to the printed document issued by the council, the session could not have been opened in October 1832, without the assistance of a loan from some proprietors, who have, therefore, a manifest pecuniary interest in the success of that application for a charter, which they have been particularly active in promoting. It has since been found necessary to raise a further supply, by way of mortgage. The consequence is, that the original 100*l.* shares are now at a discount of 75 per cent.; so that, for the sum of 25*l.*, a person may become a proprietor of this institution, and have a voice in the appointment of professors, and in the granting of the proposed degrees.

Your petitioners humbly submit, that a fluctuating body of shareholders, amounting to above 1000, (especially when the shares may be bought for 25*l.*) is unfit, by its very constitution, to exercise the powers now solicited on behalf of the London University;—that this establishment, even if its constitution were unobjectionable, is entirely deficient in the essential requisites of security and stability, and can only be regarded as an experiment, of which the result is at present extremely doubtful.

In consequence of the state of the finances, the council of the University, before the opening of the session in 1833, withdrew from further pecuniary responsibility, by giving up the control of the schools, in a great measure, to the professors, and receiving from them a guarantee to the amount of the estimated annual expenditure. Thenceforth, therefore, the medical and surgical department of the London University can only be regarded as an association of teachers conducting the school as a speculation of their own, superior in no respect to the medical schools of London, but inferior to most of them in not possessing the means of that practical instruction at the bed-side of the sick, without which a due knowledge of

medicine and surgery cannot be acquired. On the other hand, the previously existing schools of surgery connected with the great hospitals of the metropolis possess the advantages of a well-organized system of instruction both in precept and in practice. In them the great body of English practitioners have been educated, and their teachers have attained a degree of celebrity as public men, to which the high character of English surgery may be ascribed.

Your petitioners therefore humbly submit, that the association of medical teachers in the London University has no claim to any superiority of power or privilege over the other medical and surgical schools of London; and that to give to the former distinctions or privileges which are denied to the latter, would involve the double injustice of unfair preference and unmerited exclusion.

Your petitioners beg leave to represent that large sums have been expended in founding and supporting the medical schools of the great hospitals in London, in erecting suitable buildings, and in providing museums, libraries, and the other requisites of professional education; that property to a considerable amount is invested in these establishments, in which gentlemen of abilities, knowledge, and zeal, are employed as teachers. Among these schools, which depend for their success merely on the talents and exertions of their teachers, and on their respective advantages in other points, an honourable competition exists, conducive to the promotion of knowledge and advantageous to the public.

Your petitioners beg leave to express, in the strongest terms, their serious apprehension of the public disadvantage that would result from any measures calculated to derange the present efficient system of medical and surgical instruction; especially from the conferring on any one school a monopoly of power, and thus degrading and injuring the other institutions.

Your petitioners are firmly convinced that the occupation of teaching, and the power of examining and conferring degrees, ought to be exercised, as they now are, by distinct institutions; and that the union, in one and the same institution, of these discordant attributes, must be attended with danger to the public welfare, on the numerous occasions in which the interest of the teacher and the duty of the examiner would interfere with each other.

Your petitioners therefore most humbly pray that your Majesty will not grant to the institution which has assumed the name of the London University the power of conferring medical degrees; and they further pray that if the expediency of such a grant should be referred by your Majesty to the consideration of your Majesty's most honourable Privy Council, they may have the opportunity of being heard by counsel on the subject matter of this petition.

BRISTOL INFIRMARY.

To the Chairman and Committee on Medical Education, &c. &c.

GENTLEMEN,

IN addition to the answers to the questions contained in your circular, we beg leave to subjoin the following statement, to which we respectfully call your attention, as connected with the subject which must form no inconsiderable portion of your inquiries,—viz. *medical education.*

In the year 1735, Bristol projected, and, in 1736, carried into effect, the scheme of an hospital, supported entirely by voluntary contributions. Its foundation is believed to be nearly coeval with, if not to have preceded that of the hospitals in London, and of similar institutions in Scotland and Ireland, (royal, endowed, and chartered foundations excepted.) This institution now contains from 200 to 220 in-patients; out-patients, from 5000 to 6000: average number of casualties, about 1200 annually.

The whole range of building is uniform and extensive, (having been re-erected within these few years), furnished with baths, warm air, and other modern conveniences, and has attached to it a spacious garden as a promenade for convalescents; in a word, no expense has been spared which could contribute to the comfort and recovery of the inmates.

This institution is also enriched with a valuable and daily-increasing museum, a library, a leeturing theatre, and excellent accommodation for examinations and dissections; in the latter of which our students are almost daily engaged under the able superintendence of the house-surgeon.

The ordinary disbursements are annually from 5000*l.* to 6000*l.*; and there has been lately expended, in providing accommodation for the out-patients, about 3000*l.* in a supplementary building.

It may not be amiss here to recount the advantages which the city of Bristol possesses for a great medical establishment: with a population equal to that of most of the capitals of Europe, with a large and well-regulated hospital, and also a medical school, the student may here learn not only the principles and practice of medicine and surgery, but also witness on a great scale, the nature of most of the diseases and accidents which afflict mankind.

With the above statement before you, gentlemen, it will perhaps be a matter of surprise that there could have existed any doubt of the propriety of conceding to us the ability to qualify any students for examination; yet the fact is, that it is only since *the 25th of November*, 1831, that we have been "*recognized*" by the College of Surgeons, and even then partially, as our dressing apprentices of five years standing are at present under a peremptory *obligation* of walking, as it is termed, some one or other of the London hospitals for a period of six months. On this subject, it is remarkable that the regulation of the College should only require a *walking* attendance of twelve months in a London hospital, from young men who never before may have been within the walls of any hospital; while our apprentices have not only witnessed the practice of the Bristol Infirmary, but have been constantly employed as *dressing* pupils, and in the charge of casualties, for a period of *five* years.

This representation, we presume, entitles the Bristol Infirmary to be justly considered an hospital of the first class. Even in the metropolis there are not above four or five that exceed it, either in size or importance, as a school for scientific and practical surgery; and none surpass it as to the regulations established by its governors for the education of the students. It may be asserted, that no hospital in the kingdom affords greater opportunities for the acquirement of medical and surgical knowledge. On this subject the surgeons are desirous of the fullest investigation; and, as some proof, they beg to refer to every one of their students who have attended in London, who will confirm the fact from their own experience and observation: besides, it is well known that students who have received their medical education at this infirmary have acquitted themselves with marked ability as candidates for their diplomas, when under examination at the College of Surgeons and Apothecaries' Hall.

For these and many other reasons the surgeons of the Bristol Infirmary have ever considered the following regulation of the council of the Royal College of Surgeons as oppressive and unjust; but they are willing to believe founded on an erroneous view of the opportunities the Bristol Infirmary affords of cultivating medical science:—Pupils and apprentices, when candidates for the diploma, must bring proof "*of having attended during twelve months the surgical practice of a recognized hospital in London, Dublin, Edinburgh, Glasgow, or Aberdeen, or for six months in any one of such hospitals, and twelve months in any recognized provincial hospital.*"

The surgeons of the Bristol Infirmary are of opinion, that the above regulation of the Royal College of Surgeons, which requires an attendance of six months as a walking pupil at one of the hospitals in London, in addition to an attendance during one or more years at a recognized provincial hospital, as a necessary condition for obtaining the diploma of the College, is highly objectionable, and for the following reasons:—

1st, Because it is unnecessary: a student may spend the prescribed time quite as profitably in attendance at a recognized provincial hospital as upon those in London. In fact, with the exception of four or five of the metropolitan hospitals, the recognized provincial hospitals are upon a larger scale than those of the metropolis.

2d, Because it is injurious to the morals of students. It is a notorious fact, that students, after a residence of only a few months in the capital, frequently fall into practices of dissipation and immorality, foreign to their previous habits, and ruinous to their future usefulness, respectability, and happiness.

3d, Because it has a tendency to render provincial education less efficient than it would otherwise be. It has often been observed, that students neglect to make a proper use of the advantages afforded to them in their attendance at a recognized provincial hospital, from a notion that the full prosecution of their studies may be delayed until their residence in London, where they imagine that the opportunities of acquiring professional knowledge will be more worthy their attention; and in consequence they crowd into that brief period a far greater number of pursuits than their time or their previously-formed habits and inclinations will enable them to follow with any prospect of advantage.

4th, *Because it occasions a very considerable increase in the pecuniary expenditure of the student, without advantages in any degree correspondent to the extent of the outlay.*

Lastly, Because it implies an invidious distinction between the surgeons of the recognized provincial hospitals and those of the metropolitan institutions; a distinction which cannot be supported by a fair comparative estimate of their respective attainments and capabilities.

In addition, we beg to draw your attention to the circumstance, that not only has the time prescribed for professional study been of late considerably lengthened, with a corresponding augmentation of expense, but the number of branches of science which must be attended to, has also been greatly increased.

Gentlemen, we will take up your time no longer than to submit to you, that we require redress, and to repeat that the Bristol Infirmary has a fair claim, in

every point of view, for admittance into the first class of hospitals in the United Kingdom.

We have the honour to be,
Gentlemen,
Your most obedient servants,
(Signed) Richard Smith.
William Hetling.
Richard Lowe.
Henry Daniel.
Nathaniel Smith.

The Surgeons of the Bristol Infirmary.

Bristol Infirmary Consultation Room,
April 22, 1834.

To Henry Warburton, Esq., M.P., &c. &c., Chairman of the Committee on Medical Education, House of Commons, London.

ROYAL INSTITUTION.

Friday, April 25.

ON THE PYRAMIDS OF EGYPT.

Mr. Davidson gave an interesting lecture on this subject, which he began by correcting some errors into which the commentators and interpretors of Herodotus have fallen. He then described the locality in which the three great pyramids are situated, traced the various passages and chambers which have been explored in that of Cheops, and mentioned many particulars derived from personal acquaintance with these structures. In conclusion, he gave his opinions at some length on the probable origin and purpose of pyramidal structures, as they are found in different parts of the world, in Mexico as well as in Egypt. They seem to have been erected soon after the flood, probably about the time the tower of Babel was built, and with a similar design. But there appears to be abundant evidence that sacred purposes were kept in view by those who had the ordering of their erection; and that many of them have been used as repositories for the dead, admits not a reasonable doubt.

NOTE RELATIVE TO MOSCATI.

To the Editor of the Medical Gazette.

Sir,

My name having been mentioned in Moscati's pamphlet, and particularly in that part of it quoted in the last number of your journal, I beg to state that the assertion therein made, of my having procured for him a card of invitation to the evening meetings of the College of Physicians, is *totally false and unfounded.* I never obtained for him a card, or admitted him to any meeting at the College; nor indeed did he ever make application to me on the subject.—I am, sir,

Your obedient servant,
John B. Sedgwick.

Royal College of Physicians,
April 29, 1834.

WEEKLY ACCOUNT of BURIALS,

From Bills of Mortality, *April* 29, 1834.

Abscess	1	Hooping-Cough	19
Age and Debility	35	Inflammation	28
Apoplexy	5	Bowels & Stomach	3
Asthma	13	Brain	1
Childbirth	4	Lungs and Pleura	6
Consumption	53	Insanity	2
Convulsions	27	Liver, diseased	3
Croup	5	Measles	10
Dentition or Teething	10	Mortification	1
Diarrhœa	1	Paralysis	4
Dropsy	11	Rheumatism	1
Dropsy on the Brain	16	Small-Pox	2
Dropsy on the Chest	1	Sore Throat and Quinsey	1
Dysentery	1	Spasms	3
Erysipelas	1	Stone and Gravel	1
Fever	7	Unknown Causes	1
Fever, Scarlet	4		
Fever, Typhus	1	Stillborn	12
Gout	2		
Heart, diseased	1		

Decrease of Burials, as compared with the preceding week . . . 178

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

April, 1834.	Thermometer.		Barometer.	
Thursday . 24	from 32	to 55	30·24	to 30.25
Friday . . 25	31	59	30·19	30·12
Saturday . 26	37	56	30·03	29·85
Sunday . . 27	40	53	29·60	29·30
Monday . . 28	41	60	29·31	29·35
Tuesday . . 29	46	64	29·43	29·49
Wednesday 30	45	65	29·46	29.54

Wind variable, S. prevailing.
Rain frequent in showers on the three last days; otherwise generally clear.
Rain fallen, ·35 of an inch.

Charles Henry Adams.

NOTICES.

The letter of a General Practitioner, on which we have some remarks to offer, has been left out this week for want of room.

Pendulum is too severe. He is over anxious to crush a bruised reed.

The paper on the Lobelia Inflata is quite too voluminous for our pages. It would require careful abridgment and much condensation before we could venture to lay it before our readers.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MAY 10, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XXXI.

DIFFICULT LABOUR.

CAUSES REFERABLE TO THE OVUM.

I have already said that the causes of lingering labour referable to the ovum are described as being,—*first*, preternatural toughness of the membranes; *secondly*, the head being larger than usual, either from natural healthy formation, deformity, or disease; — *thirdly*, the head being too strongly ossified, though not of larger dimensions than ordinary;—*fourthly*, malposition of the head;—*fifthly*, ascites or tympanites of the fœtal abdomen;—*sixthly*, the umbilical cord being unnaturally short, or being twisted around the body or limbs of the fœtus;—*seventhly*, unusual bulk of the trunk or limbs, arising either from exuberance of natural growth, or from, *eighthly*, monstrosity. Some of these causes exert a most powerful influence in retarding labour; while others,—as far at least as delay in the expulsion of the head is concerned,—must be considered of little or no account.

PRETERNATURAL TOUGHNESS OF THE MEMBRANES.

This is by no means a very frequent cause, nor is it difficult to overcome, when clearly distinguished: it is, indeed, by far more common for a premature rupture of the membranous cyst to produce a protraction of the process; since the passages are then deprived of the advantage of that soft dilating medium which it offers when entire. If, however, the membranes be exceedingly strong, as occasionally they are, — although possessing their usual thinness and pellucidity, — it is evident that the very circumstance of the bag remaining whole after the full dilatation of the parts is effected, will necessarily more or less prolong the labour; since the ovum must either pass unbroken, or a greater force than ordinary must be exerted by the uterus to destroy its integrity. It is not to be expected that the ovum will be expelled whole, provided the term of gestation be nearly perfected; nor indeed is such an event desirable, because of the dangers which must accrue both to the mother and the fœtus;—to the mother, in consequence of the great probability of hæmorrhage from the sudden emptying of the uterine cavity;—to the fœtus, from its being deprived of the means of life through the placental circulation before it can enjoy the equivalent advantage of respiration. Some instances, indeed, are recorded in which the ovum, nearly matured, has passed entire; but these are exceedingly rare.

I have already laid it down as a principle, that in ordinary cases, so far from desiring the early rupture of the membranes, we should be anxious to preserve them as long as possible;—until, indeed, they have perfectly opened the os uteri, distended the vagina, and somewhat protruded externally. As soon, however, as they have appeared in the least outward to the vulva, we may suppose that all the advantage which can be derived from them has been gained; and, should they still resist the power of the uterine contractions, we may conclude that their preternatural toughness is retarding the exit of the head.

Treatment.—In this simple case it is only necessary to perforate the bag with the extremity of the finger, a pointed quill, or a stilette: the waters will escape; the head of the child will then enter the pelvis, if it

has not previously done so; and,—provided this be the sole cause of delay,—the difficulty will immediately vanish.

It is remarked by some obstetrical teachers, that as the urinary bladder occasionally prolapses before the head of the child under labour, it might be mistaken for the membranous cyst, and be ruptured; and I have heard Dr. Hamilton state, that he has actually known an incautious practitioner so far forget himself as to perforate the coats of the bladder in mistake. It cannot be necessary for me to give you the diagnostic marks between the bladder and the membranes of the ovum; I should presume that no man who has attended in the least to the science of midwifery, can make such an egregious blunder.

It appears to me much more probable, that the prolapsed bladder should be mistaken for a watery cyst, the result of diseased action,—an enlarged ovary, for instance,—and that it should be punctured with a sharp instrument, under that erroneous impression. The diagnosis would be most easy; the introduction of the catheter would withdraw the fluid, and dissipate the swelling; and I should really hope no person would propose tapping a fluctuating tumor in the pelvis, through the vagina, without previously evacuating the bladder. So obvious is the necessity of such a proceeding, so simple, easy, and satisfactory, is this mode of arriving at the truth, that when speaking of pelvic tumors, I neither thought it worth while to insist on your using the catheter, as a means of diagnosis, nor to warn you against falling into such a fatal error, as mistaking a distended bladder for an enlargement of morbid growth.

HEAD PRETERNATURALLY ENLARGED.

The second cause referable to the ovum is a preternaturally large head, either from healthy formation, monstrosity, or disease. We have already learned that the size and weight of infants at birth vary exceedingly;—that my father was once present at the birth of a child which weighed sixteen pounds and a half avoirdupois; and we may naturally conclude, that when the general bulk so prodigiously exceeds the common average, the head will partake of the exuberant growth, and occasion a proportionate difficulty under labour.

In such a case it is probable that the true cause of protraction will not be discovered until the head have entered the pelvis, or engaged somewhat in the superior strait. But its mere extraordinary size would not influence our treatment, or abrogate the general rule—that we should desist from interfering instrumentally until symptoms supervened indicative of distress, and requiring relief.

Rare as the last cause of protraction must necessarily be, it is still more uncommon for a monstrous formation of the head to impede its transit: the most usual irregularity in development is a want of brain, of which you have some striking specimens before you; and such a malformation can offer no impediment under labour. It is possible, however, for other deformities to exist; and each of these cases must be treated according to its own peculiarities, since no general rule can be laid down for our guidance, except that so frequently quoted.

Children are occasionally born with tumors attached to the head, as you see in the instances on the table. These usually contain fluid, and would offer but little resistance to the accomplishment of the process of parturition. A collection of water within the fœtal skull itself—constituting congenital hydrocephalus—is a less infrequent disease; though this is also very rare. It has been my lot, however, to meet with such an enlargement on many occasions. The quantity of fluid effused is sometimes extraordinarily great. This fœtal skull [presenting it] would hold a pint, independently of the brain; but I have known instances where the head was very much larger than the one I hold in my hand, even previously to birth.

Now although the relative normal size which ought to exist between the pelvis and the head is here destroyed, and the difficulty and danger must be in proportion to the dimensions the head has acquired, it does not follow as a matter of necessity that the woman would die undelivered if art did not step in to rescue her: for I have already mentioned a case in which a head, containing a pint of fluid, was squeezed whole through the pelvis, to the great danger of the sacro-iliac ligaments and the pelvic viscera themselves. In other instances, when putrefaction has occurred to a considerable extent before the commencement of labour, I have known the scalp to burst, and the fluid to be evacuated: the bones have then collapsed, the difficulty has been over, and the flattened head has protruded. But in other cases,—and they are by far the most frequent,—instrumental aid will be found necessary before delivery can be effected.

There is great danger in allowing a fœtal head, enlarged by hydrocephalic disease, to remain for a long time locked in the pelvic cavity; because, as I have before mentioned, from its compressibility and the open state of the fontanelles, it so completely adapts itself to the shape, and moulds itself into the irregularities of the cavity, as to produce constant, strong, uninterrupted, and almost universal pressure upon the lining structures, to their im-

minent and certain hazard. We should naturally expect sloughing to occur: the bladder and the rectum might be implicated, and a fatal termination result.

Such being the dangers attendant on this case, it becomes a matter of the greatest possible consequence, that we should detect a hydrocephalic head as early in the process as possible: nor is the diagnosis difficult. We may ascertain the existence of the disease by the volume of the head being so much greater than ordinary, by the bones being so much wider apart, the fontanelles and sutures being more open and discernible, and by there being a certain degree of fluctuation evident within the skull. These peculiarities it will certainly not be easy to discriminate before the os uteri is dilated to a moderate extent; or if we are content with inquiring by the first finger of the right hand: but I have before laid it down as a maxim, that we should introduce two or more fingers of the left hand, to determine the cause of delay, provided the labour be not progressing satisfactorily; and even should the pelvic cavity be but little occupied by the head, we shall in most instances be able, with care and attention, to satisfy ourselves of the true nature of the case.

Treatment. — Having, then, detected a hydrocephalic head either above the brim or occupying the cavity of the pelvis, what must be our practice? —Are we to act on the principles I have before so often enjoined, of waiting as long as possible, compatible with the patient's strength, before affording any means of relief?—are we to incur the hazard of contusion, inflammation, and sloughing?—are we to run the risk of the patient's powers becoming exhausted by useless struggles;—of her system being so much depressed as to endanger her sinking? —Certainly not. When we have ascertained that nature is unable to overcome the difficulty except at a great expenditure of power, we are fully warranted in having recourse to early perforation, that the fluid may be evacuated, and an opportunity afforded to the bones to collapse; the case will then most probably be terminated by the contraction of the uterus alone. I think myself justified in offering this recommendation, because of the danger of inflammation, and all the dreadful consequences which may follow impaction of the head —because of the slight probability there exists of the ultimate preservation of the child's life. Suppose even that the infant was born living, is it likely to survive for any length of time?—Is it probable that the disease, originating in an early period of pregnancy, will be removed, or even suspended?—Are we not rather to expect that it will go on increasing, to the ultimate destruction of the little sufferer?—Is the child, then, likely to be a comfort to its parents?—Is it likely ever to enjoy the perfect possession of its faculties, whether corporeal or intellectual?—Is it likely to become an useful citizen, or valuable member of society?—The probability is much against even the least of these advantages.—Can we, then, for a moment put the woman's safety in competition with the preservation of a hydrocephalic child?—If it be objected that life must necessarily be destroyed by adopting the measures just recommended, and that it is the duty of the physician to preserve life, if possible, under the most aggravated circumstances of pain, misery, helplessness, and fatuity, I would answer, here is life pitted against life;—the life of the mother of a family, in other respects healthy, against the puny, slender, scarce animal vitality of a diseased infant. Even should the child pass alive, and the woman also be preserved, her structures must be seriously endangered; and two miserable instances of sloughing, when the head was full of serous fluid, have come under my own immediate notice, occasioned by the praiseworthy— though in the case under consideration, falsely-founded — horror inspired by the idea of craniotomy. In following up this practice, however, let us beware of error; —let not our ignorance lull us into a fatal assurance. Let us be *perfectly certain* of the existence of disease in the fœtal head before we take the perforator in hand. What an appalling and sickening feeling must overspread the mind of that man who plunges the murderous instrument into the centre of the brain of a living, healthy fœtus, under the erroneous belief in the presence of hydrocephalus! What would his sensations be, when, instead of the expected water, a stream of pure and unmixed blood flows from the inflicted wound! What bitter remorse must overwhelm him, when, after the keenness of the first shock has passed away, leisure is afforded him to contemplate the rashness of his conduct!—The mischief is done;— the death blow is struck;—the act is irrevocable!

UNUSUAL FIRMNESS AND MALPOSITION OF THE HEAD.

As occasionally an exuberance of growth takes place throughout the whole fœtal body, so at other times we observe some of the systems more particularly developed than others; and this is most remarkable in regard to the skeleton. The cranial bones partaking of this increased deposit of osseous matter, become thicker, harder, and firmer than is usual; the membranous spaces which separate them from each other are diminished in extent; and such a degree of solidity is imparted to the en-

tire head, that it is incapable of undergoing that compression which so materially lessens its lateral diameter, and so much facilitates its transit. Proportionate difficulty will therefore be produced under labour, and the same effects will result as though the head was actually of extraordinary size.

It is not probable that this peculiar conformation will be detected early in the labour; but when delay in the descent of the head appears, we may be able to satisfy ourselves both that it is not larger than common, and also that it is more strongly ossified than usual, by the introduction of two or more fingers of the left hand up to the pelvic brim, as before more than once advised. This latter indication we may collect as well from the preternatural solidity of its feel, as from the indistinctness of the sutures and fontanelles, and the small space which they occupy.

Regarding the *treatment* of such a case, I have nothing to offer beyond the instructions so often inculcated;—that we should wait as long as is consistent with the woman's safety; and, when compelled, use those means most applicable to the case: the long or short forceps, if the head have descended sufficiently low to lie within their grasp; the perforator, if by its agency alone we can snatch the patient from impending death.

Having already fully discussed the subject of malposition of the head in the lecture on *irregularities of head presentation*, it cannot be necessary for me to enter again upon this cause of lingering labour; and as far as respects the treatment, I have nothing to add to the instructions I formerly gave.

ASCITES AND TYMPANITES OF THE FŒTAL ABDOMEN.

An effusion of fluid will sometimes take place during fœtal life into the thoracic and abdominal cavities: both hydrothorax and ascites, however, as congenital diseases, are very rare; the latter is perhaps the most frequent of the two. It is not likely that any difficulty to the passage of the child would be produced by a collection of water in the chest; nor would an abdomen enlarged from the same cause, however much increased in bulk, offer any impediment to the birth of the *head*. Delay would occur, however, in the transit of the *body*; and if means of relief were not applied, the woman might sink under her sufferings, although the child were partly in the world.

The case would be known by the shoulders remaining at the outlet of the pelvis after the birth of the head, resisting both the expulsive powers exerted by the uterus, and the extractive efforts of the medical attendant. On the hand being passed into the pelvis, along the body of the child anteriorly, it would detect the abdomen, large and distended, soft and fluctuating to the feel, entirely blocking up the pelvic brim.

If our endeavours to perfect the birth by traction at the neck, or by hooking the finger or some blunt instrument under the axilla, were not crowned with success, we should be compelled to diminish the bulk of the body by evacuating the contained fluid. This could easily be effected by a trocbar, or even by the obstetric perforator. The only objection which could be started to the performance of this operation, consists in its apparent cruelty; but every consideration must give way to the preservation of the woman's life; and we shall mostly find, that the child has ceased to exist before this means of delivery has become necessary. Unless it be breathing vigorously, the pressure exerted on the umbilical cord will most likely have destroyed it; and that pressure must have been carried to a great extent, if we are unable to withdraw the body without puncturing the peritoneal sac.

Tympanites is the effect of putrefaction; and gas will be generated in the abdominal cavity, in the intestinal canal itself, and in the cellular structure underneath the skin.

We can have no difficulty in determining that putrefaction has occurred after the head is born; the cuticle will desquamate most easily, and the scalp itself will be emphysematous. If, under this state of things, difficulty occur in the passage of the shoulders, we can be at little loss to understand the cause; and should we be disappointed in our attempts to liberate the infant by the finger, or blunt hook passed around the shoulder, we must here also perforate the abdomen, let out the air, and give an opportunity for the body to collapse. The diminution in bulk will then readily allow its extraction. When putrefaction has taken place, we cannot hesitate to operate in the manner recommended; for the child being certainly dead, no additional injury can be inflicted on its person.

SHORTNESS OF THE FUNIS UMBILICALIS.

Another cause of lingering labour attributable to the ovum, has been supposed to be shortness of the umbilical cord. I have already shewn that the funis umbilicalis varies to a very extraordinary degree, both in length and thickness, but particularly in length; so that it sometimes measures five or six feet, and in other cases has been known scarcely to exceed six inches. Presuming it to be as short as some of these upon the table, it has been said that that

circumstance alone would prevent the ready passage of the head. This was particularly the opinion of the ancients, who considered that the child by its own efforts fought its way out of the uterine cavity; and that these efforts would be frustrated and rendered of no avail, by its being tethered, as it were, to the uterus, and on that account incapable of liberating itself. I have shewn you satisfactory proofs that the child is a perfectly passive body under labour; that no exertions of its own facilitate its escape; and therefore this reasoning must fall to the ground. You will recollect that I told you, under the action of its fibres, the fundus uteri descends, and follows, as it were, the child's body; there is, therefore, always nearly the same distance between the umbilicus of the child and the placenta, whether the uterus be perfectly quiescent, or whether it be acting vigorously. So far, then, as the head of the child is concerned, the shortness of the funis umbilicalis can produce no such impediment to its exit as to cause a lingering labour.

But the case is different when the head has passed, and the shoulders are about to escape; then, if the funis umbilicalis be preternaturally short, or rendered so by being twisted round the body or limbs of the fœtus, a difficulty in the expulsion of the shoulders may be experienced, or dangerous consequences may be produced;—the placenta may be prematurely separated from its attachment, or its mass may be broken; a portion may be expelled, and the remainder, retained in utero, may give rise to violent hæmorrhage. We may suspect that a preternatural shortness of the cord impedes the passage of the shoulders, provided we find, after the head is born, that the body of the child does not advance, although the uterus continues to act strongly; that no preternatural enlargement of bulk exists; and if, on endeavouring to pull down a loop of the cord, we find it tense and tight, resisting all our efforts to withdraw it.

Treatment.—In a case of this kind it would be right not to hurry the extraction of the child, provided it be breathing freely; but to obtain all the advantage derivable from the contraction of the uterus. By this means we shall probably avoid the risk both of immediate and eventual hæmorrhage: for as the uterus contracts more perfectly, the body will be expelled, and the placenta will most probably be separated at the same time. The same impediment may be produced, if the funis be coiled around the child's neck. I have already adverted to the possibility of this occurrence, the accidents it may occasion, and the mode of preventing them.

UNUSUAL BULK OF THE TRUNK OR LIMBS FROM EXCESSIVE DEVELOPMENT—MONSTROSITY.

We sometimes, though rarely, find that the different fœtal members do not grow in their just proportion, but that some are deficient, while others are abundant in development. Incarcerated within this glass jar you see a fœtus measuring in length twenty-four inches, whose shoulders are seven inches across (the average width being under five); while the cranium is smaller than ordinary! Such a prodigious bulk would necessarily occasion difficulty after the head had passed; and the case must be met by the ordinary means. Taking especial care, if the child be alive, not to injure the arm or the shoulder-joint, the finger, the corner of a handkerchief, or a blunt hook, must be insinuated first under one axilla, then under the other, traction may be made in the manner I now shew you; and by perseverance our object will generally be effected: for the compressibility of the viscera, and the elasticity of the thoracic parietes, are fortunately so considerable, as to allow of a great diminution in capacity, and permit the extraction of the body through a comparatively narrow channel. In making these efforts, however, we must bear in mind the delicacy of the structures on which our purchase is fixed: we may break the humerus, separate the epiphysis, or dislocate the articulation,—accidents all of serious consequence,—unless we use the power we are in possession of with the utmost tenderness.

It may be our fortune to meet with other more rare and more complicated species of monstrosity. On the table before you are two specimens of a double fœtus, both having arrived at the full period of intra-uterine maturity;—the one two perfect children, joined together from the upper edge of the sternum to the pubes,—each possessing a head and proper complement of limbs;—in the other instance the individuals are attached to each other by the side of their trunks; so that to the two heads are appended apparently a single body, with two arms and two legs only. I need not insist on the difficulty and danger attendant on such a birth.

It is not my intention at present to enter into the speculations designed to account for such productions; an after opportunity will, perhaps, be afforded us for the discussion of that subject: nor can I practically offer you any rules for your guidance, should such a malformation fall under your particular notice;—the management of each case must depend on its own individual peculiarities.

These, then, are the only causes which I can suppose to operate in inducing lingering labour, all of them possessing an influence to a certain extent; some of them very much impeding the expulsion of the head, and others the passage of the body when the head is born. But it must not be forgotten, that a labour may be rendered tedious by two or more of the causes enumerated, acting in concert. Thus an unfavourable position of the head may exist, in concurrence with atony of the uterus, or rigidity of parts; or all with a diminished capacity in the pelvic apertures.

MANAGEMENT OF THE PATIENT UNDER COMMON LINGERING LABOUR.

I shall now briefly offer a few recommendations regarding the general management of a woman under common lingering labour; and I consider it a matter of the utmost importance to insist on these apparently trifling circumstances, because the present comfort and future well-being of the patient mainly depend on our own conduct, and the regulations we lay down for her management.

The chamber should be preserved cool and quiet, to avert fever and entice sleep. It is highly necessary that she should not be kept in one posture, because of the inconvenience, the irksomeness, and additional distress a constrained position must occasion. She may stand, walk, or lie, alternately,—especially during the first stage,—or place herself in any situation under which she is least uneasy. We must, by every persuasive argument, prevent her from bearing down, or using any voluntary efforts, for the purpose of aiding the action of the uterus. The attendants in the lying-in room often suppose that, when a certain number of hours have elapsed since the commencement of labour, a proportionate progress must necessarily have taken place; and accordingly, with the best intentions, they are constantly urging the patient to exert those powers which are under the guidance of her own will, in the belief that such exertions will facilitate the child's birth.

After what I have said, I need not call to your recollection the uselessness and danger of this untimely exercise of the assistant muscles: not only may the strength be prematurely expended, which should be husbanded for a future period; but injury may arise from the too forcible propulsion of the head against the undilated and unprepared passages.

Nor must we think it immaterial to regulate her diet. I have already said that solid food should not be allowed under labour,—and this observation holds good, particularly with regard to a protracted state,—because the nervous energy, being principally directed into other channels, digestion goes on but imperfectly. For reasons, also, before given, stimulants should be avoided: nourishing fluids may be given, *ad libitum*, and the blandest are generally the most desired.

A great objection is often made to the exhibition of cold, and especially acidulated drinks, under lingering labour; on what grounds I cannot understand; and therefore I would by no means interdict them, if they are grateful and palatable. Effervescent draughts, and the subacid fruits, will often be found highly refreshing.

With regard to our own conduct, for the reasons before mentioned we must abstain from frequent examinations, and from close attendance at the bed-side of the patient. By too great assiduousness during the first stage, we shall either impress her mind with injurious anxiety, or induce her to believe that the labour is on the point of being completed; and we shall perhaps be adding disappointment to bodily suffering. We must not be carried away by the calls of the patient for "help," however importunate she may be; but reason calmly with her, and assure her that, when the period arrives at which our assistance can be useful, our best endeavours shall be exerted to mitigate her sufferings. We must speak cheerfully both to her, and in her presence; and endeavour to preserve not only her confidence, but her spirits: for the feelings and the passions exert a most powerful influence over the progress even of natural labour.

The most important duty of all, however, which we have to discharge under lingering labour, is carefully to watch the state of the bladder. Every three or four hours we should place our hand on the vesical region, to ascertain whether it has become materially distended. It is less difficult to gain this information under labour than when the uterus is unimpregnated, because, in the latter case, the organ falls lower into the pelvic cavity, and becomes somewhat buried within the surrounding viscera: but when the abdomen is pretty nearly filled by the enlarged uterus, and the pelvic cavity is more or less occupied by the child's head, the bladder cannot retire either backwards or downwards, but is thrown forwards, and becomes so much the more evident to the hand. In making this examination—the patient lying on her left side—we pass the right hand between the thighs; and, presuming the membranes are ruptured, we feel rising, even above the umbilicus, a hard, firm, solid tumor, which is the uterus itself, on which we can make no impression,

and which is observed to be sometimes harder and sometimes softer, in proportion as alternate contraction and relaxation take place in its fibres. Beneath this we shall find another tumor, more circumscribed in shape, occupying the hypogastric region, just peeping above the pubes, encroaching more or less on the cavity of the abdomen; varying, therefore, in size, according to the quantity of urine it contains, and giving a certain degree of indistinct fluctuation to the hand,—sufficiently perceptible, however, for us to determine that the tumor is the distended bladder. But we must not suppose that in all cases we shall feel the bladder, although it contain a considerable quantity of fluid, distinctly evident in its usual situation; because it may have subsided to the right or left side, and, instead of being found in the centre of the hypogastric region, it may be on one side of the enlarged uterus, appearing above one or other groin; or it may have prolapsed before the head of the child, offering itself as a soft tumor in the pelvic cavity. Having our mind, then, directed to such possibilities, we must not at once conclude that it is empty, although it may not be discoverable immediately above the pubes.

It is of the greatest possible consequence that we should not permit much urine to collect under protracted labour; not only because a distended bladder both adds greatly to the suffering endured, and interferes with the efficient action of the propelling powers, but also because of the danger incurred of injury to its own structure. It may burst,—inflammation may attack its lining membrane, which may terminate in the destruction of its coats; or a fistulous orifice may be formed between its neck and the vaginal canal,—which disastrous accident is much more likely to happen under an accumulation of water within its cavity, than if it be kept in a collapsed state.

For information respecting the condition of the bladder, we must depend only on our own personal examination, and not trust to the declarations either of the patient or her nurse; for we are often told, in answer to a general question, that the water passes plentifully and freely; but if we are more minute in our inquiries, we find that fluid dribbles away, as the patient lies, with each return of uterine action; and that the bladder has not acted voluntarily for many hours. This fluid may be the liquor amnii, or it may be urine squeezed out of the bladder by the compression exerted on that viscus by the abdominal muscles. In the latter case, it may be known by its urinous odour; and the very circumstance of its being forced out thus involuntarily is a proof of the cavity being over-distended, or at least containing a considerable quantity; for if it were entirely or nearly empty, this dribbling would not occur. It is, then, the surest indication of the necessity of having recourse to artificial evacuation.

The cause of this inability to pass urine under labour will mostly, if not always, be found to consist in the stricture formed at the neck of the bladder, or in the course of the urethra, by the compression those organs suffer between the fœtal head and pelvic bones. I shall not now enter at all into the mode by which the catheter is introduced,—that will most naturally come under our consideration when we are treating of the diseases of the external organs of generation: it is sufficient for our present purpose that I call your attention,—which I wish to do most strongly,—to the necessity of paying strict attention to the state of the bladder under lingering labour; and to the circumstances which sometimes arise to deceive and to confound our judgment.

TWO CASES OF GALL-STONE,

With Observations.

By Dr. Borrett, of Norwich.

(Communicated by W. P. Borrett, Esq.)

Case I.—*Discharge of Gall-stones through the Abdominal Parietes.*

A woman, the mother of fourteen children, became the subject, in 1824, of chronic inflammation of the liver, which was complicated with dropsy. A mercurial inunction, continued during one month, removed the complaint, and her health was restored.

Two years and a half from that time, after suffering for some weeks "sharp nipping pains," (to use her own expression), she observed a swelling above and to the right of the navel. The part was very tender, and every movement of the body caused excessive pain; so much so, that a posture of the body, inclining forwards, and to the right side, was constantly maintained. The tumor, of whose nature mere conjecture could be formed, was repeatedly leeched and poulticed: it continued to increase, and at length discharged a concrete substance of great size, which was found on the poultice in the morning. I was sent for in alarm (says my brother) at the strange occurrence, and found that a calculus had passed through an opening about an inch above, and to the right of the umbilicus. The aperture contracted; but not closing, a colourless

discharge continued to flow through the fistulous opening in such a quantity as to soak, during the day, a napkin several times folded.

Upon examining the calculus, which has a smooth concave surface at one end, I suspected there was another with which it had been lying in close contact, in the gall-bladder. About two years after, the patient having complained of similar nipping pains, a second gall-stone came away by the same opening. Up to my last visit in 1830, a copious limpid fluid, always unmixed with bile, exuded from the opening, fortunately to the complete cure, as the patient supposed, of her dropsy.

There being a convexity at one end of the second calculus, corresponding to the concavity of the first, and the other end being flattened, as if by attrition also, no doubt remained of the presence of even a third calculus in the same receptacle. In 1832, the patient died; and a third calculus was found in the neck of the gall-bladder, where it contracts into the cystic duct. The gall-bladder was seen to adhere by its fundus to the abdominal parietes, and its coats were thickened, and it contained no bile.

The calculus which came away first was analyzed, at my request, by Dr. Turner, Professor of Chemistry to the London University, whose pupil I had been. "The concretion which you left for me," he says in his note, "consists of the same substance as biliary calculi; it is composed of cholesterine, in a state of minute crystalline scales, tinged in some parts of a yellow, in others of a green hue, by the colouring matter of the bile." Its form is cylindrical, its long diameter above two inches, and its weight six drachms, or 360 grains. The second calculus is not more than half the size: it is in the possession of the family, and has not been analyzed.

The circumstance of two calculi having been protruded externally by the same exit, is of itself remarkable; but the extraordinary size of that which came away first is particularly deserving notice. I only know one instance in which it is exceeded. In a case published by Mr. Blagden, in the Medical Transactions of the College of Physicians, there was abscess of the liver, which pointed externally, and a calculus of the enormous weight of nearly one ounce and a quarter was discharged in the same direction. The patient, a lady, sixty-six years of age, gradually recovered.

Of other recorded cases of gall-stone, the most remarkable are those of Dr. Craigie, Mr. Brayne, and Mr. Thomas. The two latter are published in the Medico-Chirurgical Transactions, and the first in the Edinburgh Medical and Surgical Journal. The largest of these calculi did not exceed 228 grains; they were all voided "per anum."

When calculi are evacuated "per anum," they must have passed either through the ducts, or directly into the intestines. The connexions of the gall-bladder with the duodenum and arch of the colon, explain how they are able to pass from the bladder into one or other of these viscera,—adhesive inflammation being set up at first, and a perforation afterwards effected through the united media by the process of ulceration. It is most probably in this way that they are generally expelled; and it is really wonderful to see what efforts nature is capable of making, as Mason Good observes, to free herself of a morbid incumbrance. Dr. Pemberton says,—"I have in my possession a calculus, whose longest diameter is two inches and a quarter; shortest diameter, one inch and a quarter; passed, after five months of acute suffering, by ulceration, as I believe, between duct and duodenum, as it is unlikely the ducts would admit of such distention." The ducts, however, are very dilatable: Dr. Baillie has noticed them to be enlarged to the diameter of half an inch; and Cruveilhier to that extent that they were almost equal in capacity to the small intestines.

From the fact that the patient never complained of pain, or experienced any symptoms of gall-stone, one may conclude that the calculi lay in the gall-bladder, where they occasioned no inconvenience. This accords with daily observation. How often are calculi found after death, or voided "per anum," where the subjects of them never had jaundice, nor, in fact, any disorder which could be attributed to their presence. Not only may they be present in the gall-bladder, but they may even increase to an enormous size, or abound in considerable numbers, and yet be borne with perfect impunity.

Heberden relates a case analogous to the one I have given. A woman, fifty years of age, had a tumor near the umbilicus, which was opened, and continued to discharge for some years. She was suddenly seized with acute pain, shivering, and nausea; and in

three days' time a calculus came away, which was three inches in length, and weighed 245 grains. A thin yellow discharge, he says, exuded for fourteen days, when the opening closed, and the patient got quite well. In this case, the discharge is characterized by the epithet "flavus:" in that before us, on the contrary, it is described as always colourless. Was it to be considered, therefore, merely an increased serous secretion of the lining membrane of the gall-bladder, unimpregnated with bile, or rather the biliary secretion itself, in an extremely dilute and attenuated state,—such as is noticeable, more or less, in all asthenic cases, with languid habit, pallid hue of countenance, feeble pulse, absence of pyrexia, &c.; among which dropsy of the passive kind, as was here the case, may be included, in which the watery parts of the bile are not taken up, as in a state of health, the absorbing apparatus itself being affected by the debility and torpor which prevail throughout the system?

There are other points also open to discussion, which are suggested by the case before us; and I may repeat the remark made by Mr. Brayne, as it is borne out by the case I have quoted from Heberden, and by that I have given also, that the subjects of such cases are, for the most part, of the female sex, and of advanced age.

The second case exhibits a very striking contrast to the preceding one. We have seen that biliary calculi of extraordinary dimensions may be eliminated by a natural process with the production of merely local pain, and without exciting any constitutional disturbance. It will next appear, on the contrary, that the mere arrest of a gall-stone, of even diminutive size, in its passage through the duct, may give rise to such severe constitutional irritation, that even death shall be the consequence: so different is the susceptibility to irritation, and the impression of pain, in different constitutions.

Case II.—*Small Biliary Calculus impacted in the Duct—Death.*

A lady, æt. 46, the mother of a large family, complained, November 20th, 1827, of uneasiness at the stomach, at times amounting to severe pain, and accompanied with nausea. For these symptoms, which had been felt for three weeks previously, and which were ascribed by the patient to indigestion (she had never experienced any symptoms of gall-stone before), a bitter infusion, with aperient medicine, was ordered.

The following day, at noon, I was sent for in haste to my patient, who resided three miles off in the country. The urgent symptoms were acute pain at the pit of the stomach, retching and vomiting; a fluid mixed with bile had been thrown off the stomach; the tongue was clear, and the pulse natural. An ipecacuan emetic was given, and pills of cathartic extract and calomel ordered. Patient to be put into warm-bath, and I was to be informed of her state early the next morning. The report was, "the emetic had acted, but the pain continues; the bowels have not been moved." I directed the purgative pills to be repeated immediately, and prescribed a mixture for the relief of the pain, consisting of laudanum, æther, and camphor.

At one o'clock the same day (Nov. 22d), I saw my patient. She was suffering excruciating pain, bathed in profuse perspiration, and vomiting incessantly. The skin, the eye, and the urine, were deeply jaundiced; the pulse was but little affected; the bowels had not been moved. Instead of the mixture, which could not be kept on the stomach, one grain of extract of opium and one grain of camphor, were directed to be given every hour till the pain abated. The patient was put into a hot-bath, and on coming out had a blister applied over the scrobiculus cordis.

Nov. 23d.—When I got to my patient, which was a little after mid-day, she had just had a severe shivering fit; the vomiting still continued; the pulse was slow and feeble, 60 in the minute; the extremities were cold; the face pale and cold; the mind wandering. When asked whether she still had pain, she expressed herself quite free from it. Eight pills had been taken. Hot brandy and water, brandy and gruel, were now given freely: the feet and legs were enveloped in hot flannels, and warmth was applied to the pit of the stomach. The same day, at ten o'clock in the evening, I learned that my patient, having expressed a great desire to be raised up and placed in an easy chair by the bed-side, had almost instantly expired. The bowels had been acted upon, but the warmth had never returned to the extremities.

The post-mortem examination was made ten hours after death, at the express desire of the family, in consequence of the unexpected event. I had given a favourable prognosis, believing it to be a case of gall-stone.

The skin, eyes, and subcutaneous fat, of a deep yellow colour; the stomach empty and contracted; slight vascularity of ileum; the intestines otherwise healthy. The liver large, gorged with blood; its substance lacerable, not unlike the spleen, and its peritoneal coat easily stripping off; on incision, dark blood and thick yellowish-green bile flowed very freely. The gall-bladder empty, contracted, and in substance fleshy. In the hepatic duct, just at its junction with the cystic to form the common duct, there was impacted a calculus of nearly spherical form, its long diameter five-eighths of an inch. The hepatic duct was distended to the utmost with bile, the cystic and common ducts contracted. The other organs in the abdomen were in a natural state. In the thorax the lungs were collapsed; the right auricle and ventricle of heart were empty; the left ventricle contained fluid blood; the aorta a fibrinous concretion. The head was not examined.

In cases of gall-stone, we find the patient, after long suffering, suddenly relieved, the calculus having effected its passage into the duodenum, or, it may be, slipping back again into the gall-bladder. If rupture of the duct ensue, the case would be likely to prove fatal; but there is nothing in the post-mortem appearances, in this instance, to account for the catastrophe. Hence, as well as from the history of the case, it is evident that death can only be ascribed to the excessive irritation upon the system at large, the intense suffering, agonizing pain, and incessant vomiting, which was occasioned by the presence of the gall-stone in the duct, and followed by such a state of exhaustion and prostration, that the feebly-propelling action of the heart was unable to counteract the effect produced upon the circulation by the removal of the patient from the recumbent to the upright posture—a step, apparently harmless to ignorant persons, but which has been frequently known to prove fatal in a very depressed state of the vital powers.

The total obstruction to the flow of bile, the complete insensibility of the bowels to the operation of powerful purgatives, and the condition of the brain, which was rather that of "nervous apoplexy," (as it has been termed), such as follows upon the shock of very severe accidents, and is very different from the comatose form which is observed to supervene towards the termination of fatal cases of jaundice, are well deserving of remark; but I do not know any recorded case which is so singular as that I have given, for its acute character, and its fatal termination in sixty hours. The idea commonly entertained, that all such cases are exempt from danger, is here proved to be erroneous, and yet it is sanctioned by the highest authority. Heberden says, "Inter frequentissimos jecinoris morbos ille est qui fit interclusis bilis viis, et est *minimè periculosus*." It appears to me, therefore, that an exception to a rule which is acknowledged to obtain universally, may with advantage be submitted to the attention of the profession.

ON PERFORATION OF STRICTURES OF THE URETHRA.

With Cases.

By R. A. Stafford,

Surgeon to the Mary-le-bone Infirmary, and formerly House Surgeon to St. Bartholomew's Hospital.

[Continued from p. 130.]

Case IX.—*Case of Hæmorrhagic Stricture perforated.*

March, 1833.—Captain S., æt. 35, was invalided from India, in consequence of bad health, brought on from a stricture of the urethra. He was within six months of the expiration of his leave of absence when he called upon me, stating that he had tried, since his return to England, all the usual means for the cure of that disease, but he could never obtain any relief. His urine only flowed from him drop by drop; and he suffered such great irritation from the bladder, and a constant desire to make water, that he could get but little sleep at night. I found, on examination, a stricture just at the entrance into the membranous portion, which was of so vascular a character that the moment it was touched it burst out bleeding, and never ceased until several ounces were lost. Although the case appeared unfavourable on this account for the operation of perforation, yet from the previous experience I had had—that a clean incision into a stricture of this character caused the

hæmorrhage to cease rather than to increase it—I recommended the treatment. Leeches were applied once or twice to the perineum, when the perforation was attempted. After employing the urethral perforator three different times, the stricture was at length permeated, and at each time the blood lost was very trifling. The obstruction occupied nearly an inch. Bougies and steel dilators were now passed at first every, and afterwards every other day, until the strictured part was as large as the rest of the canal, and it would have been impossible to have told where the obstruction had existed. In this case the bladder had partially lost its power, and could not expel the whole of its contents, leaving eight ounces, and sometimes more, of urine behind. A catheter, therefore, was passed night and morning, to draw off the residue, and steel and other tonic medicines administered. From this treatment the bladder gradually gained its power; and in about three months the patient was quite well, and enabled to return to India.

Remarks.—There are two points of interest in this case:—1st, the hæmorrhagic character of the stricture; 2dly, the partial paralysis of the bladder, after the stricture was cured.

I have seen several cases of what may be called the *hæmorrhagic stricture;* and it appears to arise from the great vascularity of the substance of the obstruction itself; that is to say, the diseased structure, which forms the stricture, resembles in some measure that of the bleeding pile. I have had only in one instance an opportunity of examining the urethra in this description of stricture, and that was in a case which will hereafter be related. In this, the nipple-like projection, from whence the bleeding came, was of a highly vascular character. The fact of a clean incision into a stricture producing no bleeding, while the passing a simple bougie caused great hæmorrhage, may be accounted for by the cut mouths of the vessels contracting in the former, and in the latter by an erasion of their surface.

Partial paralysis of the bladder, after a stricture has existed for many years, is not an uncommon occurrence. At first the muscular coat becomes thickened, and the urine is expelled with greater force than natural; but when the stricture has become so close as only to allow the urine to pass through it drop by drop, the bladder frequently, and from time to time, becomes distended and stretched beyond its normal size. The patient likewise cannot, from the same cause, make a sufficient effort to expel the whole of its contents at one time: a certain quantity of urine, therefore, is always left in it, and consequently the bladder only contracts as far as the fluid contained in it will allow; and thus a habit is formed by which the muscular power is weakened, and partially lost.

Case X.—*Stricture of 17 years' standing divided by the Double-Lancetted Stilette.*

March, 1831.—Captain B. gives the following history of his case:—That in the summer of 1814 he first found a difficulty in passing his urine, but being then about to embark with his regiment, he had no opportunity of attending to his complaint. In the following year, however, a moderate-sized bougie was passed with difficulty into the bladder; he had not, however, sufficient resolution to continue their employment. In 1818 he contracted two or three gonorrhœas. The disease now became much worse: he had violent pain and spasm at the neck of the bladder; and he voided his urine so indifferently, that he was threatened with retention. He resorted to medical aid, and bougies were introduced for him, until a No. 8 could be passed freely into the bladder. In two years afterwards, and from no particular cause, the disease became more distressing than before; and he had frequent attacks of retention. He underwent another course of bougies, and found temporary relief. In 1829 spasms came on, from having taken too violent horse-exercise, and he lost the power of entirely emptying the bladder. He lingered on in a great state of suffering until 1831, when he retired into the country, and consulted an eminent surgeon there. The old treatment was resumed, but with so little effect that his surgeon advised him to have the stricture divided within the urethra, by the lancetted stilette. He immediately came to London, and placed himself under my care.

On examining this gentleman, I found a stricture situated exactly at the bulb, through which could be passed a very small bougie. As the common methods of treatment had failed, I recommended him to have it divided by the double lancetted stilette. The operation was accordingly performed with this instrument, and the strictured part was at once enlarged to the natural size of the canal. A No. 12 catheter was now introduced, and worn for five days afterwards. Steel dilators

were passed on the alternate days, for a month; at the expiration of which time he left my care, having lost all his distressing symptoms, and making water in a natural-sized stream.

Remarks.—The disease having so frequently returned, and caused such distressing symptoms, there is but little doubt that no other treatment would have answered. This case may be classed among a particular description of strictures, which might not improperly be termed *the elastic stricture;* that is to say, the structure of which the diseased part is composed is of that elastic nature that it somewhat resembles the consistence of Indian rubber, encircling the urethra. Hence, if a bougie be passed through a stricture of this description, and it is opened even to a considerable extent, yet it gradually closes up again. This disposition to contract may vary. It may occur immediately after the bougie is withdrawn, or in a few hours afterwards; a week may elapse, or a longer period, before the part wholly, or partially, closes up. This must depend upon the different degree of elasticity of the part. If it be highly elastic, then it immediately contracts; if not, this process gradually takes place. Examples of this description of stricture are of daily occurrence; and there is one case under my own observation at this present time, in the person of a surgeon, who has a stricture (the hard substance of which can be felt externally between the finger and thumb) between two and three inches from the orifice. He never can make water without first passing a probe through the stricture to open it. Immediately after the withdrawal of this instrument, the urine flows. If, however, he wishes to void his urine an hour afterwards, the stricture has closed up, and he is obliged to pass the probe again to open it. This might by some be considered as spasm. I have no doubt, however, in my own mind, that it is mechanical obstruction. I have found in these cases, if the part be divided within the canal, the indurated substance composing the stricture becomes absorbed, and the disease does not return.

Case XI.—*Stricture of 29 years' duration perforated.*

The following case, (for which I here beg to thank him,) was sent to me by Mr. Abernethy, Surgeon to the Union Dispensary, Farside, Cloyne.

Terence Howard, ætat 50, land-steward to a gentleman in this neighbourhood, has had stricture of the urethra for many years. He states that so long since as the year 1803 he received a hurt from the saddle, while learning to ride, in the 13th Light Dragoons; in consequence of which the penis was much swollen, but by means of the usual remedies, it was slowly subdued, leaving permanent, however, a small hard tumor in the part. He voided his urine with some difficulty ever since. He has had repeated attacks of gonorrhœa, and after each the difficulty increased until it amounted to almost a total inability. He has frequently undergone courses of the usual treatment, with bougies, &c. &c., while in the service, from which he was discharged in the year 1817. He was at that time able to pass a small bougie, but soon after found it impossible to introduce any instrument: the attempt has been made at different periods since by surgeons whom he consulted, but without success. He has not been able to void his urine in a stream for several years; it dribbles continually, and he is harassed with the most urgent desire of micturition, at very short intervals, (accompanied by severe pain in the glans penis) which must be complied with under any circumstance, although no more than a drop or two can be forced out on such occasions. He has frequently of late suffered under the total suspension of even this melancholy and trifling means of relief, being for several hours unable to pass a drop; and whilst writhing under the torture of a distended bladder, declares he could contemplate with satisfaction the awful consequences of its rupture, which he was convinced would quickly terminate his misery.

On examination I found a tumor the size of a large marble, hard as a piece of cartilage, devoid of tenderness, occupying the corpus spongiosum, about four inches from the orifice of the urethra, the course of which canal, anterior and posterior to the tumor for more than an inch and a half either way, was marked by an indurated line, feeling as if a piece of whipcord had been inserted. The frænum preputii is deficient; the meatus urinarius laid open, exhibiting the cicatrix of former ulceration. The patient says he can feel the urine advance from the bladder in a column, until it reaches the indurated part of the urethra, close behind the symphysis pubis, where it suddenly becomes arrested. A small silver probe cannot be passed farther than an inch from the meatus, the surface of which is tender and excoriated.

In this case, according to the patient's statement, dilatation had been tried and

proved ineffectual, and now appears quite impracticable. Caustic and all other escharoties are out of the question; on account of the length of the strictured portion, and its situation presents several objections to the operation for artificial urethra, by external incision; requiring the total division of the scrotum, to be followed probably by infiltration of urine; and as the penis should be nearly its whole length laid open, the complete union of the wound over the catheter would be an exceedingly questionable result. He had lately requested a surgeon to perform this latter operation on him, which he described as having witnessed in a military hospital, but it was prudently declined.

Thus cut off from a prospect of success in any plan of treatment I had seen practised, and having read some similar cases in which you had successfully employed the lancetted stilette, I at once determined to attempt piercing the stricture from within. Accordingly, I proceeded to operate as follows.

May 9th, 1832.—Finding that I could not introduce the canula beyond an inch from the meatus, I laid it aside; but before employing the single bladed stilette, I resolved to use every effort to introduce the fine wire which the canula carries, unsheathed; this, by long and steady perseverance, I succeeded in effecting. In the passage of the wire through the indurated canal it observed an exceedingly tortuous course, so that were it not for its flexibility, as well as its extreme tenuity, it could never have passed. When sufficiently introduced I applied the double lancetted stilette, and carried it steadily forward through the entire of the strictured portion, until, the lancet being withdrawn, the instrument passed onward without further obstruction. While cutting through the callous tumor considerable resistance was offered, resembling that which a half ossified sterno-costal cartilage might give the knife. A No. 8 catheter was then introduced, and passed freely into the bladder; on being withdrawn, a few drops of blood followed from the wound, and soon after a considerable quantity of dark-coloured ropy mucus was passed from the bladder. The patient did not complain of pain during the operation, but declares he scarcely felt it being done. 10th, next day—The patient has passed urine in a tolerable stream several times since the operation; and does not feel the desire so often as before it. Complains of some smarting while passing urine, and slight tenderness in the penis: he was directed ℥j Mag. Sulph. and barley water, six leeches to the part, and fomentations. 11th.—The inflamation of the part much reduced: the patient has been out about his usual business: can retain his urine for two or three hours, and passes it without difficulty or pain in a full stream.

A bougie was passed every day for a fortnight, increasing the size to a No. 10. At first this caused more pain than the original operation, and for the first few days was followed by a drop or two of blood each time. The smarting in making water soon disappeared: the tenderness on introducing the bougie continued for some time, chiefly felt in the part anterior to the tumor. The bougie was smeared with ung. hydr. nitr. oxyd.: and after the first fortnight was introduced only every second or third day for a few weeks more. The patient continued to introduce the bougie once or twice a week for two or three months; long after all uneasiness or difficulty in introducing the instrument had ceased. He was never an hour confined in consequence of the operation, after the first day: and has since enjoyed better health than, he says, he ever had since a boy.

Feb. 5th, 1833.—I have this day examined Howard, and find that the tumor above described has nearly disappeared, being now scarcely as large as a pea: the indurated line is quite gone. All irritability of the bladder vanished immediately after the operation; he is able to retain his urine for the normal period, and void it with force in a full stream, without the least difficulty. A large sized bougie may be passed into the bladder without the slightest pain or difficulty.

Remark.—The history of this case is so ably and so fully detailed, as to render any observation of mine unnecessary.

[To be continued.]

INTERNAL HERNIA—STRICTURE OF THE LARGE INTESTINES.

To the Editor of the Medical Gazette.

Sir,

The obscurity accompanying diseases of the abdomen, which pass too frequently under the indefinite term "constipation of the bowels," induces me to offer, briefly, for publication, the particulars of the two following cases, as they appeared immediately before death and on dissection.

L. D. was attacked with pain in the abdomen, and had five loose stools during the first day of her illness: subsequently she had constant nausea, with vomiting, at intervals, of feculent matter, and a confined state of the bowels. The

stronger aperients were given. Calomel, colocynth, sulphate of magnesia, elaterium, croton oil, &c. and repeated enemas, were administered carefully, but ineffectually. At the end of ten days she died. On dissection, it was discovered that from two to three feet of intestines, including the lower part of the ileum, cœcum, and commencement of the colon, were encircled by an adventitious membranous band, about two inches in length, passing from the mesentery to the mesocolon, and forming a stricture on the intestines, which were thereby thrown into a high state of inflammation, approaching to mortification, if not actually in that state, for their contents had escaped into the abdominal cavity either before or during the incision of the parietes.

Mrs. M., two years ago, had inflammation of the peritoneum immediately after childbirth, requiring active treatment for its subdual. Since then her bowels have been troublesome from costiveness, and have three or four times with difficulty been relieved at all. In her last illness the more powerful aperients were used, which produced secretion from the bowels, but without affording the customary relief. Two or three days previous to her decease she had frequent nausea, with a tympanitic state of the abdomen. On examination after death, a stricture was found of the descending colon, extending about an inch, just where it takes the name of sigmoid flexure; and the intestinal canal was so nearly obliterated at the strictured part, that the small end of a blowpipe, such as is commonly used in anatomical pursuits, could hardly be made to pass through the remaining opening. In external appearance, the intestine is considerably less at the strictured part than elsewhere: its coats are much thickened, and of cartilaginous hardness. It is worthy of remark, that but forty-eight hours before death, some firm feculent matter was discharged from the bowels, bearing the shape of the intestine below the stricture; so that it must have passed through the diseased part in a fluid state; and the more aqueous portion had been removed afterwards by the absorbents of the rectum.

Your obedient servant,

J. F. Marson,
Mem. Roy. Coll. Surgeons.

10, Montague-Street,
Russell-Square.

COOL REMARKS ON THE BURNING OF ONE'S OWN FINGERS.

To the Editor of the Medical Gazette.

Sir,

Did you ever burn your fingers? Do not misunderstand me: I know an editor never burns his fingers in a metaphorical sense; but without a figure—to repeat my question—did you ever burn your fingers? *I* did this evening; and if you like to publish my case, here it is. The burn occurred through taking up, between the index and thumb, a china cup in which a wick had been for several minutes alight in oil.

Symptoms.

Local.—Sense of burning heat; sudden appearance of two flat blisters, of the size of elongated silver sixpences.

General.—Through the sudden rush of the animal spirits to the injured extremity, the mental equilibrium disturbed; influence of the organ of veneration temporarily suspended; strong tendency to break the third commandment—but repressed.

Treatment.

General.—The practice which allayed the general irritation consisted in throwing the china cup with violence under the grate. The relief was instantaneous. But I hardly venture to take much credit for this idea in therapeutics. The practice, I am aware, is not new; and I think, after all, the action should be viewed rather as a symptom and measure of the injury sustained, than as part of a curative intention; or, perhaps, it is something between both—a sudden effort towards relief suggested by the *vis medicatrix*, and proceeding, according to the language of different theorists, either from the superadded nerves of Bell, or from an instinctive impulse along the single system of nerves of Mayo, or from the reflex functions of Marshall Hall.

Local.—1. Index and thumb inserted into a tumbler of cold water; pain gone in five seconds. Fingers withdrawn; pain returns.

2. Pieces of ice held between index and thumb; pain gone. Treatment discontinued after a few minutes; pain returns. It was evident there would be

no return of pain, if the application of cold were continued.

3. Index and thumb inserted into, and held in a cupful of flour, loosely piled round the digits; pain unmitigated.

4. Index and thumb re-introduced into ditto; the flour now *pressed down* into the cup; the *sensation of burning lessened of half its pungency;* this effect seemingly permanent while the treatment is continued. After five minutes fingers withdrawn; pain returns.

5. Index and thumb introduced into a cup of cold salad oil; relief the same as in cold water. Upon taking out the fingers, but not wiping them, thus leaving them incased in a sheath of oil, so as to exclude the atmospheric air,—pain returns.

6. A twisted silk handkerchief wound twice tightly round the top of penultimate phalanx of index and thumb; pain in a few seconds perfectly numbed and extinguished. On removing the ligatures, pain returns. Ligatures re-applied, and continued from eleven till one o'clock, when the pain had entirely gone. Every ten minutes, during this period, the ligatures were removed, when the pain returned; and as instantaneously subsided on their re-application. During the last hour the pain was evidently on the decline, being less severe at each removal of the handkerchief.—*Plura otiosus. Vale.*

MUTIUS C. SCÆVOLA.

Quinto Calendas Maii.

EFFECTS OF MOUNTAIN ELEVATIONS UPON THE HUMAN BODY.

BY P. CUNNINGHAM,
Surgeon, R.N.

(Communicated by A. COPLAND HUTCHISON, F.R.S.)

THE singular contrast of high mountains in the northern hemisphere producing reverse effects upon the travellers ascending them, to those in the southern hemisphere, never having been, to my knowledge, animadverted on, a short parallel between the two may not be unacceptable. At high elevations in the northern hemisphere (such as the top of Mont Blanc), there is a strong determination of blood to the head, indicated by swelling and lividity of the face and lips, sleepiness, and bleedings from the mouth, nose, eyes, and ears; while at similar elevations in the southern hemisphere there is a strong determination of blood in a contrary direction, indicated by paleness and shrinking of the features, sleeplessness, giddiness, faintness, and vomiting;—apoplectic symptoms thus characterizing the first, and all the usual accompaniments of fainting the second. The treatment affording relief in each is also equally opposite (with the exception of the horizontal posture, which is useful in both); the stimulant drinks, and the stimulant applications to the external parts, such as the mouth, nose, and ears, so beneficial in the southern affection, being hurtful in the northern one. The apoplectic symptoms experienced upon Mont Blanc and other high northern mountains, have been hitherto ascribed to the great rarefaction of the air, whereby the soft parts of the human body are permitted to be expanded by the reduction of the atmospheric pressure upon them; but as no such effects, but, on the contrary, effects of an opposite nature, are produced by similar elevations in the Andes of Southern America, we must consequently look to some other cause than atmospheric rarefaction to account for them *.

In South America there are two distinct species of affections noted as liable to attack those unaccustomed to high ascents: the first being the usual difficulty of breathing arising from atmospheric rarefaction (common to all high elevations throughout the world), and the second, the more alarming one, previously spoken of, which proved so fatal to Bolivar's army at the crossing of the Andes, as recorded in the interesting "Memoirs of General Miller." The first is known in Peru by the name of *punya*, and the second by that of *sorochè*, in consequence of a belief that a *sirocco*, or foul wind, is the cause of it. According, however, to the opinion of Mr. B. Scott, the intelligent English engineer superintending the great aqueduct at Tacna (Peru), electricity is its exciting cause—an opinion which he has arrived at in consequence of observing particular parts of the Cordillera to be more subject to it than others (and these not always the highest), at

* It is said, however, that when Humboldt ascended Chimborazo, to the height of 19,798 feet, blood issued from his eyes, lips, and gums.—ED. GAZ.

which parts the electrometer was always greatly deflected.

The above reverse effects in the respective hemispheres may, I conceive, be readily accounted for by the curious fact of the electric polarities of bodies being the *reverse* in the southern hemisphere to what they are in the northern; a fact for the observation of which, I believe, we are indebted to Lieut. Peter Lecount, R.N., when employed at the Island of Ascension, during the period of Bonaparte's detention at St. Helena. Thus, electricity being found to occupy the *upper* portion of bodies in the northern hemisphere, and the *lower* portion in the southern, will consequently tend to propel the blood towards the head in the first, and towards the feet in the second; thereby giving rise to an apoplectic tendency in the one, and to a tendency to fainting in the other, the symptoms in both being necessarily mitigated by the horizontal posture, whereby the electricity is more equably distributed throughout the body, in consequence of its occupying the latter longitudinally instead of transversely. I am further disposed to this opinion, from finding that travellers in the Andes seldom experience any unpleasant feeling as long as they remain on horseback, where they are in a great measure insulated from the electric influence of the earth, by the non-conducting sheepskin paddings placed under the saddles in Peru.

In July, 1833, I accompanied Captain Hope, of H. M. S. Tyne, Mr. Young, a merchant of Tacna, and Mr. Scott, engineer, on an interesting excursion to the aqueduct excavating on the plain of Chinchillar, nearly 14,000 feet above the level of the sea, and no unpleasant symptoms were experienced by any of us until we had crossed the highest ridge of the Cordillera, and alighted to walk, when all except Mr. Scott were instantly attacked with the *sorochè*. The moment that my feet touched the ground, I was seized with giddiness, dimness of sight, and nausea, followed by such an universal tremor and debility that I could barely support myself until I remounted my mule, when, by lowering of my head, and the use of a smelling-bottle, I was enabled to continue my journey to our dining place, two miles farther on. Animals are as subject to it as man, the first symptoms with the mules being a violent trembling, hurried breathing, and disposition to stand still, which, if not permitted, they tumble down, seldom to rise again. The moment the rider perceives the warning, he slackens the reins, to enable the animal to hang down its head until it recovers; of the beneficial effects of which a mule is so well aware, that when the complaint came upon mine in ascending a declivity, it invariably turned round, with the head down hill, in order to make it more dependent.

The treatment found most useful in fainting, is found also the most useful in *sorochè*—viz. the horizontal posture, brandy and water, cold water dashed upon the face, and smelling-salts to the nose: rubbing of the nose, mouth, and ears, with garlic, being capable of averting its attacks in both man and beast. The Peruvians compare the *sorochè* to sea-sickness, to which it has certainly a very close resemblance, as well as to the state of body resulting from the sudden abstraction of a large quantity of blood; the faintness, sinking of pulse, prostration of strength, irritability of stomach, pallor of feature, and cold perspirations, being similar in all. How opposite to the above are the symptoms consequent on the ascent of Mont Blanc, and other northern mountains—a difference so marked, as to point to some distinct cause existing in each hemisphere, through which such diametrically opposite effects are produced—a cause which, I doubt not, gives rise also to those distinctive features characterizing so strongly the animals of the southern hemisphere from those of the northern.

ON THE PROBABLE DURATION OF LIFE AMONG MEDICAL PRACTITIONERS.

By Professor Casper, of Berlin.

There is no part of medical statistics more painfully interesting, and at the same time important, than that which relates to the probabilities of the duration of life. Inquiry on the subject, it is true, scarcely tends to lift the thick veil which conceals the limits of individual life, and has little power to satisfy the pardonable curiosity which we all have, to know how long we may have

to live: yet, when applied to masses, it enables us to arrive at results of great consequence, whenever provision is to be made for the duration of life in general, or for the probable duration of it at certain epochs.

For upwards of ten years I have been engaged, during my leisure moments, in researches of this kind—the difficulties of which can only be compensated by the value attaching to each result. I have considered most of the circumstances which tend to modify the duration of human life, and those more particularly which are connected with the practice of the different professions. Much has been attempted within the last two hundred years on this subject by mathematical and statistical inquirers, yet much remains to be done; for, in fact, with the exception of Deparcieux's work on the mean duration of life among monastic persons of both sexes, we cannot be satisfied with any thing that has hitherto been accomplished.

Although I should not feel justified in laying at present the whole of my work before the public, there is, notwithstanding, a portion of it with the correctness of which I am so well satisfied as not to hesitate in publishing it: the portion or fragment to which I allude relates to the probabilities of the duration of life among medical men.

Establishments having a close connexion with civilization, such as life assurance companies, those for annuities to survivors, for mutual assurance, &c. have become numerous of late years; and the institution, set on foot by the illustrious Hufeland, for the benefit of members of the profession, may serve as a model of the kind. But none of these establishments, as the experienced can testify, and as lamentable events have too clearly shewn, can possibly subsist for any lengthened period, unless founded on very exact tables of mortality. It was with this impression that the following tabular view was formed.

In order to obtain the most satisfactory points of comparison, I have noted down 624 cases of death, occurring among physicians and surgeons, mostly all Germans,—and excepting anatomists, veterinarians, naturalists, and medical men, who are solely devoted to literature *. I have taken the age of twenty-three as my *point de départ*, both for the medical as well as the other professions (a comparison of which it is my intention hereafter to make public); but if others should choose twenty-four or twenty-five as the age from which we should begin to calculate, it in nowise interferes with the exactitude of the results.

My chief authorities in ascertaining the dates of the births and deaths of those on whom I reckoned were, Ersch, in his Manual of Literature (Leipsic, 1822), and Voigt, in his Necrology (1833), whose valuable collection is known to have been principally formed from manuscripts communicated by the friends and relatives of the deceased.

In column A. of the following table we have the ages at which our medical men died: B. contains the number of deaths at those ages.

Now we suppose the 624 individuals on whose deaths the table is constructed to have been contemporaries, or that they were all aged 23 at the same time. On this supposition the cypher 624 is placed in column C, opposite the age of 23; and as there are two deaths for that age, 622 is the cypher which must stand next in column C, corresponding with the age of 24 in column A. Thus the column C is formed throughout, until the whole of the 624 are deceased, which is found to be at the 92d year. Column D exhibits the number of years which may be allotted to each period—that is, the probable duration of life according to Halley's method *. I have preferred the form which Halley follows to that of Deparcieux, as being less complicated, at the same time that it leads to the same results. By Halley's method, we find the probable duration of life very readily: we look into the column C to ascertain at what age only half those living at any given period survive. Thus, if of 122 medical men who are 72 years of age, we find that the half are dead at 77, it would seem that the chances of the 122 reaching that age, and of not reaching it, are equal; and the difference of the ages (77—72)—nearly five years—is the length of time which those of 72 may hope to live †. Nothing positive, of

* I have since calculated the mean duration of the lives of seventy-six other medical men—thus making up the entire number 700; but I have not thought it necessary to enlarge my table by the addition of the results, which only serve to confirm those already obtained.

* Philosophical Transactions for 1691 and 1693.

† In general terms: if m be the number of persons who have attained the age a, and that at the

course, can be predicated from the table respecting the probable duration of the life of any individual; but the general conclusions, as experience has proved, are nevertheless *extremely certain.*

TABLE OF THE MORTALITY OF MEDICAL MEN.

A. Age.	B. Deaths.	C. Living.	D. Probable Life.	A. Age.	B. Deaths.	C. Living.	D. Probable Life.
Years.				Years.			
23	2	624	35, 5	58	10	317	11, 0
24	1	622	34, 4	59	17	307	10, 6
25	4	621	35, 4	60	12	290	10, 3
26	3	617	33, 0	61	15	278	9, 7
27	7	614	32, 0	62	14	263	9, 0
28	5	607	31, 4	63	19	249	8, 8
29	5	602	30, 1	64	20	230	8, 5
30	5	597	29, 8	65	11	210	8, 0
31	11	592	28, 6	66	18	199	7, 5
32	8	581	28, 0	67	6	181	7, 2
33	11	573	27, 6	68	16	175	6, 5
34	11	562	26, 8	69	9	159	6, 0
35	8	551	26, 0	70	17	150	5, 5
36	7	543	25, 5	71	11	133	5, 4
37	8	536	24, 7	72	15	122	5, 0
38	14	528	24, 0	73	14	107	5, 0
39	8	514	23, 5	74	13	93	4, 7
40	9	506	22, 7	75	10	80	4, 6
41	11	497	22, 0	76	9	70	4, 4
42	6	486	21, 3	77	8	61	3, 9
43	8	480	20, 3	78	10	53	3, 3
44	8	472	19, 7	79	4	43	3, 0
45	11	464	19, 0	80	11	39	3, 0
46	4	453	18, 2	81	6	28	4, 0
47	14	449	17, 3	82	3	22	4, 0
48	11	435	16, 7	83	3	19	4, 0
49	12	424	16, 0	84	2	16	4, 0
50	13	412	15, 4	85	3	14	4, 0
51	8	399	15, 0	86	2	11	3, 5
52	11	391	14, 2	87	—	9	2, 6
53	10	380	13, 5	88	2	9	2, 0
54	18	370	12, 8	89	4	7	1, 5
55	14	352	12, 6	90	1	3	0, 5
56	13	338	12, 4	91	2	2	0, 0
57	8	325	11, 9	92	—	0	——

From this table we gather the sad certainty of the short career of those who practise the medical art. Supposing the ordinary duration of human life to be 70 years, we see that scarcely a fourth part of our medical brethren attain that age; and hardly one in 15 reaches the age of 80. Or again: if in youth the resolution be taken to devote oneself actively to the laborious pursuits of medical science and practice, in order to enjoy in old age the fruits of a life of exertion, it is a melancholy fact that one half the number of practitioners is cut off before that period: so that the remarks of Jean Paul are peculiarly applicable to our case. "The life of man," says this author, "has often been compared to that of travellers or pilgrims: it seems to me rather to resemble that of an innkeeper, who is ever busy about his guests, receiving them, and seeing them depart; and who, on the occurrence of every interval of unprofitable repose, longs for a fresh bustle, desirous of more work when at rest, and of rest when at work; al-

age n the number is found to be reduced to $\frac{1}{2}$ m, n will express the probability of vital duration, and $n-a$ the number of years on which the individuals m may calculate.

ways hoping for the time to come when quiet and ease will suffer him to enjoy his arm-chair in tranquillity; but, in general, before that time arrives, he is gathered to his eternal rest."

There are a few points of comparison between the mortality in the medical and that in other professions, which I cannot willingly omit to notice. Divines *(les théologiens)* among all the professions, are those who seem to stand the best chance of long life; and if *they* die off more slowly, or enjoy more longevity than the other, it is just the reverse with medical practitioners. The following summary will show the difference:—

Ages.	Deaths.	
	Physicians.	Divines.
23 to 32	82	43
33 — 42	149	58
43 — 52	160	64
53 — 62	210	182
63 — 72	228	328
73 — 82	141	255
83 — 92	30	70
	1000	1000

And the following list may serve to show how much shorter the probable length of life is among medical men than among others. Taking 100 individuals in each calling, the number who attained the age of 70 have been, among

Divines	42
Agriculturists and foresters	40
Employés in high offices	35
Mercantile persons and traders	35
Military men	32
Employés in lower departments	32
Advocates	29
Artists	28
Teachers, Professors	27
Medical practitioners	24

But what are the causes to which we must attribute this low place in the scale of vitality held by our profession? It would be needless to enumerate them to those who are familiar with the extent of labour which the practice of medicine entails. There is, perhaps, no profession which requires more moral and physical exertion than ours, which allows less of repose, or of that tranquillity which is so conducive both to internal and external life. There is none which exposes the members of it to such bodily fatigue, such mischief from bad weather—such disturbance of the night's rest,—so much watching, irregularity of meals, disorders of the digestive organs, and mental affections of all kinds: in a word, to such dangerous influences, perpetually recurring, and all tending to sap the vital powers. If we add to this, that there are far more practitioners cut off by contagion than are commonly supposed, we shall easily see the groundlessness of the satirical observation, that *sybaritism* is the rock on which medical men are wrecked. It may be mentioned, that in the table given above, the great majority of those who are the subject of it were country practitioners, whom to accuse of luxurious living were too bitter a satire,—one, indeed, wholly undeserved by a profession, which has such strong claims to public gratitude, since those who practise it abridge at least, if they do not absolutely sacrifice, their existence, in endeavouring to prolong that of others*.

MEDICAL STATISTICS.

COMPARATIVE MORTALITY OF ENGLAND.

To the Editor of the Medical Gazette.

SIR,

I APPEAL with confidence to your love of justice, and to the respect you profess towards the general practitioner, for the insertion of the following extracts from the last Parliamentary Reports on the population of this country, as they will tend materially to qualify the objections you have made to the results deduced from them in your editorial remarks of last Saturday. The compiler of these reports (Mr. Rickman) says, that "the increased duration of life in England is a phenomenon which has attracted more notice abroad than at home, but seems now to be generally admitted by those who have studied the subject with the best means of arriving at a well-founded conviction;" and he refers to Sir G. Blane's Reflections on the present Crisis, 1831. Again, in reference

* Berliner Medicinische Zeitung, and Annales d'Hygiene.

to the defective registers of this country, as compared with the more perfect reports of the Continent, he says, that "absolute exemption from defective or erroneous entry in these registers must not be expected; but it may fairly be doubted whether such avowal of imperfection is not preferable to the official appearance of perfection which is offered to the inquirer in those nations where by positive law and severe penalties the acknowledgment of imperfection in civil registers (as they are called) is effectually prohibited, and where no allowance can be made by the calculator for negligence, which yet may have sometimes existed. In the first case, as in England, the degree of omission and defect is open to investigation and discussion, and reasonable approximation is attainable." But we have other testimony to the superior value of life in this country, upon which we may unhesitatingly rely. Take, for instance, the mortality among the insurers of the Equitable Society (as shewn in their published tables), for the twenty years, from 1800 to 1820 (individuals, be it recollected, of all ages, from adolescence to extreme senility), the ratio is one *only* in eighty-one and a half, considerably less than that of any country in the known world; and contrast with it the mortality of the French soldiery between twenty and thirty years of age—men under discipline, in the prime of life, in a time of peace, as recently published by M. de Chateauneuf, No. 20 Annales d'Hygiène Publique: it will be found to be nearly double, being one in 44 and a fraction. Without multiplying indirect proofs, it will be sufficient to refer to the elaborate report of Moreau de Jonnès, read at the Académie des Sciences of Paris only last year, to shew that the best-informed foreigners (making such allowance as they consider necessary for supposed inaccuracies), still place us at the top of the scale as to the value of life, his ratio of mortality being, for England, one in fifty-eight, and for France, one in thirty-nine, or one-third more—a difference so great as to put the medical practitioners of this country beyond the fear of competition (on this ground) with the most favoured of their Continental brethren.

I am, sir,
Your obedient servant,
A GENERAL PRACTITIONER.

April 24, 1834.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Annales d'Hygiène publique, et de Médecine légale. Avril 1834.

THE leading paper in the present number is one by MM. Orfila, and Parent Duchatelet—an elaborate report made to the French authorities—on the Effluvia proceeding from Starch-works, with a view to determine how far the emanations from such sources resemble those from marshy localities. M. Villermé also supplies an article on the unhealthy influence of marshes. After which we have a curious contribution from M. Roussel de Vauzème, *On the Salutary Effects of the Potato on the Crews engaged in the Whale-fishery.* This gentleman gives an account of a voyage he made in the Massachusset, a whale-fisher from Havre to the South Seas. The crew were well supplied with a variety of wholesome provisions; they had coffee for the morning, and tea for the evening beverage; they had plenty of biscuit, pudding, beef, pork, rice, peas, maize, and potatoes; the utmost cleanliness was observed about the ship, and the persons of the seamen; exercise was constantly taken, and cheerful recreation was enjoyed. Yet, on the homeward voyage, several of the men were attacked with scurvy, in consequence of the stock of potatoes having failed. In a subsequent voyage the malady was warded off by an abundant supply of potatoes; and when symptoms of its approach were apprehended, a tub of raw potatoes was left at the foot of the mast, daily, to be consumed at discretion by the sailors. The whale-fishers have long been thoroughly impressed with the perfect efficacy of the potato as a preservative against, and a remedy for, scurvy; and the men are even in the habit of carrying out a little private stock of the trusty antiscorbutic, to provide against the day of trouble.

M. Duvergie has published in this number his elaborate paper *On the Means of Detecting Corrosive Sublimate in Complex Mixtures;* and M. Orfila, in a short letter which follows, has borne testimony to its merits, while he puts in a short *reclamation* for himself. We propose giving an analysis of

the paper in a future number. Of Professor Casper's valuable essay, *On the Probable Duration of Life among Medical Men*, which has been judiciously extracted from the Medical Gazette of Berlin, we have given a version in a preceding page.

MEDICAL GAZETTE.

Saturday, May 10, 1834.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

FACTS CONNECTED WITH THE SALUBRITY OF THE METROPOLIS.

A CORRESPONDENT, whose letter we give in another page, has recalled our attention to the reputed salubrity of this town; he goes, indeed, to a greater extent than we did, and quotes some authorities tending to establish the degree of salubrity which he would attribute to the country at large. From the tone in which he writes, we must allow him to be not a little zealous in endeavouring to support his opinion; and it would seem, by his appealing to our *justice*, that he fancies we may be reluctant in admitting the facts which he adduces. We, however, in the recent observations which we made on the system of parochial registration practised in this country, went no farther than the metropolis; our remarks were purely founded on the Bills of Mortality of London and Westminster for the past year, which had just been laid before the public. We said nothing about the reputed mortality of the country parts—though we may do so another time—and much less did we pretend to draw conclusions as to the general mortality of England. We attempted to show that the mortality of London and Westminster is considerably underrated by those who pretend to fix it at 1 in 42, or even 40; and that the erroneous impression to that effect is founded on the vaguest evidence—the optional returns of the parish clerks of a certain number of districts—returns which are not only imperfect, from the negligent manner of keeping them, but extremely defective, and almost unmanageable, inasmuch as they contain only the entries of buried people belonging to the established church, or whose friends are willing to have them buried as such; thus excluding the large proportion of Dissenters who are interred in cemeteries belonging to the different sects, or, at all events, out of the jurisdiction of the worshipful company of parish clerks. Yet we freely stated our conviction of the possibility of applying such corrections to those vague returns, and of procuring such approximations, as should render them not unsatisfactory, or perhaps procure results not very far from the truth. Some examples of these approximations we gave; and it must be confessed they were not exactly in accordance with those commonly, and, as we conceive, too easily received.

Mr. Rickman, the able compiler of the Population Returns, who is so constantly referred to by our statistical inquirers, gives his testimony strongly in favour of a low rate of mortality in this country. But it ought to be observed, that he depends altogether for the grounds of his opinion, on the authority of others. He acknowledges that the *absolute* mortality cannot be stated; and that the utmost we can arrive at, is a *comparative* estimate of its amount. He allows that there is a deficiency in the returns of the metropolis Bills, amounting to 8000 burials per annum. *We* say that there is that number at least; for in 1811 the deficiency was calculated, in official documents, at 7000,—whence we thought it but reasonable to

allow one-third of the number returned, as that to be put down for the unregistered; and this is all we demand—to compensate for the needful allowances to be made for Dissenters (who are certainly not diminished in their numbers since 1811)—for Jews, Quakers, &c., and for parishes irregular, or altogether defaulting *. Now adding only one-third (certainly much less than we might reasonably add) to the registered returns, we say that, even with this most moderate correction, the rate 1 in 42, or 40, *cannot be made out*. We gave a few examples, founded on the Bills of the last few years; but these, it may be said, are too isolated, and too much, perhaps, of the nature of exceptions—on account of the presence of cholera, influenza, &c.—to be taken as fair specimens. Let us then take an average of the yearly mortality of the ten years, from 1821 to 1830, inclusive; and we find that, allowing for the increasing population, the supposed equal increments of the dissenting part of the community, and taking only one-third of that average to cover all deficiencies, the rate is still above 1 in 40. With the most straitened allowance, it cannot be made to exceed one in 39·28,—while, if Mr. Rickman's 8000 were adopted, instead of the third, it would give one in 37·8. Nay more, an average of the last five years gives one in 35·5: and we have not the smallest doubt but that, if *all* the due allowances were equitably made, the *bonâ fide* mortality of the metropolis must be rated at least as high as that ratio.

We have taken some pains to satisfy ourselves on this point;—we should be very happy to be able to corroborate the prevalent notion; but we can only say that we are reluctantly obliged to leave that task to somebody else. Let others, especially those who are so confident about the 1 in 42, or even 40, bring forward their *proofs*: let them not echo one another, but show the grounds of what they assert: let them point to the justification of their assertions; and once more, we say, let them show us their *proofs*.

The credulity of most persons on this subject can be easily accounted for. The registered returns are very small, compared with the vast population of the metropolis; and few recollect that this vast population embraces five large parishes,—those of St. Pancras, Mary-le-bone, Hampstead, Kensington, and Chelsea,—which have nothing to do with the returns within the general Bills. Then, there are parishes even within the Bills,—such as St. George's, Hanover-Square,—which for years have given no return of burials. But the great source of fallacy, as we conceive, and have already said, is,—that of the great body of Dissenters, Jews, and Quakers, who bury their dead apart from the cemeteries of the Established Church, we have no returns. Now, in order to approach any thing like correctness in forming an opinion of the real mortality of the metropolis, allowances must be made on these several heads, and few are willing to take that trouble.

We have quoted the results of the Chelsea returns, (which, for a few years, have been drawn up independently, and with great care); they appear to be, beyond all comparison, more correct than the General Bills; yet the mortality for that large section of the metropolis,—which certainly, one would think, might vie in salubrity with any part of the City,—would seem to be above one in 30.

But, supposing we fix the mortality of the metropolis at 1 in 35, instead of

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* The registered burials in 1811 were only 17,043: they were, however, 19,893 in 1810; and the average of the three years 1810, 1811, and 1812, gives 18,410: so that a deficiency of 7000 at that date is much nearer to two-fifths than one-third.

1 in 40, still we surely have great reason to be satisfied, and may congratulate ourselves as living in as healthy a capital as any in the world. As to foreign statistical authorities admitting into their comprehensive tables an excessively low rate of mortality for this town, and for England generally, we can only attribute it to their too great reliance upon the correctness of our returns: and if M. Moreau de Jonnès is satisfied with the results, which after all he can only gather from documents procured among ourselves, we must allow somewhat for his faith in our steadiness, and not a little, perhaps, for the multifarious nature of the laborious task with which he is engaged.

Mr. Rickman and others may, as our correspondent informs us they do, depreciate the official and compulsory registration adopted in France, in favour of the circuitous or loosely approximating system, that must, under present circumstances, be had recourse to here. This seems to be merely a matter of taste; but at the same time we cannot help thinking it rather too whimsical to have weight with many reasonable persons. It strongly reminds us of an eccentric friend of ours, who delights to have his watch set a couple of hours behind the time, that he may have *the pleasure of calculating* what o'clock it is.

Finally, we would just advert to one more circumstance. The general fact of the low rate of the mortality of London being admitted, we observe that there are those who, notwithstanding the complicated considerations to which such a fact must give rise, would endeavour to account for it in the simplest way possible; and, among others, our correspondent seems willing to attribute no small credit to the profession for leaving or keeping (we will not say which) the mortality so low. We should be extremely loath to refuse a civic crown to our professional brethren in so noble a cause, if we thought there was really any good claim to set up for it; but we fear it would be hard to convince the community generally that the salubrity of London depends mainly on our medical skill. There are certain data, at least, requisite before we can arrive satisfactorily at such a conclusion. In order to prove that the merits of British practitioners should on such grounds be inferred to be superior or equal to those of their Continental brethren, it would be necessary to shew that there is an equal proportion of disease to deal with here as there is in Paris or Berlin—a fact which, if it were made out, would at once destroy all the pretensions of this capital to be considered as endowed with superior salubrity. It has, we must confess, always appeared to us, that the small rate of mortality which is fortunately the lot of this metropolis, can only afford a *negative* proof of the skill of its medical practitioners: it shews, beyond a doubt, that not so many "die of the doctor" here as in most other places; but until we have much more exact data to go upon than we have at present—until, in fact, we enjoy a better system of parochial registration, and of obtaining public and official returns from the hospitals—it seems to us to be perfectly idle to indulge in speculations, the very broaching of which can only be, perhaps, laid to the account of an inordinate, if not a most unjustifiable, vanity.

CHOLERA IN PARIS.

Some of the French medical journals state that a certain number of cases of cholera have been met with in Paris during the last month; it does not appear, however, that they have been of great

severity; indeed, from those which are given in detail, we should be disposed to say that the disease was no more than is usually met with when the weather becomes warm, and such as would not have attracted the slightest notice anterior to the irruption of the more formidable disease from the East.

COLLEGE OF PHYSICIANS.

At the meeting on Monday, the 28th ult. a paper was read by Dr. Hawkins, from the pen of Dr. Johnstone, of Birmingham, on the subject

Of the Aliment and Organs subservient to Alimentation in Fœtal Life.

The author observed, that Democritus and Epicurus are said to assert that the fœtus in utero is nourished by the mouth—the Stoics, by the umbilical vessels; but Alcmæon, a disciple of Pythagoras, maintained that this took place by its whole skin, which drinks up, like a sponge, the necessary aliment: and this, Dr. Johnstone believes to be the fact.

Dr. William Harvey gave his *Exercitationes de Generatione* to the world in his seventy-third year. This work had exercised his genius from an early period of his professional studies, and its amplitude and exactness, though not perhaps its utility, exceed those anatomical exercises "de motu cordis," which have made his name so illustrious.

Without adverting to the thousand subjects contained in these Exercitationes, which underwent the meditation and observation of this great man for near half a century, the author of the paper alluded only to those which relate to the gradual accretion of parts in the ovum, and the alimentation of the fœtus.

It would be unjust, he observed, not to remark that the observations which Harvey has made on eggs, from the first day of incubation to the last, confirm the accuracy of most of the opinions of Aristotle, and that more recent experimentalists bear testimony to the veracity of both.

The discovery of the microscope has since presented a larger power and wider latitude of observation, as to priority in the formation of parts. Harvey, with his own unaided vision, saw the punctum saliens as the primary formation. Malpighi, with his glasses, detected the outline of the head and trunk some hours before the heart; thus giving a shew of proof to the theory of the French anatomist, Serres, that growth goes from the circumference to the centre.

On the subject of nutrition, Harvey is decidedly of opinion that the albumen is the substance which nourishes the chicken in ovo; but as this expulsion may be delayed after the consumption of the whole albumen, the yolk is then applied to the same purpose, being attached to the abdomen of the young animal, and being extruded with it from the shell. In viviparous animals, according to Harvey, the liquor amnii performs the same office.

In the oviparous arrangement it is clear that the mother is not requisite after the expulsion of the egg. Temperature alone, within certain limits, is requisite to produce the chick. In the perfect egg, the two coloured substances, it is well known, form its internal bulk, each contained in its own membrane. The other parts are of small size, the cicatricula placed on the yellow, midway between the obtuse and acute ends; the chalazæ attached to the membrane of the yolk at either pole, but stretching into the white; and the air-bubble at the obtuse end, exterior to the membrane of the white. The albumen and yolk have ascertained natural and chemical properties; but little is known of the structure of the chalazæ, except their knotty contexture, and their firm adhesion to the membrane of the yolk. The chick is never formed in the ovary of the hen: the egg appears and remains the same compound even for weeks, unless incubated. After the incubation of twenty-four hours, the yolk ascends to the air bubble, the chalazæ change their position, and from this time minute but sensible changes, in colour and consistency, take place, until the fourth day: among others, the cicatricula, enlarging itself like the pupil of the eye, and the punctum saliens appearing with a little purple thread stretching to the edge of the albumen. Here visible life begins, and from this beginning the lineaments of

the chick gradually open to the naked eye.

On the fourteenth day, the albumen being nearly consumed, the chick lies incumbent on the yolk: the chalazæ, which extended into the albumen, begins to shrivel up, till it is converted into a sort of earthy matter, on the twentieth or twenty-second day. Then the chick is expelled, with the yolk attached to its belly, into which it is passed by a particular duct; and this remnant of yolk is not entirely absorbed till several days after the chick has become a walking and respiring animal.

Harvey observed in the doe, about two months after impregnation, the first signs of the punctum saliens in something like an egg; and shortly after, a little vein, of a purplish colour, proceeding from it to an imperfectly-defined figure, seen swimming in the transparent liquor of the ovum. In six or seven days more, the regular fœtal figure begins to appear, and from that time goes on in regular progression to its ultimate growth; but, till near the third month, the ovum is not attached to the sides of the uterus, though the fœtus is seen formed, and may be extracted even of the length of an inch.

This account of the progress of gestation in deer is important, as the same may be said of the fœtus in man, and all vertebrated, viviparous animals,—that, till a certain period of gestation, it remains unattached by placenta to the uterus. At this period, therefore, of its existence, its life, like that of the oviparous animal, is all its own.

If the fact be granted, that in the three first months of pregnancy the viviparous ovum is not attached to the uterus by means of placenta, it is a necessary inference that, like the chick in ovo, it is alimented by the materials which surround it,—in both cases, the fluid in which it swims: that fluid, in both cases, is of the nature of albumen, with certain animal salts dissolved in it. When the fœtus weighs eight pounds, it is surrounded, according to Haller, by two pounds of liquor amnii; but the proportions, in various periods of gestation, are not accurately ascertained. At birth, on the skin of the fœtus, a curd-like matter is always found, probably deposited from the liquor amnii. This matter has been analyzed by Vauquelin, whose experiments prove that it is of the nature of fat, and consequently that this deposition of liquor amnii has undergone some animal transmutation. Now, then, asked Dr. J., how can we account for this deposition in the skin of the young animal, except from some transmutation or metamorphosis produced by an animalizing process? Does such a composition, as liquor amnii is known to be, naturally deposit a fatty substance? Certainly not. We are then to look to the living properties of skin for the explanation of this phenomenon, and we are naturally led to absorption as the explaining cause,—for what other action of the skin can we suppose? If we admit from these data that the skin absorbs, what has it to absorb but liquor amnii? If liquor amnii be absorbed, what can become of the matter absorbed, but that it pass through the necessary glands, and be thence forwarded to the circulation of the blood?

In the fœtus, a large glandular organ, provisorily constructed for its use, is found, which begins to lessen as soon as respiratory circulation and intestinal absorption are established. Another organ, the renal capsule, also diminishes in size after birth, though it never entirely disappears. On the other hand, the thymus, gradually diminishes in bulk, and altogether disappears in the end.

In regard to its use, Sir A. Cooper observes, "As the thymus secretes all the parts of the blood, albumen, fibrine, &c., is it not probable that it is designed to prepare a fluid fitted for the fœtal growth and nourishment from the blood of the mother, before the birth of the fœtus, and before chyle is formed?" To this great physiologist the discovery of the emissary ducts of the thymus gland in the calf is justly due; and can it be doubted, inquired Dr. J., though it has not yet been demonstrated, that the same structure exists in the human fœtus and the mammiferous tribes generally? If so, surely we are justified in extending the analogy of the use of conglobate glands, when intestinal absorption is established, to the thymus in the fœtal state; it is the gland, or the series of glands, placed for the infiltration, purification, and animalization of the matters absorbed by the skin—the skin which absorbs from liquor amnii the substance of alimentation for the fœtus, as the bowels

afterwards, by their lacteals in respiratory existence, drink up the matters fit for animal repair, and transmit them through the mesenteric glands to the great reservoir which pours them into the venous circulation.

May we not infer, continued the author, that albuminous matter is alike the food of oval and fœtal growth?—that in oval growth the organs called chalazæ are the instruments of conveyance, and probably of infiltration of the albuminous, for they shrivel up and are converted into earthy matter when the albumen is all absorbed? The conglobate mesenteric glands act the part of adult thymuses, purify, digest—perhaps secrete afresh—the matters already animalized and proper to be converted into blood. Respiration defecates and decarbonizes the blood. The arterial circulation sends it on its way to the million ramifications, of which the ultimate points deposit matter of alimentation, add to parts, supply deficiencies or take away redundancies; and, in fine, perform those physiological and pathological changes, the phenomena of which, though constantly before our eyes, have hitherto baffled our endeavours to explain them.

ROYAL INSTITUTION.

Friday, May 2.

BABBAGE'S CALCULATING MACHINE.

Dr. Lardner undertook to deliver a lecture on this subject this evening; but he devoted so much time to explaining the importance of numerical tables for astronomical purposes, and the difficulties of procuring them free from error, that a very few minutes remained for pointing out the excellencies and peculiarities of the calculating machine. Mr. Babbage, it appears, was originally struck with the uniform appearance of mathematical tables, and conceived the idea of the *possibility* of manufacturing them. And this is what he has actually accomplished. He has constructed a machine which can *manufacture* all kinds of numerical tables, and which is infallible in its operations. It not only calculates, but registers on copper its results. The principle, it may be briefly stated, on which it acts, is simply that of addition; and the various sums which result from the intermediate and final processes are thrown out by wheel-work, on a large frame divided into compartments by horizontal and perpendicular lines, at equal distances. While in action the machinery performs two grand movements: the figures proceed *laterally* in their successive additions, till the number to be registered is attained; and there is a *vertical* motion at the same time, which is one of compensation, or that by which the "carriages" are provided for. Thus the appearance of what takes place upon the frame is compared to the motion of waves proceeding in two directions, at right angles to each other, yet without mutual interruption.

At the conclusion of the lecture Dr. Faraday announced the presence of Dr. Dalton, of Manchester, among the visitors; and mentioned that he had prevailed on him to give a lecture on Friday evening next, on the philosophy of his own great discovery—the Atomic Theory.

CLINICAL OBSERVATIONS,

Delivered at the Middlesex Hospital, by

Mr. Arnott.

ERYSIPELAS.

Among the patients under my care, gentlemen, you have recently witnessed several cases of erysipelas;—you have had an opportunity of observing it in various parts of the body—the legs, arm, and head;—you have seen it following upon injuries, and occurring without any external cause: and as I may not have a better occasion for so doing, I will now briefly recal to your attention some of the particulars of these cases, and afterwards draw such inferences as may enable you more accurately to estimate the value of the general principle which guides my treatment of this affection. I am obliged to be egotistical on this subject, because it (the treatment of erysipelas) is one upon which the profession are even now not agreed. Some practitioners think it ought to be treated by bark and wine; some by bleeding and antiphlogistic regimen; and others by tartarized antimony: the notion of one party being that it is a disease of debility; of a second, that it is a purely inflammatory affection; and of a third, that it is somehow or other invariably connected with a derangement of the biliary system. Without dwelling on these speculations, we will first take the facts as they have been presented to us in the wards.

Case I.—*Phlegmonous Erysipelas of Forearm after Injury—Treatment by Incision.*

Thomas Goodman, 44 years of age, three days before his admission, was thrown from a horse, and pitched upon his left elbow. The parts were found greatly swollen and inflamed, with a lacerated and contused wound right across the olecranon, which wound looked foul, and was suppurating. The swelling was such as to conceal the outlines of the joint, and impeded all motion: no fever. Confinement to bed, low diet; a dose of calomel and jalap; twenty-five leeches to the elbow. Lotio plumbi cum opio. Second day, leeches repeated; salines with antimony. Fourth day, the swelling generally diminished; but on the back part of the forearm from the oleeranon half way down to the wrist it was increased, with redness of integument—not however strongly marked; but the swelling communicated to the finger that peculiar sensation described as a "boggy feeling." Purulent and glairy discharge, evidently from the inflamed bursa of the olecranon, which part is extremely painful. An incision, about three inches in length, made through the skin and subjacent cellular substance from the elbow downwards—the cellular substance loaded with yellow lymph and presenting one or two minute points of suppuration; the bleeding encouraged by fomentations; poultice afterwards. Sixth day: the inflammatory state of the forearm greatly abated. The case now proceeded uninterruptedly towards recovery.

The morbid condition of the parts in this instance has been here sufficiently described: it was inflammation of the integuments and cellular substance, constituting what has been called phlegmonous erysipelas, from local injury, this local injury involving a bursa. Phlegmonous erysipelas is not an unfrequent result of the injury of a bursa. I have seen some very severe cases of the leg and arm, from wounds of the bursa of the olecranon and patella; and situated, as these bursa are, it is not surprising that inflammation should extend from them to the cellular substance. You no doubt observed, that though the leeches diminished the general swelling, they were not confided in so soon as that "boggy feeling" was discovered in the part which indicated such a condition of the cellular substance as was shewn by the incision. The term "boggy" has been objected to, as unintelligible. I do not know who first employed it in the description of this disease: I suppose it must have been some one who had crossed a bog; and to every one who has done so, the word will not appear unintelligible or inapplicable. The object in incising the parts was to prevent suppuration and destruction of the cellular substance, which would in all probability have here taken place to some extent, had incision been omitted.

Case II.—*Erysipelas of Lower Extremity—Venesection—Disease limited in its progress by the Nitrate of Silver.*

Frances Giles, 42 years of age, was received into Bird's ward with a varicose ulcer of the left leg, of two years standing. She was confined to bed, and had the caustic potass applied over the diseased vein, so as to produce an eschar the size of a shilling: the original ulcer healed kindly; that left by the separation of the eschar more tardily, as a portion of the vein itself had sloughed, and was slowly detached. About six weeks after her admission, when the last-mentioned sore was nearly healed, the integuments of the leg, for some extent around it, became inflamed, attended by general febrile disturbance, white tongue, and a pulse with some resistance. No cause was discovered for this attack, but it was suspected to have arisen from an error in diet a few days previously. Twelve ounces of blood, which was slightly buffed, were taken from the arm. A dose of calomel was given, followed by a black draught. Cold lotion was applied to the parts, and she was put upon low diet. On the following day there was less heat of skin, and thirst. The inflammation had spread all round the lower part of the leg. She had been freely purged, and was now put upon salines with antimony: the cold lotion was continued. On the fourth day she had a dose of castor oil. On the sixth there was still less constitutional disturbance, but the inflammation was more extensive, attended with partial vesication and considerable swelling. The redness of the integuments had diminished in intensity in the lower part of the leg, but, on the other hand, it had extended to the knee, upon which it encroached. The solid nitrate of silver was applied to the sound integuments on the lower part of the thigh, so as to form a narrow circle (about half an inch broad) of vesication all round the limb; and warm fomentations were substituted for cold lotion to the leg, on account of their seeming more agreeable to the feelings of the patient. On the eighth day the inflammation had extended with great intensity up to the circle formed on the thigh by the nitrate of silver, but it did not pass beyond this. On the eleventh, the inflammation seemed to acquire fresh intensity on the lower part of the leg, and it now spread to the foot, with vesication, attended by a bitter taste in the mouth, and yellow tongue; the mercurial; and the saline purgative was therefore repeated for the third time. After this the symptoms gradually abated; and on the

fifteenth day, the tongue having become clean and moist, the patient was allowed beef-tea, with infus. rhei, mist. camph., and spt. ammon. aromat. for medicine, under which she gradually recovered.

This case might be called one of phlegmonous erysipelas. The swelling of the leg was very considerable; but there was no reason to believe that the effusion into the cellular substance was other than serous, and therefore not requiring incision.

CASE III.—*Erysipelas of Head preceded by Sore Throat, not from External Injury—Mild Treatment.*

John Playfair, nine years of age, pale, and rather delicate, was received into the hospital on account of simple fracture of the right thigh. A fortnight after his admission, having the three preceding days complained of sore throat, was found to have redness with vesication of the left ear. The tongue was furred in patches, as if embossed, but moist; the pulse frequent but soft; bowels purged. This boy's mother had been detected, a few days previously, bringing him in improper food; and a large piece of heavy plumb-cake was found concealed behind his pillow. Five grains of the hydrargyrum c. creta were ordered him twice a day, and the beef-tea which he had previously been allowed was continued.

On the third day the redness had extended to the cheek and part of the scalp. The head was shaved, and warm fomentations applied. By the fifth it had extended to the whole head, but with little swelling, the affection being limited apparently to the cutis; at the same time the face remained uninvolved, and pale. There was no affection of the sensorium, or heat of skin; the tongue had lost its patchy appearance; the bowels were opened twice daily, the motions being of a natural colour, but glairy. The hyd. c. creta was discontinued, and arrow-root substituted for the beef-tea, of which he had got tired. The redness of the scalp diminished; by the eighth day it had disappeared; and the patient was allowed a portion of animal food.

CASE IV.—*Erysipelas of Face and Head, not from external injury—Moderate Antiphlogistic Treatment.*

Timothy Cunningham, æt. 35, was admitted with three ulcers of the left leg, of two months' duration; but he had been subject to ulcerations of this leg for the last twenty years. At the expiration of a fortnight two of the sores had healed, and two days subsequently redness and swelling of the face took place, the eye-lids became closed, the affection spread to the anterior part of the scalp, and then gradually subsided, so that in little more than a week the parts had almost regained their natural appearance. There was very little acceleration of the pulse in this case, trifling heat of the skin, and no disturbance of the sensorium.

The treatment consisted of a mercurial, followed by a saline aperient, which last was twice repeated. In the intervals salines with antimony were exhibited; warm fomentations were applied to the parts affected, which he stated to feel comfortable.

CASE V.—*Surgical Typhus Fever — Phlegmonous Erysipelas of the Leg—Antiphlogistic Regimen and Treatment.*

Charles Dolly, 30 years of age, when presented for admission was stated by the house-surgeon to be more properly a physician's case, as he had typhus fever; and sure enough on examination he had symptoms sufficient to authorize the supposition. There was a degree of stupor upon him; he had a dull heavy eye; the skin was hot and parched; the pulse frequent; the tongue dry and brown, glazed at the edges, and cracked in the middle, with sordes on the teeth. It appeared that a month previously he had slightly scalded his right leg, which he had neglected, and that he had never been well since. The leg was found from the knee to the toes to be swollen, red, hot, and painful, with a small superficial granulating sore, over the shin. We decided that this was a *surgical* typhus fever, and took him into our wards. The patient was put upon low diet, had immediately a dose of the haust. sennæ comp., and the following day he was ordered five grains of the pil. hyd. every other night, and ʒss. of magnesia sulph. in infusion of roses every eight hours, which medicines were continued for a fortnight. A poultice was applied to the sore, and a spirit-wash elsewhere all over the leg. For two or three days I was in some doubt respecting the condition of the cellular substance, and feared that I should be obliged to make an incision through it, in the vicinity of the knee, where the swelling was greatest, tense, and firm; but this, with the pain and redness, gradually abated, and in about eighteen days they had disappeared, the ulceration also having healed. Commensurate with this was the abatement of the febrile affection; but you would observe that the tongue very slowly recovered its healthy condition, remaining for twelve days dry and cracked. The patient was three weeks in the house before we could allow him any improvement in his diet—any animal food. This case also might be called one of phlegmonous erysipelas, following a neglected ulceration from a scald.

CASE VI.—*Erysipelas of Head and Face, accompanied by Sore Throat—Venesection.*

John Johnson, ætat. 50, was re-admitted, having been discharged three days previously, on the healing of an abscess in the neck, for which he had been in the hospital about a month. On the day after his re-admission there was found to be considerable febrile disturbance, with head-ache, sore throat, full frequent pulse, and dry tongue. There was redness and swelling of the cheek, which gradually extended to the upper part of the face, and over the head, with slight delirium. Sixteen ounces of blood were taken from the arm; he was placed on low diet, and had one-drachm doses of the sulphate of magnesia, with fifteen minims of the vinum antimon. tart. in mint water, every six hours. The head was shaved, and the unguentum hydrargyri was applied over the whole of the affected parts. The action upon the bowels having been kept up for several days, the sulphate of magnesia was omitted; but as the tongue continued dry and furred, two grains of calomel were given with each dose of the saline and antimonial mixture, and under this treatment the attack, which was rather severe, gradually subsided. In nine days, desquamation commenced in the face; and on the eighteenth day the tongue having become most and clean, the salines were omitted, and he was allowed half diet.

CASE VII.—*Erysipelas of Head and Face after Injury—Local application of Leeches.*

Richard Thorpe, æt. 20, was admitted with erysipelas of the face and head; having, a few days previously, received a blow on the latter, which produced a superficial wound. The tongue was dry, and the pulse 92, without much resistance. The head was shaved, and cold lotion applied. He was put on low diet, and was ordered half-drachm doses of the sulphate of magnesia, with 10 minims of the vinum antimonii tartar, every four hours. On the second day after his admission the bowels had been freely opened, there was great thirst, and more redness and vesication of the face; pulse 94, easily compressed. Sixteen leeches were applied to the inflamed integuments of the temples; the saline and antimonials were continued, omitting the sulphate of magnesia.

3d day.—Leeches had bled freely; head felt relieved; tongue dry. Calomel and James's powder, of each three grains, to be repeated in six hours.

4th day.—Tongue moist, but furred; face and head greatly swollen, and red; pulse 98, small. Twelve leeches to the temples. Ol. Ricini, ℥s. stat. Omit. haust. Salin. et Vin. Antimon.

Evening.—Respiration frequent; bowels have been opened thrice; complains of pain in his stomach. Face very red, with great pain; pulse 102. Six leeches to head, and cold lotion. Hyd. c. Creta, gr. v. 4tis.

5th day.—Face much paler; pulse 92, small and feeble; tongue moist. There had been so much appearance of sinking this morning, that half an ounce of wine had been administered by the house-surgeon. To have five grains of the carbonate of ammonia, and a scruple of powdered bark, in an ounce and a half of camphor mixture, every four hours.

On visiting him in the evening, I found the face much flushed, the pulse frequent and voluminous. He had had one dose of the medicine, and as he complained of feeling extremely cold, another half ounce of wine had been given. Bowels opened four times; motions watery. I desired the wine, and the bark and ammonia, to be omitted, and that he should take five grains of the hyd. c. creta every six hours.

6th day.—The face paler; bowels open; motions still watery. Two drachms of castor oil immediately; omit the hyd. c. creta.

7th day.—Better in every respect. Tumefaction of face and head subsiding; pulse less frequent, and soft; tongue cleaning at the edges. Omit all medicine.

9th day.—Continues to improve; allowed a little beef-tea. About a week after this, when desquamation had commenced, this patient had (apparently from eating grapes) a fresh attack of erysipelas of the face and head, which again greatly reduced him. After his bowels had been relieved by castor-oil, decoction of bark, in combination with liq. acetatis ammon., was tried, but could not be borne: it was discontinued, and he gradually recovered, under a very rigid regimen.

CASE VIII.—*Erysipelas of Head and Face after Injury—Venesection.*

John Beach, thirty-nine years of age, falling from a height of seventeen feet to the ground on some loose rubbish, received a contused and lacerated wound of the scalp, over the right parietal bone; and from which he lost a considerable quantity of blood. The house-surgeon, finding the bone uninjured, after having had the head shaved, brought the edges of the wound together, applied cold lotion, and administered an aperient, which was repeated: for six days no unfavourable symptoms occurred. The wound, however, now suppurated; he complained of head-ache; there was frequency of pulse, and some appearance of inflammation of the cutis of the forehead. Fourteen ounces of blood, which was neither buffed or cupped, were taken from the arm; simple dressing was

applied to the wound, which was suppurating, and spirit wash to the rest of the scalp. A dose of the haust. sennæ comp. was administered, followed by salines with antimony. The following day the head-ache was relieved, the pulse was soft, and the bowels had been opened, but the redness and swelling occupied the whole forehead; and on the 8th day it extended to the eye-lids and face. On the tenth, the swelling and redness of the scalp was greatly diminished, but that of the face increased; the pulse was frequent, but soft: three grains of calomel were given with each dose of the saline and antimonial mixture. On the 13th, desquamation of the face had began; the calomel was omitted, and the patient was allowed some beef-tea; and a few days afterwards, quarter roast. Small abscesses formed in the upper and under eye-lid of the left eye, which were opened as soon as discovered, and left no permanent deformity.

I stated to you, gentlemen, that a difference of opinion still prevailed as to the nature of erysipelas, and of its treatment. Now with regard to the former, let us first consider what has been the character of the phenomena presented to you in the preceding cases. You have seen, then, in all, and in the majority after injury, redness, swelling, heat, and pain of the common integuments, accompanied by more or less of general fibrile disturbance. To the local affection you cannot hesitate to apply the name of inflammation. It has, indeed, been said that it is not a pure, a healthy inflammation, and chiefly from the different course it observes from a phlegmon—the essential difference being its great tendency to spread. Now, without alluding to the impropriety of the term *healthy* inflammation being applied to any particular form of the disease, seeing that they are all morbid processes, you will at once perceive that the distinction does not hold good when you carry your observations to other parts of the body. In other membranes as well as the skin, inflammation, when once excited, readily extends. This is the case with the serous membranes, the mucous and synovial membranes, &c.; so that if a distinction is to be established on this ground, the majority of those which we meet with in practice would not rank under the head of healthy inflammations. It has been stated that there must be something peculiar in the inflammation of the skin in erysipelas, as it only occurs in some individuals: no doubt it is so, but there is nothing singular in this. Wounds, and very severe wounds too, of other textures, are not invariably followed by inflammation. In the instance, too, of internal inflammations, as of pleurisy or bronchitis, although many individuals are exposed to the supposed and less manifest causes, only a certain number are affected.

Thus far in regard to inflammation of the skin following injuries: but you have seen that, in several of the cases which have been related, there had been no injury of the part in which inflammation of the skin showed itself, and that it arose without any very tangible cause. In these cases the face and head were the seat of the local affection, and when thus originating, and so situated, it constitutes what has been called by some *medical* erysipelas. It has been assumed that this affection is different in its nature, and requires different treatment, from the *surgical*; and as it not unfrequently occurrs in broken-up constitutions, in whom depletion is not well borne, it has been viewed by some as a disease of debility. But we are too much in the habit of considering inflammation to be a proof of, and to be dependent on, increased power, and of supposing it to be established that our remedial agents act by diminishing this increased power. However common such language may be, our real knowledge merely amounts to this, that certain morbid phenomena, which we designate by the term inflammation, more readily and certainly cease or disappear, under the administration of some remedies than others. We do not, however, find that all inflammations are equally amenable to blood-letting, mercury, or other antiphlogistic remedies; and, on the other hand, though they may not be so immediately subdued by these remedies, yet they are far more effectually guided to a safe termination, by a modified use of them, than by their omission, or by very opposite ones. Whilst, then, I acknowledge that the progress of this "medical" erysipelas has sometimes considerable resemblance to that of an eruptive disease, that there may be fever and sore-throat, as in the 3d and 4th case preceding the inflammation of the integuments of the face and head, and that the complaint may run a tolerably regular course, yet neither in the local affection itself, nor in the accompanying constitutional disturbance, nor in the effect of remedies upon both, does there seem sufficient grounds for supporting an essential difference in the nature of the two affections, nor for altering, though they may slightly modify our treatment.

Without, however, further enlarging upon this part of the subject, I will now come to what is more appropriate—to what is practical; and state, that I believe erysipelas to

be essentially an inflammatory affection, and is to be treated as such; but that this treatment is to be as much influenced by the result of observation and experience as those of other inflammations.

You would observe, then, that in all the cases which have been related, the treatment was essentially antiphlogistic; the patients were, with one exception, placed upon low diet, had aperient medicine and mild antimonials. That, in some, general blood-letting was employed; in others, local; whilst in one-half neither the one or the other was deemed requisite:—that cold local applications were used in most instances; in others, warm fomentations:—that, in only one case, stimulants were tried, and were obliged to be abandoned immediately, from the aggravation of the symptoms which ensued.

In the employment of general blood-letting I am influenced by the condition of the patient, the seat of the affection, and the degree of the constitutional disturbance: where the patient is young and strong,—where the head and face are affected, or where there is considerable fever and a strong pulse,—it is always a question with me whether I should not take blood from the arm. In one case (the 7th), a severe one of the head and face, where the patient's pulse did not authorize general blood-letting, leeches were applied to the temples, and repeated with marked relief. Aperients or purgatives are almost invariably useful; and where there is a foul state of stomach, I associate with them calomel or tartarized antimony. Respecting local applications, without making it an absolute rule, I generally order the cold or spirit wash, where erysipelas is the result of local injury. When the inflammation extends over the greater part of a limb, warm fomentations usually prove more agreeable to the patient's feelings than cold ones; and the same is sometimes experienced in erysipelas of the head and face of so-called spontaneous origin. You have seen the progress of the inflammation of the skin in one case limited by the application of the nitrate of silver; and in another of phlegmonous erysipelas you have witnessed the decided advantages of incision. It is almost superfluous to observe, that in some cases neither blood-letting, purgatives, or medicine of any kind, is requisite, but that simply by attention to diet and regimen, the disease will run its course in safety.

I am not unaware that a very contrary plan of treatment is adopted by some, that even to the young and strong wine and bark are given. Upon this I can only remark, that I have only very rarely found them either requisite or beneficial. That I do not entirely prohibit them, you must have noticed; but where they are employed, their effects should be closely watched, or in some cases irremediable mischief will be the consequence.

In conclusion, it may be as well for you to recollect, that in the treatment of the medical erysipelas at least you are rather to have in view the guiding your patient safely through the attack than the cutting of it short; and this object, I believe, you will more securely attain by adopting the general principles of treatment which you have witnessed than by any other.

SUPPOSED CASE OF HYDROPHOBIA,

Cured by Bleeding and Vinegar.

The *Gazette Medicale* of the 2d of May copies from a Bourdeaux journal a case under the above title, communicated by M. Dubedat. It appears that on the 22d April, the father of this gentleman was called to a woman supposed to be affected with hydrophobia. She was 32 years of age, of robust constitution, and the mother of three children. She stated, that on the evening of the 18th March she was bitten by a dog on the left heel; that the animal was suspected of being mad, and was killed in the neighbouring *commune*, having been hunted down in consequence of biting several individuals. Several dogs which had been bitten were destroyed by order of the Maire. When the woman, the subject of this case, learned these circumstances, which was not till four days after the accident, she became very much alarmed; but the wound was already almost cicatrized, and she endeavoured to compose her mind, and to forget it. In eight days she suffered from some excitement, speaking in such manner that she was several times suspected of having drunk too much. She afterwards became irritable and impatient, particularly on seeing or touching water. The wound became swollen, red, and itching. Finally, all the above symptoms increased, till, according to her own account, she had no longer any control over herself, and had a strong inclination to bite. At the time she was seen by the medical men, she was affected with general convulsions, constriction of the larynx, foaming at the mouth, screaming and groaning, and all her features betokened insanity. These symptoms were regarded as indicative of hydrophobia; and this

opinion was confirmed by observing that paroxysms were produced by the sight of water. She was bled to two pints, and immediately became calmer. She was then offered some drink, which she took; but, on attempting to swallow it, again fell into convulsions. Strong vinegar was afterwards given to her, of which she spilt a little, but drank about three ounces. She immediately became more tranquil. Again she was bled to another pint, and no paroxysm occurred for two hours. When an access seemed about to supervene, a glass of vinegar was administered; by which she was immediately tranquillized. Next day, only one threatening of a paroxysm occurred; and in a week she was able to return to her occupations—*blessing God and the doctors.*

RE-VACCINATION.

M. Moreau lately called the attention of the Academie de Médecine to the fact of his having then under his care a lady, who had been vaccinated early in life, but who took the precaution of having the process again recently performed, lest she should catch the small-pox from her husband. Though there was ample evidence of the original vaccination having been complete, the new process was not the less successful: the vesicles were perfect—*superbes.* Other cases were mentioned by different members present; and M. Bousquet took the opportunity of stating the following remarkable fact:—The son of Professor Adelon, a lad of sixteen, had been vaccinated when an infant; he received the pock again only eight days before M. Bousquet made these remarks; and that gentleman added, that he obtained as many vesicles as there were punctures made: he had even communicated the virus of those vesicles to four children, in all of whom it succeeded perfectly.

THE PARLIAMENTARY COMMITTEE.

The proceedings continue to be carried on briskly by Mr. Warburton and his fellow labourers. The chief witnesses examined this week have been Sir Astley Cooper, Sir Charles Bell, Mr. Brodie, Mr. Lawrence, Mr. Travers, Mr. Wardrop, Mr. Green, &c.

ST. THOMAS'S HOSPITAL.

Mr. South, we understand, was elected assistant-surgeon to this hospital on Wednesday last.

PRIZES AT THE COLLEGE OF SURGEONS.

We mentioned in a late number, that the Jacksonian Prize for 1833 had been adjudged to Mr. J. G. Crosse, of Norwich; we should have added, that Mr. R. R. Robinson, of Cooper's Row, Trinity Square, London, and Mr. G. T. Morgan, of Aberdeen, had obtained premiums of equal value for their dissertations.

NEW MEDICAL WORKS.

An Inquiry into the Principles and Practice of Medicine; founded on Original Physiological Investigations. By G. Calvert Holland, M.D. 8vo. Vol. I. 12s. bds.

Vindiciæ Medicæ; or a Defence of the College of Physicians. By Sir George L. Tuthill. 8vo. 3s. sewed.

The Dublin Journal of Medical and Chemical Science. No. XIV. 8vo. 3s. 6d. sewed.

Graham on Diseases peculiar to Females. 8vo. 10s. bds.

WEEKLY ACCOUNT of BURIALS,

From Bills of Mortality, *May* 6, 1834.

Abscess	3	Fever, Typhus	4
Age and Debility	28	Gout	2
Apoplexy	3	Heart, diseased	2
Asthma	5	Hooping-Cough	11
Cancer	1	Inflammation	35
Childbirth	4	Bowels & Stomach	3
Consumption	56	Brain	1
Constipation of the Bowels	2	Lungs and Pleura	10
Convulsions	21	Liver, diseased	5
Croup	5	Measles	5
Dentition or Teething	11	Mortification	5
Dropsy	11	Paralysis	2
Dropsy on the Brain	13	Spasms	2
Dysentery	1	Tumor	1
Erysipelas	1	Unknown Causes	4
Fever	4	Stillborn	12
Fever, Scarlet	3		

Decrease of Burials, as compared with the preceding week . . . 21

METEOROLOGICAL JOURNAL.

(Not come to hand.)

NOTICES.

We beg to decline the proffered critique on Castle's edition of Blundell's Lectures. The writer must be a very simple person to send us such an article.

Mr. Maclure's paper has been received.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MAY 17, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XXXII.

DIFFICULT LABOUR.—INSTRUMENTS.

Although in skilful, and especially discriminating hands, obstetric instruments must be regarded as great blessings to the suffering sex, yet it is a question with me whether by their unnecessary and improper use they have not produced on the whole more injury than good. From some practical knowledge of this subject, I am persuaded that instrumental interference has been often most culpably had recourse to, and that many women at this moment survive miserable victims of impatience, ignorance, or violence. I have reason, also, to fear that in no few instances human life has been the sacrifice—I speak in reference to the child—when patience, perseverance, and reliance on the natural powers, were the only obstetric auxiliaries required.

Let it not be thought by these observations that I am unable to appreciate the advantages sometimes resulting from instrumental aid;—let it not be supposed that I am silly enough to draw an argument against a valuable measure from the possibility of its abuse. I know too well that nature sometimes fails even in her grandest and proudest work—the continuance of the human species; and that occasionally death both to the mother and her offspring would ensue, unless art stepped in to relieve by her co-operation the embarrassments of her tutoress.

But I must beg deeply to impress upon your minds that urgent necessity alone will warrant us in taking up an obstetric instrument; and that when a choice is allowed us, we should leave nature to accomplish her own purpose,—provided, indeed, we can with safety trust her.

In your practice you will find it much more difficult to determine the time when instrumental aid may have become necessary, than to administer that aid; and, unfortunately you will find the most deadly means most easy of application. Many times, also, you will be almost persuaded against your own opinion to the adoption of these means by the urgent and unceasing solicitations of your patient. Let me entreat of you neither to allow these considerations to weigh with your judgment, nor to let that less worthy motive, a wish to take advantage of the *eclat* likely to result from a successful operation, tempt you to act contrary to your own feelings of propriety.

Two species.—We have already arranged instrumental cases under the second order of difficult labours, and have divided that order into two species;—the first, those which are accomplished by instruments perfectly compatible both with the life of the child and the safety and continuity of the mother's structures; and the second, those in which either the child's body must be mutilated, or a cutting operation be performed on the mother's person.

Instruments employed.—Four kinds of instruments, differing essentially in their fashion and mode of application, have been used to overcome the lesser degrees of difficulty which we meet with; by the employment of either of these, the labour is reduced to one of the first species of this order: they are the long and short forceps, the vectis, and the fillet;—the latter means is now most properly discarded.

The instruments resorted to in the second species of this order of cases are of a cutting character, and they may be re-

solved into two varieties—the *first*, those which are applied to the child, and are necessary for the performance of craniotomy, viz. the perforator or craniotomy scissors, the crotchet, the blunt hook, and the craniotomy forceps; the *second*, those which are applied to the mother's person, by which the Cæsarean section is performed, or the symphysis pubis divided, viz. the scalpel, bistoury, and others, which are auxiliary, and sufficiently well known in surgery to require no particular mention here.

FORCEPS.

History.—Among the ancient writers on medicine and surgery we meet with no description of any obstetrical instrument at all resembling our forceps. Hippocrates, indeed, and Celsus, both allude to instruments for the purpose of facilitating difficult labour, but they were of a kind designed merely to extract the child without reference to its life: they consisted entirely of hooks and crotchets; and their use must necessarily have mutilated the fœtal body.

The first gleam of such a contrivance sparkles in the works of Rhazes, the Arabian, who, in the latter part of the tenth century, described a fillet supposed to be adapted to this purpose. We find in Avicenna, whose work appeared nearly one hundred years after Rhazes wrote, the obstetric forceps mentioned by name; but whether they were of his own suggestion, or had been in use previously, is by no means clear: it is generally believed, indeed, that he was the orignal inventor. After giving some directions, the purport of which it is not very easy to make out, he says, "liget (obstetrix) fœtum cum margine panni, et trahat eum subtiliter, valdè cum quibusdam attractionibus. Quòd si illud non confert, administrentur *forcipes*, et attrahatur cum eis; si vero non confert illud, extrahatur cum incisione, secundùm quod facile sit, et regatur regimine fœtus mortui." In this quotation we have a plain proof that both the fillet and forceps were in use among the Arabians.

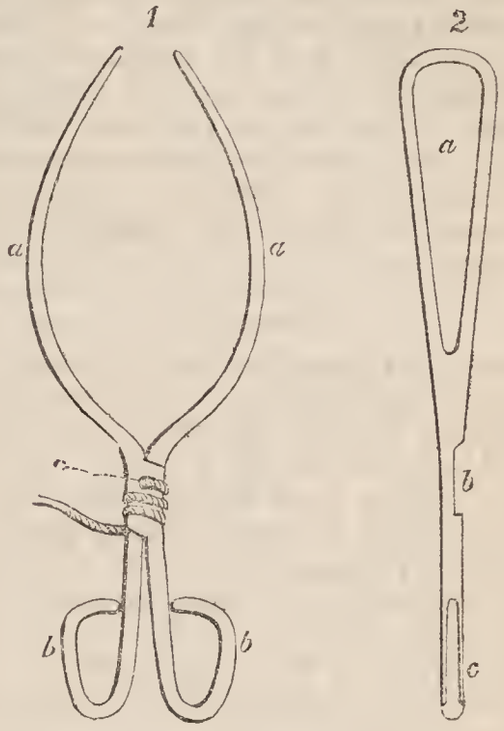
None of the contemporaries or immediate followers of Avicenna mention the forceps as an instrument applicable to expedite lingering labours except Albucasis, who lived about a century after. This latter surgical practitioner, however, has given a description and figures of numerous obstetrical instruments, among which are a short and long forceps; the former he styles *misdach*, the latter *almisdach*. It is sufficiently clear that these inventions were intended to supersede the necessity of opening the child's head, or otherwise mutilating it; and they seem to have been proposed with the view of extracting it alive. But I am quite at a loss to understand how this purpose could be answered; for they were formed with a sharp beaked point at the extremity of each blade, and projecting teeth on the internal surface, so that the integuments of the child's head and face must of necessity have been dreadfully lacerated. Albucasis, indeed, even gives directions, that if the head be too large to pass, it should be *crushed* by the closure of the blades. On the whole, therefore, the Arabian forceps, so far from being considered an improvement on the instruments previously in use, can only be regarded as a clumsy and barbarous attempt at what has fortunately in after ages been accomplished.

We find no particular mention of obstetric forceps from the time of Avicenna till the year 1554, when Rueffe, of Zuric, in a work, "de conceptu et generatione hominis," dedicated a chapter to the consideration of obstetric instruments, under the title "de modo, quo et quibus instrumentis impediti et mortui infantes producendi." In this treatise he describes and delineates two kinds of instruments, the one with a beaked extremity, and the other perfectly smooth and unarmed; which latter he distinguishes as "forceps qua dentes eruuntur:" and this is the first attempt recorded of any obstetrical assistant (except the fillet) by which it was possible to extract a fœtus without injury to its person. Rueffe's forceps, however, like all the former, were made with a common fixed joint, so that both blades must necessarily be introduced at the same time, and consequently they were almost useless, from the difficulty of their application. Rueffe has not left us the dimensions of the instrument he suggested, but from the cut attached to his work, they appear very similar to our common lithotomy forceps, with the exception of the internal surface being smooth.

It is believed that we are indebted to the elder Chamberlin for the valuable improvement of separating the blades, introducing them singly, and fixing them after their application. But the lock designed by Chamberlin (who was, from the clumsiness of the workmanship, doubtless not only the inventor but artificer of his own instrument), was by no means so firm and steady as to give the required stability, although it led to the joint now in use in England. As secresy in inventions, however, was in Chamberlin's time not only tolerated but practised among the most scientific and enlightened men, the peculiar formation of his instrument, and the mode by which he proposed to effect the extraordinary good he boasted, was not known to his contemporaries; and even to

this day we are without any authentic document to inform us in what his peculiar mode of delivery consisted.

His son Hugh, in the preface to a translation of Mauriceau's work, which he put forth in 1672, on his return after his disastrous expedition to Paris, has given us these memorable words:—"My father, brothers, and myself (though none else in Europe that I know), have, by God's blessing and our industry, attained to, and long practised, a way to deliver women, when the head, on account of some difficulty or disproportion, cannot pass, without any prejudice to them or their infants; though all others (being obliged, for want of such an expedient, to use the common way) do and must endanger or destroy one or both with hooks." He afterwards makes a lame apology for not giving publicity to their invention, considering it as a secret peculiarly their own, and one which they had every right to use in the way most consonant with their own advantage. By a fortunate accident, chance has brought to light what neither liberality nor a desire to benefit mankind would have conceded to us. Dr. Peter Chamberlin (the brother, I believe, of Hugh, whom I have just mentioned) purchased, some time before the year 1683, the estate of Woodham Mortimer Hall, near Maldon, in Essex; which continued in the family till about 1715, and was then sold to Mr. Wm. Alexander, who bequeathed it to the Wine Coopers' Company. About the year 1815, the tenant in occupation discovered in the floor in the uppermost of a series of closets, which are built over the entrance-porch, a trap-door. In the space between the flooring of this closet and the ceiling below, was found, among a number of empty boxes, a cabinet containing a collection of old coins, divers trinkets, many letters from Dr. Chamberlin to different members of his family, and some obstetric instruments. These instruments were given to Mr. Cansardine, by the lady of the mansion; and that gentleman, with the most praiseworthy generosity, presented them to the Medico-Chirurgical Society, on the 17th of May, 1818. The letter accompanying this valuable donation was published in the 9th volume of that learned Society's Transactions. From the very fashion of these instruments may be traced the progress of Chamberlin's invention: one appears to be a simple lever, with an open fenestra; another a double blade, connected together by a pivot, which, however, is rivetted; a third, in which the joint is formed by a pivot on one blade, receivable into a corresponding hole in the other; and a fourth, in which the instrument is fixed and made perfect by a tape passing through two holes—one in the commencement of each blade, where it springs from the handle. Such, then, we have now no doubt, was the improved invention of the Chamberlins.



The accompanying cut is taken from a drawing of the most perfect of Chamberlin's instruments. No. 1 is the forceps locked: *a*, the blades; *b*, the handles; *c*, the hole in the joint, through which is passed the string to connect the blades.

No. 2, the front view of a single blade: *a*, the fenestra; *b*, the groove in the shank, forming the lock, by which the two blades, perfectly similar in form, are adapted to each other; *c*, the handle.

The following are the dimensions: extreme length, 11½ inches; length of blade, 7¼ inches; of handle, 4¼; greatest width between the blades, 3 inches and 3-8ths; width between the blades at the points, ⅜ths of an inch; greatest breadth of the blade, 1½ inch.

The celebrity enjoyed by Chamberlin's *arcanum*, could not but attract the marked attention of the practitioners of his day; and consequently other men of inventive genius directed their minds to the same object. Dr. Wallace Johnson distinctly states that a pair of forceps came into his possession, which belonged to Mr. Drinkwater, of Brentford, "who commenced practice in 1668, and died in 1728:" these he describes as being similar to those used by Chapman and Giffard.

To Giffard, indeed, the credit is generally acceded of having improved the forceps, by forming them with an open fenestra; but after the account I have just given of Chamberlin's instruments, which all possess open blades, it will appear very questionable whether this idea is correct. Giffard began to employ the forceps in the year 1726, and sometimes used one blade simply as an extractor, sometimes both. We are in perfect possession of his practice, by the cases published after his death by his friend, Dr. Hody.

Well acquainted as we are with the minutest improvements in surgery, it is singular that we should be in ignorance as to who was the inventor of that greatest of all alterations in the obstetric forceps exemplified in our modern joint. That the idea originated from those which Giffard, Freke, and Chapman used, is sufficiently evident; but the improvement in stability, security, and power, is so great as to amount to an actually new invention. The credit has been generally given to Smellie; but I question whether this is correct; for Mulder, in his History of the Forceps, states that his preceptor, Du Pui, bought a pair of forceps with the improved joint, at the sale of Falconar's Museum, in London, in the year 1778; that he inquired of all most skilled in the science of midwifery, to whom was to be attributed the credit of the alteration; but that nobody could inform him, though all agreed it was made after Chapman had openly described his instrument. The latter-named practitioner, indeed, who was the second public English lecturer, used to shew a pair to his pupils, explain the mode of applying them, and published a treatise on the subject in 1733. Before this time—viz. in the year 1720—Palfyn, of Ghent, had also put forth a description of the forceps used by him, under the denomination of the *tire tête*; but from their delineation, as given by Mulder, we must judge them to have been clumsy, even in comparison with Chamberlin's.

The great similarity that exists between the instrument used by Chamberlin in the latter part of the seventeenth century, and that described by Chapman in 1733, forms a very curious part of the history of the forceps. The representation of the latter given by Mulder resembles, in many points, that specimen of Chamberlin's, a sketch of which I have just shewn you. It is difficult to account for this circumstance, when we consider the extreme jealousy with which Chamberlin kept his invention secret, except on the supposition that the instrument had been described to Chapman by some persons who had seen it employed by Chamberlin. Smellie, indeed, in his Introduction to Midwifery, states, that Chapman's instrument was the same which Chamberlin used; from whom he obtained his information, he does not disclose; but through whatever channel he became possessed of it, he seems to have placed perfect reliance on his authority, and that with good reason.

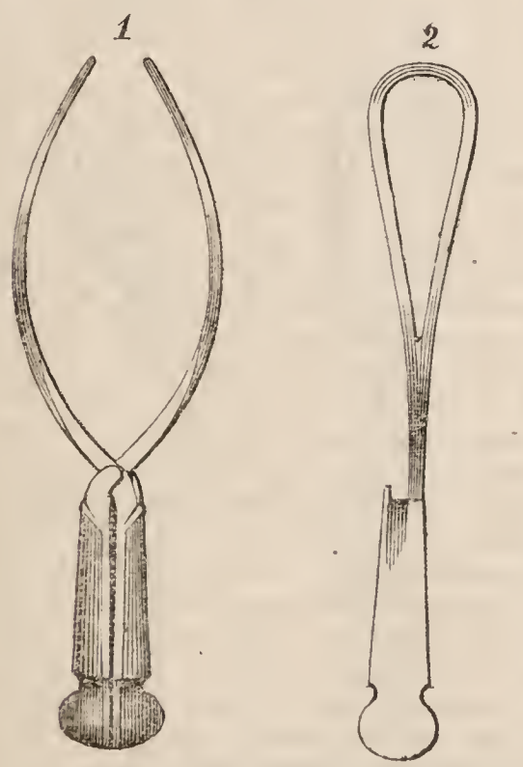
The blades of all hitherto described were straight; and Levret first added the lateral curvature, that the convex edge might fit into the concavity of the sacrum, and the danger of injuring the rectum by the point be prevented: this form was afterwards, with some variation, adopted by Smellie, Osborn, and Clarke.

Modern forceps. — Whim, caprice, and fancy (and perhaps also some little desire for professional distinction), have suggested an infinite variety of forceps, of almost all which Levret's and Smellie's patterns have formed the ground-work. Others are strongly stamped with an original character of increased utility; and such are the modifications suggested by my talented friend and colleague, Dr. David Davis, who has applied his mind, with very considerable acuteness and ingenuity, to improving the mechanism of this department of surgical science: he has adopted a wider blade than is commonly used; which allows the parietal prominence to pass more perfectly through the opening, and which embraces the child's head at more points of contact than Denman's, Smellie's, or Levret's. But it was evident that the irregularities of many pelves would not permit the introduction of two blades of the width which he thinks most appropriate; and he has therefore proposed to obviate this inconvenience in two ways—either by adapting a narrower blade to the wide one, or by using a short blade, as an antagonist to that first introduced. The great objection to Dr. Davis's invention, consists in the number of instruments which such a contrivance requires we should be furnished with, and the inconvenience attendant on that multiplicity; for it would seem that a fresh instrument, or at least a fresh blade, would be necessary for almost every variation in the position of the head.

Other less useful changes have been made by different practitioners: some have preferred adapting a hinge to the shank of that blade which was to be introduced under the right ilium, that the necessity of bringing the patient close to the edge of the bed might be obviated; others have, with the same intention, recommended that the handle should be made to unscrew, so that the blade might be introduced first, and the handle fixed afterwards. Saxtorffe, indeed, in 1791, added a hinge to each handle; but this was merely for the convenience of carriage, and is of a very different construction to that employed by Hamilton; for Saxtorffe's hinge turns inwards, while Hamilton's is bent outwards. Leake, in 1774, proposed a forceps with three blades; but this suggestion has been followed by no one of repute, as far as I know. For a more particular account of the different fashions of these instruments, down to the end of the last century, I shall refer those of you who are curious on the subject, to *Mulder's Historia Forcipum et Vectium.*

Points to be attended to in the choice of an in-

strument.—For my own part, I consider it of much less importance to discuss the merits of the various alterations which the forceps have undergone, than to obtain a knowledge of the cases requiring their assistance, and the mode in which that assistance should be rendered. I am in the habit of using Denman's straight forceps; and these I recommend to you, at least in the commencement of your practice, because each blade is, in shape, exactly similar to its fellow; either may be introduced first or uppermost; each becomes a right or a left-hand blade, according as it is adapted to the pelvis; and no thought or calculation is required as to which should be applied over the one or the other side of the fœtal skull;—which consideration in itself is very likely to embarrass a young operator, and may be the occasion of his failure. This instrument measures 11⅜ths inches from the extremity of the handle to the tip of the points; of which the blade occupies 7 inches, the handle the remainder; the groove for the lock being ⅜ths of an inch deep. The greatest width between the blades is about their centre, and measures 2⅞ths inches; the space between the points exactly one inch. The fenestra is in the shape of a kite, but considerably longer in proportion to its width: the blade in its widest part, near the extremity, measures an inch and three-quarters across; the extreme width of the fenestra being one inch and three-sixteenths. The blades spring from the locking part in a regular sweep outwards; there being no shank, properly so called. The whole instrument weighs ten ounces and three-quarters.



1. The instrument closed.
2. The back view of a single blade.

Besides these peculiarities necessary to be attended to in choosing a pair of forceps, there are many others of more trivial moment, which must not be passed over without notice. They should be manufactured of the best tempered steel, else they are liable either to break or bend. The lock should particularly engage our attention; this should be formed rather loosely, so that when the blades are adjusted one to the other, there should be a slight lateral motion allowed: for the space of at least an inch and a half from the handle, each blade should be of an uniform thickness,—so that it may be slid to that extent within the groove of its antagonist; for this, we shall find, assists much in its application under labour. No shoulders are admissible near the lock, no ornamental ridges, no serrated edges; every portion of the locking part should be perfectly smooth, and the corners rounded. The external face of the blade should, of course, be somewhat convex; so, indeed, should the internal also. All the instruments that I have ever seen have a rounded convex external surface, that the parts of the mother may not be injured; but in most the internal surface in contact with the child's face is flat. Every flat surface must have two sharp edges; and if strong pressure be applied, these edges will cut. To obviate the chance of disfigurement, then, the inside of the instrument must be rounded also. My father has strongly insisted on this improvement being generally adopted; and I have practically seen the utility of the recommendation in numerous instances.

Another point of great consequence is, whether we should have the instrument coated. It was the old fashion to cover each blade entirely with leather, that it might be less formidable to the sight; that, in locking it, little noise might be made; and that it might be softer to the woman's person, and therefore not so likely to do injury. Many of the various instruments on the table you see finished in this way. This practice was, in my opinion, objectionable on many grounds. In the first place the leather takes up room, and does not afford strength equivalent to the space it occupies; and we shall find in difficult labour, when disproportion from any cause produces the delay, that it is of consequence even to gain the thirtieth part of an inch in space. Again, the instrument does not pass up so easily when covered with leather, as when it is plain and polished. A still greater objection, however, has been urged against this practice, and one that has caused it to be generally abandoned. It has been supposed that infection—the virus of syphilis or gonorrhœa, for exam-

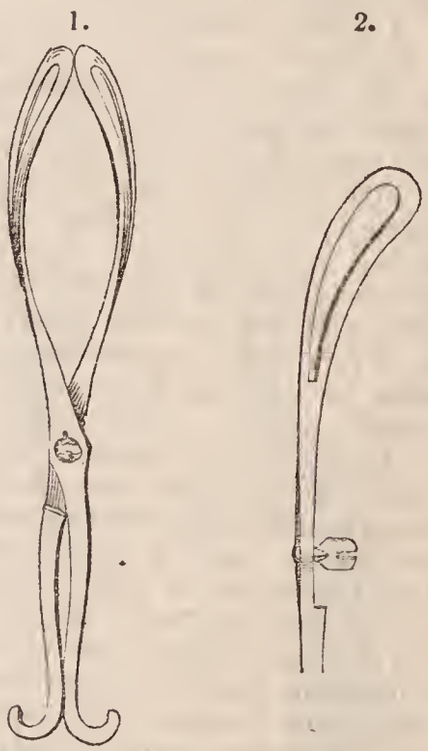
ple—has been carried from a diseased to a healthy person. If there be the slightest probability of such an accident, it would be our bounden duty either to discard the leather entirely, or to change the covering after each time the instruments are used.

In Smellie's plates you observe many of the forceps delineated as clothed in leather, and one or two are before you, in which the coating is only continued to the commencement of the fenestra; these are for the latter reason equally objectionable. For myself I care little for the clicking noise sometimes elicited on the blades being adjusted, as I never think of using these or any other instrument surreptitiously. Nor am I particularly anxious to conceal them from my patient's view, as if I were making use of a means I was ashamed or afraid to shew. It is neither necessary to do this, or to make an ostentatious display and parade before her. Many times, indeed, on being requested, I have not only allowed her to inspect them, but have in some degree described their action, and have permitted her to run her hand along them, that she might assure herself they possessed no cutting properties, and were therefore unlikely to hurt either herself or infant. I never found their use objected to in consequence of their being discovered to be formed of naked steel.

The only coating I would admit of is a silver wash: to this I have not the slightest objection; and if you are inclined to be very *natty* in your professional appurtenances, you may without any detriment to the efficacy or value of your instruments, require them to be disguised under the specious semblance of the precious metals. Those gentlemen who purpose practising in a warm climate,—the East or West Indies,—would do well to incur this additional expense in their obstetrical forceps, as well as other surgical instruments, not for the sake of appearance, but to prevent rust.

I have recommended the straight short forceps in preference to one with any of the lateral curves now in more general use, and given you some short reasons for my choice. I do not think it necessary to enlarge on this topic at present: when I come to speak of the adaptation of the instrument, you will see, I trust, good reason for following my recommendation.

French forceps.—In no part of operative surgery is the superiority of British practice over that of our ingenious and enlightened neighbours on the other side the straights of Dover more strongly exemplified, than in the contrast between our short forceps, and the one even to this day in use among them. The instrument I now hold in my hand,—more like a pair of tongs than a contrivance to extract the head of a living fœtus through delicate and highly-sensitive passages,—was presented to my father a very few years since, by a much-valued friend and late pupil of his, Mr. Wm. Wickham, of Winchester, who brought it from Paris, as a specimen of the advancement the French have attained in this department of obstetrical science. It was made after the most approved fashion in every respect; and he assured me that he saw a perfectly similar instrument employed by the celebrated Dubois, while he was sojourning in "the grand city." Its extreme length is nineteen and a half inches; the blade ten and a half inches; the greatest width between the blades is two inches and three-quarters; the extremities of the points are in perfect contact when the handles are closed; it possesses a slight lateral curve; each blade is fenestrated, and slightly hollowed out internally; across the widest part it measures two inches; and the greatest diameter of the fenestra, near the extremity, is one inch and a quarter. The whole weight of the instrument is two pounds, five ounces, and three quarters. Since the time I speak of, however, the forceps in common use in Paris has been decreased in size. Dr. Davis, in his valuable work on operative midwifery, published in 1825, gives the dimensions of a pair he had lately received, which are rather smaller than the one before you, weighing *only* two pounds and one ounce avoirdupois; and a pupil of mine, recently returned from France, tells



1. The instrument closed.
2. A partial front view of one blade, to shew the curve and peg by which it is locked.

me that Madame La Chapelle (the niece of the celebrated authoress on midwifery of the same name), who is now giving public lectures in this branch of science to the pupils of that city, uses one of still curtailed proportions, though of exactly similar form. The joint is on a very different construction from ours; and, without prejudice, I think we may affirm it is much more clumsy. It consists of a pivot and mortise lock, loosely fitted, and not difficult to adjust. The handles are entirely steel,—a continuation, indeed, of the material of the blade itself,—and bluntly hooked outwards at their extremities. With such an instrument in use, we cannot wonder at the necessity of the many cautions we meet with in the French works to prevent their *slipping*. We can only feel surprise that any child could be extracted alive through a pelvis under the compression which its head must necessarily suffer, if the forceps retained their hold.

Forceps a lever of the first kind.—It must be evident to you all, that when the two blades are adapted to each other, so that a compact instrument is formed, it becomes a lever of the first kind—the resistance being at one end, the moving power at the other, and the fulcrum between the two: you observe also, that this fulcrum is situated at the joint; that it is fixed, and its seat cannot be altered; and that, in the action of the instrument, one blade so completely antagonises the other, as to leave but a slight probability of the mother's structures being seriously compressed, provided it be used with caution and tenderness. There is but one modern of repute who has altered the situation of the fulcrum;—I allude to Assalini, so well known by his admirable surgical forceps for the purpose of securing deep-seated arteries. This ingenious surgeon has been by no means so happy in his attempts to improve the obstetrical forceps; his fulcrum is at the extremity of the handles, so that a lever of the third species is formed,—the resistance being at one end, the fulcrum at the other, and the moving power in the centre. Every body acquainted with the rudiments of mechanics must be aware that much power is lost by this contrivance: this objection, however, would be but trivial; for with the common forceps we possess much more power than we can generally dare to use. A greater objection would be the chance of making injurious pressure on the maternal structures, in working the instrument; and this has deterred me from making a trial of them. That they must possess some good points, however, is evident from the fact of the late Dr. Gooch (a name not to be slightly spoken), using and recommending them: for I am informed by a respectable surgical instrument maker in this town, that he has made many pairs according to Assalini's pattern, for Dr. Gooch; and furnished also many others to different practitioners, both in London and the country, on his recommendation. Although Assalini is generally supposed to be the original inventor of this variety, an instrument similar in principle was suggested at the commencement of the last centu y, by a Frenchman named Le Doux, as we read in Mulder's history. We there also find his forceps delineated; they consist of two separate blades, the *extremities* of the handles of which are tied together by a tape, which would render the whole instrument a lever of the third kind, and to all intents and purposes in action assimilate it to Assalini's.

MEDICINAL EFFICACY OF SARSAPARILLA.

To the Editor of the Medical Gazette.

Sir,

If you think the following communication worthy of insertion in your valuable journal, be pleased to insert it at your earliest convenience.—I am, sir,

Your obedient servant,

Isaac Lyon, M.R.C.S.L.

Hull, May 12, 1834.

In the number of the Medical Gazette, published on the 12th April last, I observed, in a paper by R. Williams, M.D., entitled, "On the Laws and Treatment of Syphilis," and read before the Royal College of Physicians a short time ago, the following remarks:—"And of a truth, after a long series of experiments to determine the sanatory properties of sarsaparilla in all diseases, I have been driven to the conclusion that its only valuable medicinal effects are those of healing ulcerated periosteal nodes."—Now, with the conclusion to which the doctor has arrived I can by no means agree; and I shall state the reasons upon which I ground my dissent. I think that my opinions may be of some value on this subject, when I state, that I have used the compound decoction of sarsaparilla not only most extensively in my private practice, and that my experience runs through a period of nine years; but that I was entirely engaged for three years and a half as one of the surgeons to the dispensary of this

town, where I had extensive opportunities of testing this subject. My experience I shall briefly state under the following heads:—The effects of the compound decoction of sarsaparilla (as that is the only form I consider of any use) in ulcers arising from buboes, in ulcerated throats, in nocturnal pains, and in debility arising from the use of mercury in the treatment of venereal disease. I have never met with any cases of ulcerated sores which have not been healed by its means, with the exception of one case, which continued fourteen months, resisting mercury, sarsaparilla, and numerous other remedies, but which healed at last, I do not know from what means. In ulcerated throats, I have never met with a case which resisted its action; indeed, if I were to point out any form of venereal disease in which the sarsaparilla has more effect than in any other, I should select this. Out of a number of cases which I have treated, I shall only relate one, which has occurred lately, and which I consider a most marked case of its good effects.

A. B., æt. 40, had sores on the penis and prepuce eight months ago, which healed under the use of mercury, applied externally and internally. The mercury was given so as slightly to affect the mouth. A month after the healing of the sores, an ulceration broke out in the throat, affecting the uvula, left tonsil, and back part of the pharynx, near the right tonsil. I thought this a fair case for trying the effects of iodine, not being aware at the time that Mr. Mayo had previously recommended the hydriodate of potash for the same complaint. I gave it internally, and also applied it as a gargle, and continued this treatment for two months, with little amendment, when suddenly the uvula was attacked with mortification, and sloughed off in two days. I now considered it high time to make a change in the treatment, and began with the compound decoction; and in less than four weeks all the ulcerations were perfectly healed. I have seen this patient a few days ago, and his throat continues well. Indeed, I may say that I have never seen a single case of a recurrence of these ulcerations, when sarsaparilla has been used. So far as a single case goes, it agrees with Dr. Williams's remarks on the uselessness of iodine in ulcerated throats.

In nocturnal pains in the bones I have never met with a case which has resisted its use; and the same I can say with respect to cases of debility arising from the excessive use of mercury. In support of the last statement I shall relate the following case, which occurred to me nine years ago, and which first gave me an insight into the value of this inestimable medicine, and for the use of which I feel indebted to my friend, Mr. Higson, house-surgeon of the General Infirmary of this town, and who holds this medicine in the same estimation as myself.

S. E., a widow, æt. 35, had different forms of syphilitic complaints, for which mercury was used very extensively, both internally and externally. When she presented herself to me she was in the following condition. She has lost the whole of the teeth of the upper-jaw, fifteen of which she shewed me in a box; she had lost the nasal bones; there was an opening from the mouth into the nose, and the right antrum was exposed, so that from being one of the handsomest-looking women in the town, she became one of the most disagreeable. By the use of sarsaparilla she became restored to health, and has since married, but has had no more children; having, by her former husband, had one.

The manner in which I use the decoction, is to give half a pint a day for the first two days, in three doses a day; the next two days, 12 oz. a day, so as to accustom the stomach to its use, and not nauseate it; and afterwards a pint a day. This quantity I continue from five to six weeks. I make the decoction after the formula of the London College, taking special care to have the sarsaparilla well bruised and macerated. I think the discrepancy of opinion, with respect to the value of this medicine, may partly be accounted for by sufficient attention frequently not being applied in its preparation. One effect of the compound decoction I have invariably found; which is, that the patient gains flesh during its use. Whilst stating my opinions with respect to this medicine, I may mention my experience with respect to dulcamara. I have tried it most extensively in the plan of sarsaparilla in the compound decoction, but have rarely found it answer, perhaps not more than once in twenty cases.

FUNCTIONS OF THE FIFTH PAIR OF NERVES—QUERIES.

To the Editor of the Medical Gazette.

Sir,

It is not without some reluctance that I again obtrude myself on the notice of your readers; but as the letter of Mr. Alexander Shaw, in your journal of the 3d inst., seems to call for a few remarks, perhaps you will permit me once more to occupy a portion of your space.

One of the principal objects I have had in view has been the carrying out, in its full extent, the principle so ably advocated by Sir C. Bell—viz. that the fifth is a nerve of motion as well as of sensation. In so doing, I have referred to various authorities, but more particularly to Sir Charles himself. I have examined the experiments which he has detailed, and have stated my views of their results. Having done this, as I conceive, fairly, and without any undue desire of straining facts to suit my own purposes, it would have been more satisfactory if Mr. Shaw had explained the fallacy of my deductions, instead of simply repeating Sir Charles's opinions. With a view of bringing Mr. Shaw at once to the point, I shall take the liberty of putting to him the following questions; and if he will have the kindness to confine himself to them, I think the matter may be soon disposed of, because on the manner in which they are answered depends the truth or error of my views.

If the fifth have no influence over the muscles of the face and forehead, why is every one of those muscles supplied with branches of this nerve?

Do the muscles—*e. g.* those of the eyeball, or the muscles generally—receive nerves for any other purpose than that of bestowing upon them the power of motion?

Is not Sir C. Bell's theory of the respiratory system as follows, viz.—that the muscles of respiration have a set of nerves for their respiratory motions, and another set for their voluntary motions; and do not the portio dura and the fifth answer to this theory as regards the muscles of the face?

Are not the muscles of the cheeks and lips used in mastication, as well as those of the jaw; and do the former acquire that action through the influence of the portio dura?

Is the portio dura a nerve both of respiratory and voluntary motion: if so, how does this agree with Sir Charles's theory of the nervous system?

If the fifth has no influence over the muscles of the face, how did it happen, in the experiment of dividing the superior maxillary branch on one side, that the lip was dragged to the other side; and that when the same branch on the opposite side was also divided, the lips were paralysed, so that the animal was unable to elevate or project them?

If the fifth has nothing to do with the motions of the eyelids and iris, how does it happen that injury to this nerve paralyses the orbicular muscle of both?

Is deformity a necessary result of loss of voluntary motion of the muscles of the forehead, any more than of a similar condition of the muscles of the jaw?

Does not the following passage in Sir C. Bell's work imply the existence of a nervous influence independent of the portio dura; and is not that other influence to be attributed to the fifth? "All such affections of the respiratory nerve will now be more easily detected; *the patient has a command over the muscles of the face; he can close the lips, and the features are duly balanced;* but the slightest smile is immediately attended with distortion; and in laughing and crying the paralysis becomes quite distinct."

Do not the following words of Mr. John Shaw imply the same? "On noticing the action of the muscles, which I did while the lady was sitting at luncheon, I observed that *no action was deficient while she was eating*, but that there was distinct paralysis when she smiled or laughed*." Will Mr. Alexander Shaw deny that his talented brother referred the action here noticed in eating to the fifth? If so, how will he get over the following remarks on a similar case related elsewhere?—"If told to laugh with the right side, she raises the angle of the mouth, but by an action which is evidently regulated *by the fifth*."

These are the points, then, on which I wish to be set right; and I think no one can justly tell me that there is no foundation for my views until he has replied to these queries. Here is ample scope for Mr. Shaw's exertions; and I do not know that his talents can be bet-

* Manual of Anatomy, 1821, page 290.

ter employed than in elucidating an obscure, complicated, and very important question.

It has not escaped my notice that experiments have been made upon the roots of the spinal nerves which go to prove that the different roots give off nerves possessing different properties; but is it quite certain that the union which takes place afterwards may not have the effect of commingling those properties, and thus rendering the various branches compound nerves? That the fifth nerve resembles generally the spinal nerve is a point which seems clearly established.

At present, my impression is, that where a branch of the fifth is distributed to a muscle, it is for the purpose of endowing it with voluntary motion, and by analogy of structure and actions I have extended the same view to the iris; and that where a branch of the same nerve is distributed to a sentient part, such as the skin, conjunctiva, &c. it is a nerve of sensation: I also consider that the portio dura presides over the involuntary or respiratory motions of the muscles of the face. In all these views I adhere to the principles laid down by Sir C. Bell in his theory of the nervous system: it is true I have differed from him in some of the details. I had the advantage of listening to his deeply interesting explanation of that theory when it was as yet in its infancy, and I have since perused most of his productions on that point. If I have come to unfounded conclusions, it will give me much pleasure to be set right, but it can only be done by showing me that those conclusions are unfounded.

I have the honor to be, sir,
Your most obedient servant,
JOHN WALKER.

Manchester, May 6th, 1834.

PROGRESS OF THE COVENTRY SELF-SUPPORTING DISPENSARY.

To the Editor of the Medical Gazette.

SIR,

THE principles on which Self-supporting Dispensaries are founded have been so luminously and incontrovertibly stated and defended in the Gazette,—in the writings of Drs. Storer and Calvert,—and in other publications,—that it would be a superfluous occupation of the reader's attention for me farther to enlarge on them: but it may be reasonably required from the advocates of these principles that they adduce, from time to time, the results of the system in actual operation, in order to show that the conclusions of reason receive the sanction of experience. Influenced by such considerations, I took the liberty, in May last, of publishing in the Gazette a statement of the origin and successful progress of the Coventry Dispensary; and as the lapse of another year has afforded additional confirmation of the correctness, and efficiency of the arrangements we have adopted, I beg leave to occupy a short space in the Gazette with such further particulars as the third Annual Report of our institution enables me to supply.

The Coventry Self-supporting Dispensary, as your readers may recollect, is unencumbered by a pauper or charity class of patients: it is confined to the "free members," or weekly contributors to its funds, and the number of these is limited to 2500. In the course of the year terminating on the 25th March last, 1668 patients were treated by the Surgeons of the Institution, and of these 515 had been attended at their own homes. The Surgeons' Report of the cases is as follows:

Cured - - - - -	1442
Relieved - - - -	44
Dead - - - - -	20
Under treatment -	110
Midwifery cases -	52
	1668

During the year, the Surgeons (whose skill and attention have largely contributed to the success of the Dispensary) had occasion to perform various important operations; among which may be named amputation of the thigh; two operations for strangulated hernia; one for aneurism; two operations for cataract; the removal of a sarcocele, &c.

From the opening of the Dispensary, 5610 cases have been treated, of which number 69 have died: and it deserves to be remarked, that of the 69 deaths, 30 were from tubercular phthisis, which reduces the ratio of mortality from other diseases very low. In their report the surgeons attribute this low rate of mortality in a great degree to one of the distinguishing excellencies of the institution,—namely, the facilities it affords for the treatment of diseases immediate-

ly on their invasion, in consequence of the unrestrieted privilege of application enjoyed by the members.

The weekly payments of the free members have this year amounted to 400*l.* 12*s.* which has been disbursed as follows:—

	£.	.
Paid for drugs - - - - -	112	12
Do. half-year's salary to the dispenser - - - - - -	20	0
Do. to the surgeons (Messrs. Nankivell and Bicknell)	268	0
	400	12

The payment of the dispenser out of this fund has now been made for the first time. The editorial artiele in the Gazette for July 27, 1833, speaking of these dispensaries, says—"Self-supporting they were intended to be, and such it ought to be the great aim to render them, whether they be for the present aided by honorary subscriptions or not: ultimately, let it be hoped that the "free members" will be able to disburse all the expenses, and thus realize the expeetations of all who patronized the plan from the commeneement." Acting in the spirit of this opinion, "the surgeons (I quote the words of our third Annual Report) desirous, that, as regards the purely medical department, the Dispensary should be as far as possible Self-supporting, have, in the most liberal manner, suggested that the dispenser's salary of 40*l.* (whieh by the third general rule was chargeable on the honorary fund) be in future paid out of the free members' fund." Few persons will hereafter dispute the right of the Coventry Dispensary to the title of Self-supporting.

The honorary fund (ineluding the balanee remaining from the preceding year) amounted on Mareh 25th to 254*l.* 5*s.* 7*d.* It should be understood that this fund is derived from the donations and subseriptions of the wealthier classes, who of eourse receive no assistance from the dispensary; and that it is applied to the payment of rent, &c.; of printing and stationery; of the salary of the clerk (who reeeives the weekly payments of the free members, and keeps their accounts); and (up to the middle of the last year) to the payment also of the salary of the dispenser. The disbursements for the last year (including 20*l.* the first half-year's salary to the dispenser), amounted to 116*l.* 14*s*; leaving a balance to be carried on to the following year of 137*l.* 9*s.* 7*d.* Of this balance, 100*l.* has been laid out in 3½ per cent stock, and will in future therefore be productive. By deducting 20*l.*, the half-year's salary of the dispenser, from the 116*l.* 16*s.* the future annual charge of our large institution, on the charity of the wealthier classes, will be shown. It would be possible, I believe, by increasing the admission of Free Members to make our Dispensary entirely Self-supporting, but in my communication to the Gazette last year (vol. xii. p. 428,) I stated some reasons in favour of an honorary subscription, which, if not decisive, are at least entitled to serious consideration.

Since our third annual meeting, Mr. Yeatman, of Frome, Somersetshire, has politely transmitted to the offieers of the Dispensary a small pamphlet recently published by him on the medical care of the parochial siek poor, with animadversions on Self-supporting Dispensaries. I have read the pamphlet with considerable interest, and am happy to bear testimony to the sound consideration which Mr. Y. has bestowed on the former of the two subjeets discussed by him. I agree with him in thinking that the system of contracting for the medieal care of paupers, cannot, in the existing state of pauperism, be got rid of; and I believe his suggestions for the correction of the prevailing abuses of that system to be practicable; that they would prove satisfactory to the community and the profession, and are of a nature to deserve the sanetion of the legislature. Mr. Yeatman lays it down as a principle, that "the moderate pay given to medical men by eountry clubs," would be deemed a fair remuneration for the attendanee on sick paupers; nor ean there be a doubt that it would. And as his great object appears to obviate that eompetition among medical men which has led to the disgraceful terms on whieh parishes have for a long time past been "farmed," he proposes that the resident Surgeons should attend, or at least be eligible to attend, in rotation.

In the proseeution of these views, Mr. Yeatman has constructed a table, according to which the rate of charge for attendance on paupers falls with the in-

crease of the number of *paupers*—not of *population*, for that would be a fallacious criterion. The table appears to me unexceptionable; and as it cannot be anticipated that medical men will voluntarily abide by it—that is, as competition will inevitably arise among them, and thus *reduce* the ratio of remuneration below the estimate of Mr. Yeatman, which is at once moderate and reasonable—it is a fit subject for legislative interference, so as to render it imperative on parishes not to contract for medical attendance on terms *lower* than those indicated by the table in question. But the system of attendance in rotation, of the surgeons residing in the same town or district, is no less indispensable than fixing a *minimum* rate of charge, otherwise the injurious effects of monopoly must be expected. The subject will doubtless attract the attention of the parliamentary committee on medical affairs.

Some of the strictures of Mr. Yeatman on Self-supporting Dispensaries are manifestly founded on garbled statements and partial evidence; and I cannot permit myself to doubt, from the candour displayed in his pamphlet, that on farther information his opinions will be considerably modified. Mr. Y. commences his observations by rather warmly objecting to the title of Self-supporting. He speaks of it as a "delusion"—an "offensive cognomen," which is "neither just nor honest"—terms manifestly stronger than the occasion calls for. Now whether we designate them Dispensary Associations, with Dr. Calvert; Union Dispensaries, with Mr. Yeatman; or Self-supporting, with Mr. Smith—though perhaps not quite a trivial matter—is not of vital importance. But as the larger proportion of the funds is every where derived from the "free members," *that* is the novel element, the characteristic peculiarity of the system; and it is as well so to denominate the institution, that its peculiar and prominent distinction be brought clearly into view. In some places—Lynn, for instance—the dispensary is entirely self-supporting; in Coventry, as regards all the medical expenses, it is equally so. These dispensaries, however, involve something more important than a mere name; they are a positive advance in the path of improvement—1st, by developing a source of revenue hitherto unattempted in such institutions; 2dly, by recognizing the principle of remuneration to the medical attendant; and, 3dly, by establishing an effectual check on that lavish administration of medical charity, the evils of which, as respects both the community and the profession, have been strongly perceived by many impartial and intelligent persons.

I am perfectly willing to concede to Mr. Yeatman, that Self-supporting Dispensaries cannot supersede contracts for medical attendance on paupers—at present at least; and I doubt, with Dr. Calvert and himself, the expediency of mixing up paupers and charity patients with a class of weekly contributors or free members. For the former, Mr. Yeatman has suggested an unexceptionable mode of supplying medical attendance: the *charity class* it is desirable altogether to *abolish*; and the rate of contribution in Self-supporting Dispensaries is sufficiently low effectually to attain that object, if it be the will of the medical men to accomplish it.

Mr. Yeatman objects that there is too much machinery in these institutions; he describes them, also, as being ever in a state of insolvency, and says, that the sums received by the attending surgeons are trifling, and unworthy their attention. As the Coventry dispensary has been conducted on a larger scale, and probably with equal or greater success than elsewhere, the machinery we have employed may very properly be brought forward in refutation of Mr. Yeatman's objections. That machinery is simple, not complicated. The clerk, who is also the dispenser, receives the weekly contributions of the members, which are brought to him at the dispensary; he keeps the account of these payments, which are deposited in the bank. The banking-book is in charge of the honorary secretary, who also keeps the general accounts of the dispensary, which accounts occupy annually but a small space in the minute-book. The minutes of the committee meetings are made by the honorary secretary. A sub-committee meets monthly; the general committee quarterly; the governors annually. The committee meetings occupy but a short time. I believe there is no more trouble in conducting our institution than there would be in a charitable dispensary of equal extent.

It is quite unnecessary to occupy the reader with any statements respecting the solvency of our dispensary, in addition to those which have been made concerning the honorary fund, in an earlier part of this letter. Last year there was a surplus income; in future the charges on it will be diminished.

As regards the receipts of the medical men, if Mr. Yeatman had taken the trouble to refer to the Gazette for September 1832 (vol. x. p. 808), he would have found, in an able editorial article on Self-supporting Dispensaries, a statement of the receipts of the surgeons of the Coventry Dispensary, which would have refuted his opinions; and before publicly advancing censures, it is but just that a writer should candidly examine *all* the available evidence on what he condemns. In May last, a communication from myself in this publication (vol. xii. p. 426), shewed that the two surgeons of our dispensary divided 263*l.*; and in the present year they have received still more, notwithstanding they have made a sacrifice in order to render the institution, with respect to the medical expenses, entirely self-supporting. Had this money been divided between six or eight surgeons, it would have been less worthy their attention, of course; and it is for that reason that it appears to me better to limit the number of surgeons to these institutions, so that the receipts may be of such amount individually, as to merit consideration. I fear these details may be tedious; but Mr. Yeatman has evinced such a zealous and considerate interest in subjects of this nature, that his animadversions are entitled to deliberate notice; and I trust what has been now stated will so modify his opinions, as to induce him to co-operate in the establishment of Self-supporting Dispensaries—a form of institution which will hereafter receive the cordial sanction of the moralist, the politician, and medical practitioner.

I am, sir,
Your obedient servant,
R. Arrowsmith, M.D.

Coventry, May 1, 1834.

P.S.—A few copies of the Rules, and Third Annual Report of the Coventry Self-supporting Dispensary, will very shortly be on sale at the house of Messrs. Longman and Co.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'Alembert.

Egypt and Mohammed Ali; or Travels in the Valley of the Nile. By James Augustus St. John. 2 vols. Longman and Co.

Passing over, for obvious reasons, the learned disquisitions on the antiquities, the eloquent descriptions of the scenery, the curious information as to the political and social condition of the people, and the estimate and illustrations of the character and views of the present despotic, yet rudely enlightened, ruler of the land of Egypt, with which these interesting volumes abound, we shall proceed to lay before our readers a few extracts which we have noted here and there, relative to medical matters—the hospitals, the schools, and the apparatus generally, which have been set up in that country for the cultivation and diffusion of the healing art.

Whatever institutions our friend Clot Bey, and his fellow-labourers, may have founded for the medical instruction of their adopted countrymen, the beneficial effects of these seem, from the following appalling account of the state of the *madhouses* at Cairo, not yet at least to have been extended to the insane; but this is as it has always been. It would appear, from the history of madhouses all over the world, that a judicious and humane system of managing the insane is among the latest fruits of civilization; the loss of reason having been, till a very recent period, in all countries, looked upon as reducing man to the same condition as the animals who do not possess it, and consequently warranting the madman's being treated exactly like a wild and dangerous beast.

"From the convent," says our traveller, "we proceeded to visit the madhouse, forming one of the wings or outbuildings of a spacious mosque, through the principal entrance to which we were conducted into the court where the insane are confined. In all countries a lunatic asylum constitutes a fearful spectacle, shocking to the feelings, and humiliating to the pride of humanity. But no where, perhaps, on earth, can any thing so terrible, so disgusting, be witnessed as the madhouse of Cairo; where, as may be certainly inferred from the ferocious as-

pect of the keepers, and the appearance of the victims, lacerated and covered with wounds, scenes of cruelty and suffering occur not elsewhere exhibited out of hell. In the centre of the court is a square pool, sometimes dignified with the name of a fountain; but which, in smell and appearance, rather resembles a cesspool, or a portion of a common-sewer. The atmosphere, impregnated by its infernal exhalations, is consequently more offensive and corrupt than that of a dissecting room in July; and the walls and pavement are covered with a green ropy matter and most dismal hue, which prepares the mind for the horrors to be witnessed in the cells. In the face of the dingy wall surrounding the court, are a number of square iron-grated holes, which would appear to lead to so many old neglected dens of wild beasts, but that within each, closely pressed, perhaps, against the rusty gratings, a human being is beheld, generally stark-naked. From the heavy iron collar encircling his neck, is suspended a massive chain; which, issuing through the grating, and running like a festoon along the wall to the mouth of the neighbouring den, connects him with his next companion in madness; so that when one retires into the cell, the other, at the opposite end of the chain, is necessarily dragged forward in proportion."

After this painful general description, the author next gives us an account of a few individual cases.

"In the first cell, commencing on the right, was a young Arab, sunk in a lethargy, from which nothing could rouse him. He turned his eyes after us as we passed, otherwise he might have been taken for a statue. The next was an Arnaout soldier, who, becoming mad in Candia, had been sent thither by Mohammed Ali, to spend the remainder of his life in chains. He sat cross-legged, close to the gratings, perfectly naked, with his arms crossed upon his breast, and his eyes closed, as if in a dream. Being roused, and called upon by name, he slowly opened his eyes, while one of the bystanders presented him with a flower, which he smelled to, and appeared to regard with interest, smiling when addressed, but uttering not a word; and when we quitted him he again relapsed into his dreaming state. The individual occupying the neighbouring cell, lying in a corner rolled up in his blanket and mat, lifted up his head when called upon, stared wildly at the spectators, and then, covering himself again, refused to come forth. Seated by the next grating was a youth about eighteen years old, who, having been forced away from his village, and carried as a conscript to the army, had grown mad with the thoughts of home; but by proper treatment might probably have recovered. Beyond this young man was a lively prating Arab, who related with singular fluency the history of his imprisonment, caused, he assured us, by his sister, who, having led him away from his village, had afterwards entrapped him into this place. To him succeeded another Arab, wanton as a satyr, equally talkative, and equally mad; but it is impossible to describe one by one all the dwellers in the prison-houses. The most fearful example of—

> 'Moody madness, laughing wild
> Amidst severest woe,'

was a Cairean, of respectable family, covered with boils and scars, gaunt, emaciated, and consumed by the fever which had destroyed his intellect. His burning eye-balls, bloodshot, and ready to start from their sockets, were rolling wildly as he exhibited, in the most shocking manner, the loathsomeness of his disorder. Close to this man was a religious fanatic, who, discovering us to be Franks, was loud in those terms of abuse which none but madmen could now utter with impunity in Egypt. Among these lunatics, there was one individual, who having, as he himself related, been guilty of a crime of unspeakable enormity, had been therefore suspected of insanity, and confined in this dismal place, where he was employed in manfacturing hooks and eyes; and except that he spoke laughingly of his demoniac flagitiousness, exhibited no signs of madness. Perhaps, when apprehended, and in danger of condign punishment, he had assumed the appearance of lunacy to save his forfeited life. The old Arab keeper who shewed us round the building, having been rendered by long habit utterly insensible of the misery he witnessed, laughed heartily at their wild incoherent babbling, which to him was merely matter of amusement. How the wretched creatures are fed and treated, I know not.

The establishment is sometimes visited by medical men; but the mere appearance and economy of the place are a blot on the character of Mohammed Ali, and prove him to be destitute of the ordinary feelings of humanity. His lions and elephant in the citadel are better housed. In the adjoining hospital, the arrangements are somewhat superior, though it may still be doubted whether it be a nuisance or a benefit to the public, the rooms, open in front, and closed at night with a mat, extending round a filthy court, and being furnished each with a stream of water, standing like a puddle. A few miserable wooden bedsteads stand in the middle of the apartments, which appeared to contain no other convenience. All the patients, several of whom lay stretched, perhaps in malignant fevers, on their shattered couches, exhibited a filthy and squalid appearance, causing us to shudder as we passed. The apartments of the women we did not visit."

Some interesting observations on the comparative salubrity of Alexandria at different times, and the general shortlivedness of the Egyptians, both ancient and modern, occur at p. 359, v. ii.; but want of space obliges us to pass them over, and to come at once to Mr. St. John's account of the medical school recently established by the Pasha. In quoting, however, what follows, we may be permitted to wish that our traveller had entered with greater and more instructive minuteness into the details.

"The School of Medicine at Abouzabel is without doubt one of the most extraordinary of all the Pasha's establishments: but it is not situated in the desert, as has sometimes been pretended; on the contrary, it is encircled by vegetation; and, in the month of March, nothing can be more beautiful than the surrounding scenery. The hospital, to which a botanic garden has been attached, is an enormous quadrangular building, divided from the lodgings of the Europeans by a broad esplanade, planted with long avenues of mimosa, sycamore, and date trees. Along the exterior, on three sides of the edifice, a large space has been inclosed; and here all kinds of vegetables are successfully raised, besides an immense quantity of oranges, sweet and bitter almonds, lemons, figs, and pomegranates, for the use of the sick and convalescent. In the centre of the botanic garden is a large square building, containing a museum, dissecting-rooms, a theatre for lectures, a laboratory, a dispensary, and dependencies, with store-rooms, kitchens, and baths, both in the European and Oriental style. A moiety of one side of the hospital is converted into a spacious school-room, capable of containing two hundred boys, the walls of which are adorned with designs, illustrative of science, and the different phenomena of nature. The remainder is occupied by a lithographic printing-office, where, by means of four presses, the young Arabs are constantly employed in printing Arabic translations of the best European works on medicine, with anatomical drawings, which they certainly copy with great accuracy.

"None of the other schools possess an air so entirely European. It is kept very clean; and the traveller is sometimes startled at being addressed in French by ill-conditioned fellahs, who are instructed in history, geography, arithmetic, botany, chemistry, together with the theory and practice of medicine and surgery. At the head of each department of the medical science is an European professor, who draws up his daily lesson in French, which is then translated into Arabic by able interpreters, who, from their long employment in the hospital, are themselves tolerably well acquainted with the science. The translations, when completed, are submitted to three learned Sheïkhs, who correct grammatical errors, and clothe them with the beauties of the Arabic language; after which they are printed, and delivered to the students. Practical knowledge is acquired by attending the sick, compounding and making up of medicines, and the constant use of the dissecting-knife. The fault, however, complained of in all the other schools, is likewise committed here; for the government, impatient to avail itself of the knowledge it has imparted, withdraws the youths from their studies before sufficient time has been allowed to perfect themselves in their profession. They are, in fact, removed four years after their entrance, however trifling may be their proficiency, and, being sent directly to the army, frequently do more mischief than good.

* Page 401, vol. ii.

Among the students are three eunuchs and four female slaves, who are educated for the interior service of the Pasha's harem. The number of pupils amounts, at present, to about two hundred; and the hospital contains accommodation for six hundred patients. Adjoining the school of medicine at Abouzabel are a veterinary hospital and school, under the direction of a Frenchman. One hundred and fifty pupils are here instructed in the duties of their profession, on precisely the same plan as that pursued at the neighbouring college. As soon as they have learned to kill a horse, they are forwarded to the regiments. The Pasha is particularly careful that all his different employés—for example, the directors of the chemical work, the saltpetre refinery, the iron-foundry, the cotton-mills, and the works where the chloride of lime is prepared—have each a certain number of youths from the college of Kaperlyne placed under them, to be instructed in their business. M. Golloway has five, and a young Armenian, who received his education in England, twelve Arab youths under his care. Mr. Wallace, the English engineer, is the only person who has refused to comply, on this point, with the wishes of the Pasha."

We here close our notice of these volumes, and in doing so we gladly bestow our thanks upon the learned author for the instruction and entertainment he has afforded us; and we strongly recommend his work to the attention of our readers, as containing a great deal of information on all subjects connected with Egypt, both in ancient and modern times; conveyed, too, in a style eloquent, from the author's enthusiastic love of his subject; and learned, from his having assiduously prepared himself, by reading and research of all kinds, for the task which he had so zealously undertaken.

Leçons de Clinique Médicale, faites à l'Hôtel Dieu de Paris, par le Professeur A. F. Chomel, *recueillies et publiées sous ses yeux*, par J. L. Genest, D. M. P. &c. Paris et Londres. Baillière.

This is the first volume of a work which is intended to be comprised in two, and to contain the *medical* opinions and practice of M. Chomel; serving, in some measure, as a counterpart for the *surgical* theories and treatment given by M. Dupuytren in the *Leçons Orales*. Yet the plan of the two performances is very different, inasmuch as we have in the latter, for the most part, the animated language of the distinguished lecturer on a variety of subjects, coming after one another in rapid succession, and always full of interest; while, in the *Leçons de Clinique Médicale*, the views of Chomel, and his eloquent expositions of the several points which he discusses, are given in a digested form, and partake more of the nature of a systematic treatise than of the didactic discursiveness of a lecture.

M. Chomel has long been distinguished in the Parisian school, for the ability and zeal which he displays in his *clinique*. He has been greatly followed, and we presume he continues to be as popular as ever, since his removal from the Charité to the Hôtel Dieu. His pupils were always delighted with the clearness of his manner and the elegant simplicity of his language: he seldom theorizes; occasionally, however, when unavoidably touching on controverted points, he never fails strenuously to advocate the opinions which he has long maintained in his writings. In his practice M. Chomel is a man of judgment and energy, an excellent observer of symptoms, and inquirer into causes; he is bolder than the generality of his professional countrymen, and ever ready to adopt that mode of treatment which circumstances demand, careless of the dogmas which characterize any particular system. He has adopted some of the principles of Broussais, and may be considered, perhaps, as one of the most rational disciples of the *physiological* school. In the treatment of fever he is known to be a most judicious practitioner; he combats the disease with practised arms, and freely adopts those *innovations* (as some of his brethren would call them) of which experience has shewn the utility.

The volume before us is wholly devoted to the consideration of Typhoid Fever. We cannot at present proceed with an analysis of it; but we shall take another opportunity of laying before our readers the best things that it contains.

MEDICAL GAZETTE.

Saturday, May 17, 1834.

" Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

MEMORIAL OF THE MEDICAL TEACHERS.

SOME idea of the importance attached to the petitions of the teachers connected with the metropolitan schools of medicine, and of the Council of the College of Surgeons, against the claims of the London *University* to a monopoly, may be gathered from the extraordinary extent to which they have roused the bile, and excited the apprehensions, of the respectable patron of that politico-medical establishment. " In both documents (says this truth-loving writer) insulting and abusive (!) language is directed against the Senate and Council of the new institution;" and again, " there is much fraud in these documents: one of them is the result of trick, and the other is redolent of falsehood." Assertions more grossly untrue, we venture to assert, never emanated even from the pen of him who is universally allowed to be beyond all question the most mendacious writer of the day; but they are thus far satisfactory, that they shew his employers to anticipate the result which is to follow, and himself to be driven, in the dearth of argument, to his usual expedient of slander.

The petition, or memorial, addressed by the teachers connected with the numerous medical schools in London, we hesitate not to pronounce to be one of the most temperate, argumentative, liberal, and convincing papers of the kind, which can well be conceived; and we *know* that it has been regarded as such by those whose opinion is of rather more importance than that of the literary bully—who inveighs against a document *which he has feared to lay before his readers.* The case of the medical schools, as opposed to the monopoly sought for by the London *University*, has been given in the pages of this journal alone; and the omission of our contemporaries cannot fail to be acknowledged as characteristic of that candour and good faith for which we have always held them to be distinguished. How far the unanswerable nature of the arguments against his employers has influenced the one, or the circumstance of the omission of his name in the list of signatures operated on the other, we leave our readers to decide. But perhaps we are wrong in our surmise, and the cause of apprehension and concealment may be, that ninety-nine gentlemen engaged in teaching the various branches of medicine, including the members of every respectable school in the metropolis, and many of them the most eminent persons of the day as practitioners and men of science, have publicly declared their concurrence in the opinions which have long been maintained in the pages of the *Medical Gazette.*

It may be matter of editorial policy with our opponent to conceal, as much as possible, from country readers, the relative position occupied by this journal and his own in London, where the merits and demerits of the different parties are best known: but we do think, notwithstanding the above obvious cause of offence, that men who have devoted their time and study to the difficult art of communicating instruction, and who have expended large sums of money in procuring the requisite materials and means of illustration—in short, that a body of men who have embarked their capital, both of intellect and gold, in the formation of medical schools, scarcely deserve to be assailed with opprobrious epithets, because they step forward and protest against one

particular establishment obtaining *exclusive* privileges, such as would place it above the reach of competition, and make its success quite independent of its merits.

This is the "fraud," the "trick," and the "falsehood," of which the great body of medical lecturers have been guilty; they have stepped between the public and the shareholders in the London *University*, and prevented the establishment of a monopoly by the very parties who have heretofore professed themselves to be the enemies of every such abuse. But the whole proceeding is an anomaly and a contradiction—a mockery of consistency, and a satire upon liberalism. A party—to many of the avowed principles of which we profess ourselves to be attached—in the days of their exclusion from political power, established an institution which they not unreasonably, perhaps,—though as the result has proved, erroneously—anticipated was to spring with rapidity into a great and flourishing school. It was to be the very antithesis of all chartered corruptions—the *beau ideal* of whatever was independent, liberal, and generous. Had the eminent persons with whom the idea originated, and who presided over it at the commencement, remained in the opposition, instead of being called into office, it is probable that the succession of blunders which has marred the prospects of the establishment in question, would have been avoided; but the very circumstance of their increased power, which might have been expected to place the infant *University* in a better position, seems to have been indirectly the cause of its decline. New and higher occupations engrossed the attention of those whose guiding intelligence was required, and probably would otherwise have been bestowed upon it; the consequence of which was, that the affairs of the institution fell at a critical moment into the hands of incompetent individuals, under whose mismanagement the *University* was brought to such a state, that while it was reduced to beggary as to finances, it became the scene, in its educational affairs, of endless caballing,—the most respectable men connected with it successively resigning, in despair or in disgust; the Warden, and the Professors of Medicine, of Surgery, of Anatomy, of Medical Jurisprudence, of Clinical Medicine, of Natural Philosophy and Astronomy, of Mathematics, of Greek, of German, of English Literature, &c. &c., having all been changed within an inconceivably short period. An inferior style of men necessarily succeeded; and some who never did procure, and never could have procured, decent audiences as private lecturers, were lauded to the echo as Professors, by papers expressly devoted to their cause. In proportion as the real claims of the institution to public patronage diminished, a system of boasting and pretension was adopted, discreditable to science and unparalleled in the records of puffing—if we except the attainments in this department of literature which have been made by the patrons of Warren's blacking or Morison's pills. Yet this is the *University* which now comes forward and claims a charter, embracing the power to confer degrees "in all subjects not theological;" or, to express their wish in plainer terms, "to confer degrees in medicine;" for the so-called *University* amounts only to a grammar-school for boys, and a medical-school, the pupils at which last can alone be eligible for any academic distinction.

To every unprejudiced mind it must appear quite clear, that however convenient it may be for the shareholders and professors that the monopoly they seek for be granted, yet there can be no *public* ground assigned for such proceeding, except that of relieving the Dissenters from a portion of their academic

disabilities. The question, then, resolves itself into this—whether, in the event of such relief being determined upon, its accomplishment would be best effected by conceding the power in question to the establishment in Gower-Street, and, by making it really a University, entitling it to the appellation it has usurped? For an answer to this question, we refer to the memorial of the medical lecturers so often referred to, in which the inexpediency as well as the injustice of such grant, is set forth in a manner which must carry conviction to the minds of all but the incorrigibly prejudiced, and those whose understandings are rendered immoveable by the weight of pecuniary interest. But is there no alternative? Does it follow, that if the privilege of conferring medical degrees be not given as an exclusive bonus to save the London *University* from sinking, it must therefore be denied altogether? Here, again, we refer to the memorial; and we must say that the spirit of liberality which breathes in every line presents a striking contrast to the prayer of those who, with the fairest professions on their lips, have secretly endeavoured to procure a chartered right of filling their own pockets at the expense of their neighbours. Hear the petition in which all the medical schools in London (except that of the *University*) have concurred, and let the public judge between the principles and feelings it displays, as contrasted with the abusive declamation adopted by the partizans of the latter, who, in the very lunacy of party spirit, free the claimants of an exclusive privilege from the charge of seeking a monopoly, and denounce those as monopolists who pray that the pupils of all the schools may stand on a footing of perfect equality!

"There is, however (say the memorialists), another mode by which the Dissenters from the established Church might, as far as academic honours are concerned, be effectually relieved; and which we humbly, but most earnestly, recommend to the attention of his Majesty's government—namely, the establishment of a metropolitan University, framed in accordance with the liberal spirit of the age, and free from the difficulties of ecclesiastical distinctions. If this idea should be thought worthy of adoption, and it should be deemed expedient that degrees be conferred in London, we take the liberty of suggesting that all the advantages of such a plan would be secured, and that all the evils before pointed out would be avoided, by vesting the requisite power in some independent body, not concerned in teaching; so that all those who have been educated in any of the efficient schools should be admissible to examination for such degrees on principles of perfect equality. In this manner a Medical University might be established in London, to which the several schools, and among the rest the London University, would stand in the relation of so many colleges."—Memorial, &c.

But why have we again entered upon this discussion, and for whom are these observations intended? Not for the Privy Council, for their decision has been already formed—not, assuredly, for the applicants to that right honourable body, and still less for their blustering advocate. It is to none of these, but to the members of the House of Commons, we desire to address ourselves, through the medium of our professional readers. The application of the London University, failing in the Privy Council, will be carried into Parliament; and it is important that the greatest possible number of honourable members should be made acquainted with the true merits of the question. Several even of the most intelligent members of the government acknowledged, when they heard the memorial read, that they had been previously quite unaware of the facts. What, then, can be expected from mem-

bers of parliament whose minds have never been directed to the subject at all? Let each of our readers place the memorial in the hands of as many members as he has access to; above all, let him enforce these two points—first, that granting the prayer of the London *University*, is neither the only nor even the best mode of relieving the Dissenters, *as they have been told;* and, secondly, that the petition is an attempt to steal a march upon the Committee of their own House, now sitting, upon the state of the medical profession generally; and that the whole of the evidence elicited by them is, without exception, against granting the coveted monopoly. The battle is already all but won; let this be done, and the victory will be complete.

COLLEGE OF PHYSICIANS—PETITION OF THE LICENTIATES.

Our opinion of the College of Physicians, as to the defects in its constitution, has been often expressed, and does not require to be again repeated: it is enough to say that it remains unchanged. But this does not prevent us from listening with candour to what is said in its defence; and we have therefore perused with attention two pamphlets which have recently appeared—one is entitled "*Vindiciæ Medicæ*, or a Defence of the College of Physicians," and the other, "Answers to the several Allegations contained in the Petition of certain Licentiates," &c. Between these two there is a similarity almost amounting to identity, the chief point of difference being that the former is more detailed than the other, but without otherwise differing as to the line of argument, or even as to forms of expression. In the latter, the several allegations in the petition of the Licentiates are taken seriatim, and printed with the answers on the opposite page, numbered to correspond—the accusation and the defence thus facing the reader in parallel columns.

No one can deny that the petition of the Licentiates was clearly drawn up, or refuse it the merit of being temperate, methodical, and imposing; but the brochures before us prove that it contains one pervading blunder — it founds the claims of the Licentiates to redress on the *law* of the case, and the law is clearly against them. On this point they are dead-beat. The petitioners ought to have taken their stand on surer ground, and have insisted on the system of exclusion being contrary to the spirit of the age—on the constitution of the corporation being illiberal—and on other general principles; but by coming to particular allegations as to matters of law, they have enabled their opponents to put in categorical answers, and in most instances with perfect success. The least satisfactory defence is that made, or attempted, in reference to the exclusion of the Licentiates from the Library and Museum—a regulation so palpably indefensible, that the attempt to justify it is rather calculated to excite distrust of the writer's discretion than to convince us of his being in the right.

As we have hinted, the "answers" do not touch the general principle; but they deal with the legal part of the question so convincingly, that we, who profess to hold the balance with an impartial hand, feel called upon to subjoin those clauses which relate to it.

Allegation.—That six physicians were named in the Charter, who, together with all men of the same faculty then resident in London, were constituted one body, commonalty, or perpetual College.

Answer.—Mr. Justice Yates*, in *Rex v. Askew.* "I am far from thinking that all the men of and in London, then practising physic, were incorporated by

* In the first pamphlet the name of Lord Mansfield stands in place of that of "Mr. Justice Yates." An attempt has been made to represent this as a wilful misrepresentation: it is evidently a mere accidental mistake, and is corrected in the *Vindiciæ*.—Ed. Gaz.

the Charter. The immediate grantees under the Charter were the six persons particularly named in it. The rest were to be admitted by them. They were not *ipso facto* made members."

Allegation.—That the perpetuity of the College was to be kept up by the future admission of all men of the same faculty into the College.

Answer.—Mr. Justice Yates, in *Rex* v. *Askew*. "Much less are future practisers of physic of and in London, actually incorporated by the Charter."

And in *Stanger* v. *The President and College of Physicians*, the concurrent testimony of Lord Kenyon, and of all the judges on the bench, confirmed the judgment of Mr. Justice Yates.

Allegation.—That several of the six physicians named in the Charter studied at, and possessed degrees from, foreign Universities; and that no distinction is mentioned as regards the University where a physician may have obtained his degree.

Answer.—When the Charter was granted, learning and science were more successfully cultivated abroad, and especially in Italy, than in this kingdom. It was therefore the practice at the time to resort to foreign Universities, for the purpose of acquiring a higher education than England could then offer, and degrees in physic were often taken in Italy. But it was also the usage for those who possessed foreign degrees in physic, to be incorporated either at Oxford or Cambridge.

Allegation.—That all physicians entitled to practise in London, are equally entitled, under the Charter, to admission to the Fellowship of the College.

Answer.—My Lord Mansfield was of a different opinion, in *Rex* v. *Askew*. Mr. Justice Yates said, "A man is not capable of being admitted into the College without being possessed of certain qualifications which are made requisite. But, granting that he is really possessed of these requisite qualifications, yet his merely being qualified for becoming a member does not make him one. The instrument which gives the licence does not mention any such thing as an admission to be a member of the College. The word 'admissus' is only used in this instrument as a more classical term than permissus. It does not import an actual admission into the College. The Charter, and the Act of Parliament confirming it, make a distinction between the college or corporation and *other men of the same faculty*, '*to govern the said fellowship and commonalty, and all men of the same faculty:* and again, Collegium sive Communitatem prædict' *et omnes homines ejusdem facultatis.*' A good deal has been said about usage; but usage only applies when the construction is doubtful. Here the construction is not doubtful."

Allegation.—Your petitioners are prepared to show that bye-laws have been framed, and long acted upon by the College, which are directly opposed to, and in violation of the letter and meaning of the said Charter.

Answer.—The existing bye-laws have been decided to be legal, and consequently not directly opposed to the Charter, by Lord Chief Justice Kenyon, and by four other Judges of the Court of King's Bench, after solemn hearing in *Stanger* v. *the President and College of Physicians.*"

SIR GILBERT BLANE'S MEDALS.

It is now several years since we announced that Sir Gilbert Blane, in his paternal anxiety to encourage merit in the medical department of the navy, had appropriated a sum of money for the purpose of bestowing handsome medals on the most intelligent and deserving officers. These honourable testimonials were awarded, on the first occasion, to Dr. John Liddell and Dr. W. Donolly; and, on the second, to Mr. Robert P. Hillyar and Dr. J. Wilson: but we regret to say, that the medals themselves have not been forthcoming. We understand, from the best authority, that the delay is in no degree attributable to the distinguished veteran by whom the prize was founded, but that it has arisen entirely from the failure of Pistrucci to fulfil his engagement in regard to the die,—the cutting of which he has postponed, on one pretext or other, from month to month, and year to year. If Pistrucci has been paid in part beforehand, as we believe is usual in such cases, we venture to suggest that a memento from a solicitor might be of service. The

risk of losing some of the golden specimens of his own art which he has received, (for the die of our coinage is cut by him), might stimulate his inventive powers, and prevent the endless procrastination which else seems likely to await the successful candidates for Sir Gilbert's medals.

NORTH LONDON HOSPITAL.

Some of the partizans of the London *University* have been very anxious to represent our exposure of the motives which led to the attempted establishment of an hospital, and of the almost certain failure of the experiment, as resulting from general hostility to the school, and not from a candid consideration of the real circumstances of the case. Instead of entering into any fresh argument upon this head, we subjoin an extract from the Morning Post, which has, in a very forcible and temperate manner, expressed the same views as have been maintained in this journal.

The arguments of the counsel employed to plead against the proposed charter to the London University have most fully shown the absurdity of the demand, and the extreme improbability of his Majesty consenting to the measure of swamping the other schools for the sole purpose of benefiting the proprietors. That any good is to be derived to science is not attempted to be proved; while the real question, and the only one in which the public are concerned, is, whether they are to have well-educated medical men, or whether they are to have forced upon them a set of young men from the London University, who know little of disease and less of its treatment.

The footing on which the existing schools are placed renders them admirably adapted for giving sound medical instruction. One among their numerous points of excellence is the free competition that exists among them, and the entire absence of monopoly. A pupil, when he comes to London, can enter at any school he may prefer, or can divide his attendance among the different schools. At one he may study anatomy; at another medicine; at a third surgery. The laws of the existing Colleges do not compel him to confine himself to any one school; but all the schools open their theatres to him in every branch of science. It is not uncommon for young men to go round and hear the different lecturers before they fix upon their place of study. This freedom of attendance is alike beneficial to the pupil and to the schools. It enables the former to seek instruction where he thinks he can gain the most information in a given time; and it ensures an active emulation among the medical officers of our institutions, by which their charitable purposes are more effectually served. The extent of the competition is seen in the various degrees of reputation enjoyed by different schools at different times. If the professors happen not to be men possessing the confidence of the medical public, and also a high professional character, the interests of the schools are injured, and the attendance of pupils falls off.

The Governors of Hospitals, therefore, have in their schools an additional motive to employ the most efficient physicians and surgeons in their establishments. The free competition among the schools thus becomes a guarantee for the best talent being enlisted in the service of the public hospitals, and affords an unanswerable argument for the union of schools of medicine with our charitable institutions. But these advantages the London University seeks to destroy. It would establish a monopoly of the most injurious nature to the public, under the plea of serving the public interests; it would do away with the characteristic excellence of the metropolitan schools, and substitute in its own favour an exclusive monopoly in the place of a liberal competition.

It is amusingly observed by the collective wisdom of the University, in their Address, that "the only point in which the medical school of the University has been hitherto inferior to other schools is, that it has been unable to furnish the means of hospital attendance and clinical instruction." This admission of their incapacity to teach is made with so much *naiveté*, that we are led to conclude that the medical guardians of the University regard hospital tuition as one of those "accidental circumstances" on which too much stress is laid, and that they purpose to supply the deficiency

rather in compliance with the general feeling, and to prevent any cavilling against their system of education, than as an indispensable auxiliary in the process of medieal instruetion; because, say they, "the medieal opponents of the University insist strongly on the want of a hospital."

The views whieh the University Council entertain in regard to medieal education may be gathered from the reasons they now assign for thus tardily providing a hospital. It is not that the physieians require patients to illustrate the mode of treating disease, or the surgeons cases to show the various operations, or beeause the pupils feel the want of eases to explain the lectures, which are otherwise unintelligible. No; all these reasons, whieh might seem to common minds suffieiently cogent for the erection of a hospital, are thought wholly undeserving of notice. They think that the art of curing disease may be sufficiently communicated to students by description, but consent to get up a hospital in deference to the opponents of the University.

It is, perhaps, the first time that a hospital ean traee its origin and existence to such a motive. Hospitals have been usually considered as asylums for the sick and maimed poor, and have been founded by pious and charitable persons in situations where they were really wanted, and for most praiseworthy purposes. Charity has been hitherto the main purpose in founding such institutions; while the stream of knowledge that issues from them, and would otherwise run to waste, has been turned into the channel of education. Charity has been the primary object; edueation has followed as a necessary eonsequenee. Two purposes equally useful have been thus combined.

The getting up of an experimental hospital is an undertaking that, we believe, nobody but the Counеil of the University would contemplate. It is not difficult to foresee that a hospital erected under sueh eircumstances and with such views, will and must fail in its objeet. We will shortly state our reasons for doubting its answering the purpose for which it is intended.

The first difficulty which the Council has to encounter, is that of raising funds sufficient to provide for the maintenanee of a large number of patients. The University is already greatly in debt. The proprietors have not of late shewn mueh alacrity in adding to their subscriptions; and it is not likely that those persons who subscribe annually to other hospitals should withdraw their subscriptions to transfer them to an experimental hospital. The building whieh they are now erecting is intended to hold only about a hundred beds, or at most a hundred and fifty*. This will be much too small for a school of medicine and surgery; so small a number of patients cannot exhibit to the pupils every variety of disease. A certain number of beds will be filled with patients sent by the subscribers for trifling ailments, who will be of no manner of use in the business of instruetion. The London University sehool will, in fact, be disqualified for giving practieal instruction by the samc cause that prevents the smaller provincial hospitals being useful as schools of medieine — namely, their wards being principally occupied by cases of little or no interest.

The large metropolitan hospitals are differently circumstanced. Largely endowed, and henee not dependent on annual subscribers, their wards are open to all severe cases without recommendation. They not only eontain a larger number, but also a different elass of patients. They are enabled, from the great number of applieants, to selcet the worst cases, and to reject those trifling ones which would oecupy the wards of small hospitals, but which are treated in the large hospitals as out-patients. Out of these again the more important eases are selected for the speeial purpose of elinical instruction. Forty or fifty of the most serious forms of disease may be seen elassed together in one or two wards set apart for the purpose, under the eare of the elinical physieian and his clerks, the latter being chosen from the pupils on account of their experience and attention. This essential department of a medical school must be wholly defieient in a small hospital like that of the University, as it requires an immense body of patients to furnish cases of suffieient importance, and in suffieient number, for the purpose of clinical instruetion.

There are two reasons, independently of the size of the hospital and its want

* This is over-rating the number: it is not calculated, even by the parties themselves, that the hospital could be made to hold more than 130 patients.—Ed. Gaz.

of funds, that will materially operate against its becoming a school of any magnitude. Its locality is one. It is built in a part of the town in which the working classes do not abound. In the neighbourhood of Gower-street there are no manufactories, no wharves, no shipping, no alleys or small streets densely peopled by the lower classes, to furnish the accidents and diseases to be seen in the wards of other hospitals; and for the few poor that reside ample accommodation already exists in the Middlesex, the Fever, and St. Bartholomew's Hospitals. But the most fatal impediment in the way of its success is the antipathy which the poor will naturally feel against it as an experimental hospital. It may be prejudice, but the feeling will not on that account be the less strong; their minds are keenly alive to the purposes of such establishments. They will view the hospital as an institution not built to meet their pressing necessities, but to relieve those of the London University; not established for the relief of their complaints, but remove those of "the medical opponents, who insist strongly upon the want of an hospital;" not to satisfy the claims which the afflicted poor have upon the rich, but to support the claims of the University to a charter; not as an asylum to house the sick and the maimed, but as the last refuge of a needy association.

CLINICAL LECTURE

ON

INJURIES OF THE KNEE-JOINT.

Delivered at the Middlesex Hospital,

By Sir Charles Bell.

The Weekly Board of this hospital has of late furnished to Parliament a list of the patients brought in, suffering from severe accidents. For my own part, I had not a conception of their numbers; and if I mention the subject now, it is to remind you that nothing but a criminal supineness can leave you ignorant of the common occurrences of practice. I cannot better employ the hour than in directing your attention to the principles which guide us in the treatment of these cases of—

ACCIDENTS TO THE KNEE-JOINT.

There is a certain peculiarity of constitution belonging to all the parts which combine to form the knee-joint: as anatomists, you acknowledge this. You know their peculiar texture; and that, consistently with that texture and their offices, they have less vascularity, and a lesser degree of constitutional power, in truth, a weaker vitality. The fascia that covers the knee, the accessory ligaments, the capsule itself, the synovial membrane, the crucial and the mucous ligaments, all have a peculiarity in their character, as well as the cartilages which tip the bones, and the bones themselves. Their vascularity and vitality, as I have just said, are less than those of other parts; and hence it is that they become subject to particular diseases, and are peculiarly liable to strumous or scrofulous action. This is a view of the matter that we must never permit to escape us. Every individual is subject to scrofulous disease; and not man only, but the beasts also. The lion, the tiger, as well as the monkey, brought from southern climates to this country, and exposed to the variations of our climate, become scrofulous, and die of consumption: they are liable to glandular disease, extensive suppuration, and even rickets; so men, apparently of the finest frame are subject, in certain circumstances, to the same class of diseases. The pugilist, called the Game Chicken, of whom you have heard as being the great athletic of modern days, who had a spring like an antelope, and gave a blow with the force of a sledge-hammer, died of consumption. If a young man be subdued of his natural strength by a course of mercury, and if in this condition he receive a blow on the knee-joint, ten to one but it will take on a scrofulous action. It is in this manner that you see younger children subject to serofula after scarlet fever, measles, and small-pox,—whatever subdues the strength of constitution, renders the individual subject to local diseases, which partake more or less of the serofulous character. The knee is more particularly liable to it; and there is no person, however great his constitutional strength, but who, under certain circumstances, may become the victim of this affection.

In all these accidents to the joints, you must not lose sight of this,—that, however necessary it may be to subdue the inflammation, by bleeding, strict confinement, and all the appliances with which you reduce inflammation, there is a point at which you must stop, or the constitutional strength will become so far influenced, that a strumous action will be set up in place of that phlegmonous action, of which you have been justly apprehensive.

The first case to which we will refer is that of a penetrating wound of the knee-joint.

You know what is meant by a penetrating wound. If a boy be climbing over railings, and slips, and a spike runs into his belly, taking a course along the muscles, that is an *external* wound only, and there is no danger; but if the point penetrate the peritoneum in the slightest degree, it is then called a *penetrating* wound; by which the surgeon declares that the lad is subject to peritoneal inflammation, and is in danger of death, not from the extent of the wound, or the violence of inflammation, but from inflammation of the cavity of the abdomen. It very often happens that in such a penetrating wound there is agglutination and union; and, as soon as these take place, all danger is at once removed; and some surgeons, finding this in one or two cases, are at last bold to say that there is no danger at all in a penetrating wound: they ought to limit the observation to that case where adhesion immediately takes place; for if there be no adhesion, there is very great danger from inflammation spreading by "continuous sympathy" over the peritoneum. The same remark applies to the knee-joint. You remember the extent of this joint,—that it is the largest and the most exposed of all the joints; and if you remember how the finger goes round the synovial membrane, from the surface of the tibia, inside the patella, up over the head of the femur, and to the great bursa, under the quadriceps femoris muscle, you have a lively recollection of the extent of the investing membrane, and therefore of the danger of inflammation of it, when the wound remains open, instead of adhering.

There is a point here respecting which it is my duty to put you on your guard. Suppose a workman with the corner of his adze or hatchet strikes his knee, and inflicts a trifling wound, perhaps of only half an inch in length; he considers it of no consequence; he goes about for some days; at length the joint gets stiff; it creaks; there is impeded motion; inflammation has supervened, and now prevents the secretion of synovia, and consequently the joint is not lubricated, so that creaking and impeded motion of it are the first of the bad symptoms. Tumefaction then arises in the knee, swelling in the leg and thigh, and now inflammatory fever rises to an excessive degree, so that in eight or ten days you find the man lying on his back, not in convulsions, but stiffened in the expression of pain; his face flushed, his eyes sparkling, his teeth closed almost spasmodically, and every limb in action, not tetanically, but expressive of the pain he suffers. He is in actual danger, then, from the violence of the inflammation, from the sympathizing condition of his system with a state of inflammation now affecting the limb from the groin to the toe; and wherever there is a deposit of much cellular membrane and fat, there you will find an abscess forming. This may be let out, or it may burst, but tedious hectic then comes on, and very often the patient sinks exhausted with fever and discharge. If he recovers, you have anchylosis in the knee-joint—that is, when adhesion arrests the spreading of the inflammation; or perhaps you must have recourse to amputation, in order to save the life.

We will take the more favourable view of the subject first. A boy, 14 years of age, was admitted last week under Mr. Arnott, with a penetrating wound of the knee-joint. "He is a shoemaker; and while he was at work, a sharp knife which he was using slipped, and ran into the knee, about an inch deep; it entered at the inner part of the knee-joint, just above the patella." You know that the great bursal cavity is situated there under the tendon, and has an open communication with the proper cavity of the joint, so that this was a penetrating wound. "There is a small wound only to be observed; but the knee is inflamed, and attended with considerable effusion, and there is pain on the slightest motion. Leeches and ice to be applied; the wound itself to be covered with plaister and a solution of sealing wax." The next report is, that "the pain and swelling are diminishing, and there are no symptoms indicative of danger. He is kept quiet, enjoined perfect rest, with a splint behind the knee. It is now seven days since the accident, and no bad symptom has appeared." This is a pleasant case to set out with, and I hope your practice will be equally fortunate. Whenever a person has a puncture of the knee-joint, you are alarmed, although he is not; you enjoin rest; you put a splint behind the knee-joint; you adopt all the means in your power to subdue inflammation; but especially you endeavour to close the outward wound; for as soon as the outward wound is closed, all apprehension is over.

There is another case, that of Cooper in Clayton's ward. "He is 18 years of age, and has had a severe wound of the left knee-joint; it was occasioned by his falling upon his scythe. He is a strong lad. The point entered the knee-joint, penetrated the capsule, and, it is supposed, cut the cartilage. The inflammation is already considerable. Leeches have been applied, and cold lotions; but notwithstanding all the means employed, the inflammation continues unabated." The next report says, that "the inflammation involves the whole leg and thigh." Another report is dated some weeks afterwards; it states that "abscesses form in succession in various parts of the thigh and leg, as

well as in the joint, and that he is now reduced extremely low, being very faint, so that poor hopes are entertained of his recovery." It appears, however, that after remaining in this state, in danger of dying from hectic, with night sweats and diarrhœa alternating, the constitution of a fine healthy country lad struggling with the disease, he seemed to rally a little with the assistance of generous diet, wine, porter, and all that could be given him to support his strength. At length slowly and progressively he shewed improvement; the great swelling of the limb subsided, the abscesses closed, and there was a hope of his being dismissed with an anchylosed joint. But then came another mischief, which all are subject to in large hospitals, a relapse, attended with erysipelas involving the leg and thigh, and having a most prejudicial effect on his constitution, and carrying him back we may say three months in the cure. He again recovered; and after being a patient seven months, he was discharged with a stiff joint.

Thus you see that what I stated at first is pretty clearly made out; if you get adhesion, all is right; but if what the vulgar call the *joint oil*, comes out, if the synovia escape, if the instrument penetrates the joint, and the wound remains open, the inflammation spreads from the wound to the synovial membrane and the surrounding surface; then comes excessive pain, extending very slowly, but at last involving the thigh and leg. Thus it was in this lad; the thigh became so large that it seemed to form the greatest part of his body; it was as thick as his waist, and suppuration, as I stated, took place under the quadriceps, and between the gastrocnemius and soleus. Wherever, in short, there is a great depôt of loose cellular membrane, there you find an abscess, just as happens in gun-shot fractures. Looking, then, to these effects, I hope you will suspend your hand in cases of loose cartilages in the joint, and think very seriously before you cut into the knee; because, although the operation is done with your scalpel, and according to the most correct rules, yet if the cut does not unite, you will have all these consequences, and your patient will either die or lose the limb.

Before I made these remarks, I ought to have stated the case of Thomas Hawkins, a lad, aged 17, admitted on the 13th of January, with a gaping wound (this is a case quite in illustration) over the right knee-joint, but not penetrating it. Of course this is a very different case from the last, and in two months the patient goes out perfectly well; neither subdued by hectic, liable to tedious scrofulous disease and abscess, nor with a stiff joint.

Now follows the case to which I more particularly call your attention. A patient comes into the house with a loose cartilage in the knee-joint. Let me remark to you, upon this sort of case, that when you examine a person with a loose cartilage in the knee-joint, you generally hear a history of some former accident, almost forgotten: or perhaps consequent upon that accident, there has been inflammation and lameness, from which, however, he has recovered. After the lapse of months (or it may be years), he is found to have a loose cartilage; the character of which is, that while the patient is walking, or running, he suddenly feels as if a bolt were run between the ends of the bones; he is lame, falls down, and cannot move the joint. This incapacity is attended with pain, of a sickening, subduing kind; not like the pain of an injury from without—not like the pain arising from a common injury. This takes place from time to time, and just when he would desire to be most active he is tripped and thrown down, and remains incapable of motion until by kneading, and, as it were, coaxing the part, he gets the cartilage from between the surfaces of the bones. I remember a young surgeon who was placed in these provoking circumstances. When he was called to a patient, and at the moment when he was about to cross the saddle, the loose cartilage would get between the heads of the femur and tibia. He required to be lifted from his horse; to be placed in bed, and there he remained for some days; and this happened so often that he was precluded from all hope of success in his profession. In such a case, you might think of operating, to extract the loose cartilage; but unless very serious results are consequent on the disease, I would never advise the operation. In that case, the means to be adopted are the employment of an elastic bandage and compress, which will cause the absorption of the effused synovia which attends the presence of this body, whilst it also keeps the loose cartilage confined in its recess, and prevents its getting between the ends of the bones, which it is most apt to do when the capsule is distended with fluid.

The woman to whose case I particularly refer, came into the hospital with a loose cartilage in this joint. She stated, that "she began to feel her knee painful, and something move in it, about three years ago." She said "she obtained no rest day or night, and there was no cessation of the pain; she could walk, but she was subject to occasional extraordinary lameness;" and, in short, "that her suffering was so great that she was willing to undergo any hazard through an operation, with the hopes of relief." The operation was performed; an incision was made on the inside of the patella; two loose cartilages

were withdrawn; severe inflammation very soon followed, with frequent rigors and constitutional disturbance. All the means for subduing inflammation and pain were had recourse to with every possible attention, but the wound did not adhere; inflammation and suppuration commenced in the joint, and extended to the leg and thigh, very much in the manner we have seen it take place in the cases of accidental wounds. There followed the inflammation and suppuration, that exhausting hectic of which I spoke; then came sloughing on the hip—and what does this sloughing of the integuments of the sacrum imply, if it be not that owing to the continued suffering and exhaustion, the patient is kept to one posture by the excess of pain? There were no hopes of saving her life, except by amputation; the operation was performed, and from that time she began to rally, and she left the hospital quite well.

With regard to the operation of extracting these loose cartilages, you perceive why you must go to it with fear and trembling—why you do not perform it unless in the most urgent circumstances. When you do perform it, you must be careful so to manage that there shall be adhesion. How is that to be effected, is the question of interest? The unfortunate occurrence in this poor woman's case, was that of having two loose cartilages to take away. I do not think I could perform the operation neatly, so as to please myself, where there were two cartilages to remove; the reason of which you will perceive if I describe the method. First, you must teach your assistant what he has to do; for the success of the operation is fully as much dependent on the assistant as on the operator. I have a piece of board cut, with a semicircular notch; and by means of this, having got the foreign body on the lateral part of the knee, just on the side of the lower head of the femur, I force it up to the angle where the synovial membrane is reflected. I push the board with the notch firmly against the loose cartilage, holding it, as it were, in a corner at the utmost point of the cavity of the knee. Placing this in the hands of the assistant, he must keep it steadily in that position, for if he does not, the cartilage will escape; and if it escape, misery follows. The next thing that you have to consider is, that you are not to cut directly down, because you have to provide for an oblique wound, that the lips may more certainly heal, and therefore you draw the integuments a little aside; and having done that, you then cut down on the cartilage. I say, then, having drawn the integuments aside, you carry your knife (sharp as a razor) across, very lightly, and cut with a perpendicular edge, taking care to avoid pressure; for if you press on the cartilage, it may escape. At last you see nothing but the cobweb of the proper capsule; you continue to draw the knife over it, and out starts the loose cartilage. But sometimes I have done thus: having made the incision through the integuments, I take a strong and straight couching-needle, and strike it through the thin membrane, direct on the cartilage, and so hold it firm; and then, drawing the knife by the side of the point of the needle, I bring out the cartilage on the point of the needle, which makes a pretty operation. If the body escapes after you have made the incision, and the synovia has come out, and you have to bend the knee to get hold of the cartilage, or if you have to put in a pair of forceps to catch it, or having adroitly brought out one, there is a second, as there was in this case, it is ten chances to one that you cannot remove it with so slight and oblique an incision as I have described; and then all the mischief that I have dwelt upon is too apt to result. You would hardly believe that a cut made with so clean an instrument should produce this mischief, yet the well-informed student knows that it must be so, because, although the operation be done according to rule, and is one of the admitted or regular operations, still it is a penetrating wound; and if adhesion does not follow, misery and suffering result—the loss of the limb or the loss of life.

There is another case, but we do not require a regularly drawn up account of it. You may see the lad lying in the ward, and take a note of it yourselves. The circumstances are these:—He is a young fellow, who acknowledges being drunk. At the time of the accident, he was thrown down, or was lying down, and trying to get up, when a heavy fellow fell upon him, with his knee pressing against, or rather falling upon, the outside of the thigh, in such a manner as to push the knee in and burst the lateral ligament of the knee-joint. I would ask you, as anatomists attending the dissecting-room, whether, when the knee is thus broken down, as it were, and the internal lateral ligament is lacerated, the crucial ligament must not also be sprained; for so long as the crucial ligament is entire the bones are bound together? Yet, on the other hand, if the knee be a little bent, the pressure may come altogether to bear on the inner lateral ligament, which, you know, is not very strong. Now, then, observe the treatment in this case. The limb is bound up; every means are taken to subdue inflammation: a splint is put on the outside of the knee, so as to support the joint and allow the ligament to unite.

I shall take this opportunity of reminding you of the nature of these ligaments. A ligament, in its natural condition, is white and tense, and not very vascular; but when it is injured, and has become the seat of inflammation, it changes its character; it becomes of a grey colour, and quite vascular, compared to what it is in the natural condition. It is now no longer an inelastic rope, binding the ends of the bones; it has become loose in its texture. That is exactly what takes place here in this accident to the lateral ligament. The accident, for the most part, happens thus: an old lady descending from her carriage, or say an old woman coming down stairs, thinks to put her foot on the pavement or the floor; she is mistaken, she has a step farther to descend, and she comes down with a jolt, with all her weight. Now you remember that the femur is oblique both in men and women, but particularly in the latter (which is the reason why the accident most frequently takes place in women.) The effect of this obliquity is, that when the weight falls thus, there is a jar upon the inner lateral ligament; and when this has taken place, it is apt to happen again and again in the same knee, for the ligament becomes so loose in its texture, that it yields more and more easily each successive sprain, and the knee falls inwards, the obliquity of the thigh-bone is increased, and therefore the accident occurss more frequently, and from less and less violence, till at last the patient cannot step at all. The obliquity of the bone, and the weakness of the ligament, are such, that she cannot bear her weight upon the limb. If, then, you are sent for to an old lady, and she tells you that coming out of the carriage she sprained her knee, you know that it is most likely the inner ligament which is hurt; and in examining the knee, you put your finger or thumb on that ligament, and ask whether the pain is not there. She replies, "it is;" and it shews her at once that you are acquainted with the case.

Recollect that while you employ means to subdue the inflammation, you must be careful in your mechanical apparatus to prevent a succession of injuries, which may destroy the natural protections of the joint. The accident which has occurred to this young man is of a different nature, so far as there being more injury done, and perhaps more care being necessary, he will be secured against the effects which too often follow the slighter jars of this lateral ligament.

DR. RIGBY'S

MIDWIFERY HOSPITAL REPORTS.

GENERAL LYING-IN HOSPITAL.

[Continued from p. 188.]

DYSTOCEA PELVICA.

During 1833, two cases of perforation occurred in the hospital, and in both instances proved fatal from other causes; the one in May, from abdominal inflammation, the other in August, from an attack resembling cholera. I mention the date, because it was when the nights were oppressively hot, and when the general state of the weather had a very unfavourable effect on the healthiness of the patients. I could have wished to have given these two cases at length; but not having been permitted to make a post-mortem examination, they present but comparatively little interest.

Two cases of dystocea pelvica have been treated by inducing artificial premature labour, and in both cases with a successful result.

May 14th, 1833.—Sarah Lambert, æt. 31; second pregnancy; perforation required in her former labour, on account of the projecting promontorium sacri, which can be easily reached by the finger. Menstruated last on the 9th of September. In about the 26th week of pregnancy, Dr. Ley and myself ascertained the defective length of the antero-posterior diameter, which is apparently $2\frac{3}{4}$ inches at the utmost. Artificial premature labour was decided upon, and she returned to the hospital on May 6th, when, according to her reckoning, she was in her 31st week.

It was determined to attempt exciting the uterine contractions, if possible, without previously rupturing the membranes; and, accordingly, after having thoroughly cleared the bowels by castor oil, 3ss. of secale cornutum was given on the evening of the 9th, which was twice repeated at intervals of two hours. During the night, moderate pains, like the first commencement of labour (according to her own description), came on, and continued regularly till morning, but nearly disappeared towards the afternoon (May 10.) To recal the uterine contractions more effectually, fifteen grains of ext. coloc. comp. were given about six in the evening; and in two hours after the secale was given, and twice repeated, as before. Brisk pains followed; and about three in the morning (May 11) she felt much griped, and vomited; they continued tolerably active all the night, but towards noon diminished considerably.

At four P.M. the os uteri was not sufficiently open to admit the finger, the cervix being still very long. Towards night the pains had almost entirely disappeared.

May 12th.—Dr. Ley pierced the membranes, and instantly withdrew the stilette, to prevent further discharge of liquor amnii, very little of which escaped during the rest of the day.

13th.—Towards evening pains began to come on, and much liquor amnii dribbled away.

14th. — Pains regular, and tolerably strong; os uteri barely open enough to admit the finger; cervix still long; head can be felt loose, and resting on the superior aperture. Towards the middle of the day the pains had considerably increased in power; liq. amnii still escapes; bowels not open since yesterday; the pains are chiefly in the lower part of abdomen, and do not appear to act upon the os uteri, but run down each thigh. As they did not appear to have the character of genuine labour pains, and exhausted her to little purpose, I gave her ten grains of Dover's powder, after which they ceased for about two hours. They now returned much more efficaciously, being chiefly in the loins; the os uteri gradually dilated; and at 3½ P.M. she was delivered of a living girl. The head was much flattened, and the right temple shewed a deep indentation, from having been pressed so long against the promontorium sacri. The child was well formed, and what would be considered as having been carried eight months. The placenta, &c. came away easily, and no hæmorrhage followed.

15th.—Feels comfortable; nipples are so little prominent, that the child has refused to draw them, but sucked well from another patient.

16th.—Bowels opened by castor oil; and a small quantity was given to the child. It sucks well, but not its mother's breast.

18th.—Breasts beginning to swell, and now the child will take her breast; it thrives and looks healthy.

The attempt to excite uterine action without previously rupturing the membranes, was done at my request, being a practice which has been very successfully followed by Naegelé. It seems reasonable to suppose, that the closer we can imitate a natural labour in every respect, the better will be our chance of success; and it is probable that many of the unsuccessful cases of artificial premature labour have been in consequence of allowing the liquor amnii to drain off entirely. The pressure of the uterus has been therefore exerted upon the child itself, which at this early age was much too delicate to bear it without injury. This was to a degree avoided by my colleague Dr. Ley instantly withdrawing the stilette as soon as he had punctured the membranes. As soon, therefore, as a little liquor amnii had escaped, they collapsed, and thus closed the opening. Dr. L. has been remarkably successful in cases of artificial premature labour, having in public and private practice successfully treated, during 1833, no fewer than five or six cases.

The other case, the particulars of which I need not relate at length, was a short woman, not deformed to outward appearance, and who had been delivered several times before in the same manner, but all the children were either born dead, or died shortly after birth. Dr. Ley ruptured the membranes when, according to her reckoning, she was seven months and six days advanced in her pregnancy. Pains came on in forty-eight hours after, and a living, although weakly child, was born, which, though it required a great deal of care at first, eventually throve well.

I now come to those cases of dystocca where labour was rendered tedious or difficult from the powers destined for the expulsion of the fœtus being insufficient, or otherwise faulty. Four cases occurred where this was more or less evident; one was a tedious first labour, the pains having been rendered irregular and ineffective from a state of general plethora, and having assumed that spasmodic character called false pains. In the other three, the application of the forceps became necessary.

May, 17, 1833.—Mary Kennedy, æt. 18, delivered of a girl; first child; first position; a robust, weather-beaten, Irish milk woman; florid complexion.

15th.—Bowels have been opened by castor oil; has been complaining of pains for some little time; they are at the lower part of the abdomen—not at all in the back. She had no pains when I saw her. Per vaginam I could feel the head presenting through the inferior segment of the uterus, and could just reach the os uteri, which was quite closed, and high up in the hollow of the sacrum. These pains being evidently spurious, I ordered her 15 minims of liq. opii sid. in aq. menth. pip.

16th.—Passed the night comfortably; pains returned, still in the lower part of the abdomen. Os uteri can be reached with difficulty, scarcely dilated to the size of a sixpence: edge very thin. Pains produce much suffering. Pulse full. vs. ad ℥xvj.

℞. Liq. Opii Sed. ℥xx.; Ol. Ricini Aquæ Menth. pip. aa ℥ss. ft. haust. stat. sumendus.

The blood was unusually dark (five o'clock P.M.); the pains did not disappear entirely, but their character changed im-

mediately, being now situated in the loins, leaving the abdomen quite free. The os uteri began to dilate, its edge to swell, so that by three o'clock the next morning (May 17th), it had entirely disappeared. The bag of membranes projected nearly between the labia. I therefore ruptured them, having previously ascertained that the posterior fontanelle was towards the left foramen ovale. The pains now increased considerably; the head came slowly upon the perinæum, and distended it so gradually, that scarcely any rupture was produced, although supported by a midwife pupil. The child was some time before the shoulders passed; but as a smart hæmorrhage came on, I assisted it a little, and the rest of the child soon followed. The hæmorrhage ceased, and in the course of ten minutes the placenta was expelled.

The thickness or thinness of the os uteri during labour is an excellent means of forming some idea as to its duration, and has been strangely misinterpreted by authors and teachers of midwifery. It has been considered, that the os uteri does *not* dilate until its edge has become thin; but this is very far from being the case, as it occurs in nature. The thin os uteri never dilates, nor will it open until its edge has become thick, soft, and cushiony. This fact is strikingly demonstrated in the above case, and is of considerable value in determining how far a venesection, &c. has been efficacious, by inducing genuine labour pains and disposing the os uteri to dilate. In cases where there is much spasmodic pain, whether before or after labour, accompanied by any vascular excitement, the liq. opii sed. is a form decidedly preferable to the common opium; it is not only safer, on account of being less apt to stimulate the heart and render the pulse sharp, but is much more certain in producing its sedative effects.

Oct. 1st, 1833.—Esther Collins, æt. 36, delivered of a boy; first child. Has had one miscarriage. Came into the hospital on the 13th of August with false pains; reckons that she has gone her full time. Can hear the fœtal pulse a little to the left, and below the umbilicus—132 in the minute, though not sufficiently distinct for me to hear the double pulsations. Pulse of the mother 68. Can hear the placental circulation no where, unless it be a very indistinct pulsation in the left hypogastrium. These spurious pains were removed, and she left the hospital.

Sept. 27th.—Returned with pains, which were evidently spurious, having no effect whatever upon the os uteri. Says that she has not felt the motion of the child, but it rolls over like a heavy weight as she turns from side to side. Upon examining with the stethoscope, I could hear the fœtal heart beating 128 in the minute, a little to the right of the umbilicus, but nothing of the placenta. She took 10 grains of Dover's powder at night, which relieved the pains.

28th.—Towards evening the false pains returned, and she had a draught of ℿxx. of liq. opii sed. and ℿxv. of liq. antim. tart. and the pains were again relieved.

30th.—Pains have begun to affect the os uteri, which is open to the size of a shilling. Head presents, and I can feel a fontanelle towards the left foramen ovale, but cannot distinguish which it is; suspect it to be the great fontanelle, as one suture runs almost directly backwards. Towards the afternoon the pains again assumed the character of spurious ones; and as the vagina was hot and dry, I bled her to ʒxij. The pains quickly became more active and effectual; the os uteri dilated, and the head advanced near to the perineum, but here it remained; nor did the pains produce any effect upon it. The liquor amnii has been dribbling all to-day and yesterday. Towards midnight the pains diminished; and at one A.M. I applied the forceps, and extracted rather a small head; the rest of the labour followed naturally.

Oct. 1st.—She complained towards evening of smart uterine pain; the tongue was dry, and other symptoms of uterine phlebitis. Ten grains of calomel and four grains of antimonial powder were given, and a hot poultice of linseed meal applied to the abdomen. In an hour afterwards she vomited; but as the bowels acted well, and produced some copious bilious evacuations, a considerable portion of the powder must have been retained.

2d.—Has felt free from pain, and comfortable; during the night had a shivering fit and return of the uterine pain. A fresh poultice was applied, and a dose of carb. and sulph. magnesiæ given, which produced two or three evacuations during the night.

3d.—Had a fresh return of pain in the afternoon, coming on spasmodically; pale, weak, and slightly hysterical; tongue dry, but pulse natural. Four grains of calomel and eight grains of Dover's powder were given, and several feculent evacuations followed. The poultice to the abdomen was renewed.

4th.—Passed a firm coagulum, with some portions of membrane, in the morning. Bowels continually open, with much tenesmus; stools consisting of flakes of lymph, like chopped membrane, floating in a reddish watery menstruum. Very weak; tongue dry and brown; feels pain of abdomen when she moves. Hyd. c. creta et sodæ carb. aa gr. iv. were given every two hours for three times.

5th.—Much pain of abdomen. Eigh-

teen leeches were applied, and hyd. c. creta and sodæ carb. aa gr. iv. repeated. Mouth becoming sore; and in the afternoon a quantity of healthy bilious fæces was floating upon the evacuation. Feels better.

6th.—Has felt comfortable during the night. Bowels open three times; healthy evacuations.

I should not have given this case at full length, had not every part of it been replete with interest. The auscultations which were made when she first came into the hospital give a tolerably good view how far this mode of diagnosis may be depended upon. The placental sound, to my mind, is *not* a certain sign—firstly, because its absence cannot be considered to denote the child's death; and, secondly, because as yet I know of no certain diagnostic between the bruit placentaire and that of the iliac and other arteries. The common situation of the placenta is at the back part of the fundus; and therefore in many cases it will be impossible to hear the rush of its circulation. And with regard to the second objection, I have repeatedly heard a deep whizzing pulsation in each hypogastrium, which, from the result of the labour, must have been produced by the iliacs. Auscultators of more tact and experience than myself may (and perhaps justly), attribute my second objection to want of practice with the stethoscope; but the strength of the patient's circulation, the thickness of the abdominal parietes, the size of the uterus, &c. are so variable, that I cannot place much dependence upon the placental sound. On the other hand, the being able to hear the fœtal circulation is a very valuable addition to our means of diagnosis; not only for deciding as to the existence of pregnancy, but also for ascertaining, before labour, whether the child be living or not. In the above-mentioned case, it was very useful in preventing me from being misled by a symptom of the child's death, which in most cases is very certain, viz. the sensation of the rolling about of a heavy weight in the abdomen as the patient turns from one side to the other. I have once before seen this symptom, where the result of the labour proved the child to be alive, and cannot account for it.

The whole subject of dystocea from a faulty state of the powers destined for the expulsion of the fœtus, is one of great importance to the accoucheur; and the different causes from which it may arise have not been sufficiently noticed in this country. The works of Wigand, Boer, Von Herder, Schmittmüller, &c. are comparatively but little known, and contain much valuable information upon this subject.

The attack which followed her labour was rather an unusual form of puerperal disease: it was an inflamed state of the mucous membrane lining the intestines, which appeared to have taken upon itself that secretion of coagulable lymph which is frequently effused by the serous membranes in inflammation at this period. The liver had entirely ceased to act, the intestinal contents appeared to consist merely of those little flakes of coagulable lymph in a reddish serum; and, with a view to restore the biliary secretion, and thus supply the intestinal canal with its natural stimulus, I kept up such an action upon the system with hyd. c. cretâ, as should induce slight salivation.

It will perhaps appear a rough sort of practice giving ten grains of calomel at a dose. I do it in these cases for two reasons—first, because small and repeated doses of calomel will not produce the same effects; and, secondly, because in the end much less is usually taken, when thus administered, than when given in doses of two or three grains. In cases of uterine phlebitis, which was the form under which the first part of her attack appeared, salivation is neither necessary nor desirable: violent depletion by venesection and purgatives is not our object. This must be to stimulate every excreting organ to general increased action; and I know of no remedy capable of exerting these effects except an eight or ten-grain dose of calomel. Every excretion is excited — the liver, the mucous membrane of the intestines, the kidneys, the skin, are all roused to a state of activity; and I cannot but attribute the return of lochia which frequently follows a dose of this sort to a similar action exerted upon the uterus. The enteritic affection which followed, required the exhibition of mercury in a very different manner; and as soon as the state of the gums shewed that it had affected the system, an abatement of the symptoms immediately followed.

——, ——, æt. 22.—Oct. 6th, 1833, out-patient; second pregnancy; second position.

An unhealthy-looking woman, whose cast of features so resembled those of a deformed person, that at first I suspected she was so; was delivered with the forceps in her last labour. Pains came on yesterday, and they have ceased almost entirely for two or three hours. Head at the inferior aperture; posterior fontanelle forwards and to the right, but not bearing on the perineum. Passages cool and moist; bowels confined. Introduced the forceps with remarkable ease, and extracted the head, which was very large; swelling of the cranial integuments on the left side. The child immediately shewed signs of life, and the rest of the body followed soon

after. No hæmorrhage. A dose of ergot of rye was given immediately after the birth of the child, and the placenta came down easily.

It might be asked, why was the secale cornutum not used to expedite labour in this case? I would answer, because not only were the situation of the head and state of the passages most favourable for applying the forceps, but also, from the weakly and unhealthy look of the patient, and the long cessation of the pains, I did not expect that the uterus would be sufficiently roused by a dose of this medicine. The result proved that I was right; for when the child was born, she declared that she would much sooner have me apply the forceps than endure one good labour pain. The ergot which was given after the birth of the child was to ensure uterine contraction and prevent hæmorrhage.

[To be continued.]

EFFICACY OF CUBEBS.

Dr. Fenoglio, of Turin, from a number of cases which he has had under his care, and which are published in the *Annali Universali*, has drawn inferences highly favourable to the superior efficacy of cubebs in blennorrhœa. He considers this medicine as almost a specific, and is positive that its use is attended with no ill consequences. The dose he employs is three scruples a-day, administered in conserve of roses; and, as a general fact, he states that from two to four ounces of cubebs, taken for a period varying from two to four weeks, are sufficient to cure the disorder.

MEDICAL STATISTICS—MR. BROUGHAM'S BILL.

We are glad to find that a legislative measure, such as that which we recently recommended, is now actually in progress: we allude to Mr. Brougham's Bill for the better registration of births, marriages, and deaths, throughout the kingdom. Though only collaterally connected with the subject, such a bill cannot but greatly promote the advancement of medical statistics. The expenses of the working of the machinery, which is to be entirely of a *civil* nature, are estimated at somewhat under 70,000*l.* per annum, which will not constitute an annual tax of more than 5*d.* on each family throughout the land.

NEW MEDICAL WORKS.

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WEEKLY ACCOUNT of BURIALS,

From Bills of Mortality, *May* 13, 1834.

Abscess	1	Hooping-Cough	16
Age and Debility	54	Inflammation	45
Apoplexy	9	Bowels & Stomach	4
Asthma	17	Brain	7
Cancer	1	Lungs and Pleura	7
Childbirth	6	Insanity	12
Consumption	93	Jaundice	1
Constipation of the Bowels	1	Liver, diseased	4
Convulsions	34	Measles	9
Croup	4	Mortification	9
Dentition or Teething	8	Paralysis	2
Dropsy	20	Rheumatism	5
Dropsy on the Brain	9	Scrofula	2
Dropsy on the Chest	2	Small-Pox	11
Fever	10	Spasms	1
Fever, Intermittent, or Ague	6	Stone and Gravel	1
Fever, Scarlet	2	Thrush	6
Hæmorrhage	1	Unknown Causes	5
Heart, diseased	1	Stillborn	23

Increase of Burials, as compared with the preceding week . . . 171

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

May, 1834.	Thermometer.		Barometer.	
Thursday . 1	from 47	to 65	29·60	to 29 65
Friday . . 2	48	67	29·67	29 69
Saturday . 3	47	67	29·72	29 78
Sunday . . 4	47	69	29·83	29·89
Monday . . 5	45	65	29·96	29 91
Tuesday . . 6	49	66	30 05	30 24
Wednesday 7	47	71	30·31	30·26

Wind variable, S. W. prevailing.

Rain frequent during the 5th, otherwise generally clear.

Rain fallen, ·475 of an inch.

Thursday . 8	from 50	to 75	30·19	to 30·14
Friday. . . 9	42	70	30·07	30·01
Saturday . 10	46	70	29·96	29·92
Sunday . . 11	49	68	29·85	29·79
Monday . . 12	51	63	29·70	29·63
Tuesday . 13	49	63	29·60	29·59
Wednesday 14	41	65	29 63	29·77

Prevailing wind, S.W.

Frequent showers during the three last days, otherwise generally clear.

Rain fallen, ·525 of an inch.

Charles Henry Adams.

NOTICES.

Mr. T.'s note shall be attended to.

The Plan of Medical Reform which has been sent us is under consideration. Its length is against it, unless its details be particularly good.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MAY 24, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XXXIII.

DIFFICULT LABOUR.—INSTRUMENTS.

APPLICATION OF THE FORCEPS.

The os uteri must be dilated, and the head in the pelvis, before the short forceps can be applied.—Before the short forceps can be applied, the os uteri must be entirely dilated, and the head must have come down into the pelvis sufficiently low to enable us to feel one or both ears distinctly. You will plainly perceive that the instrument can neither be introduced without difficulty, nor worked without danger, unless the mouth of the womb be fully opened; and you will equally understand, that it is necessary to touch one or both ears, because they become the guide to the proper adaptation of the blades. To employ the instrument with advantage, then, the exact position of the child's head must be accurately made out; and this we learn by paying attention to the situation of the ears as regards the pelvis, and to the irregularities in their form. We know that the back part of the organ,—the helix, or flap,—is free and unattached, and looks towards the occiput; while the tragus is bound more closely down, and is directed towards the face. Thus the position of the ear, in respect to the pelvic cavity, informs us whether the head has made its turn; and the direction of the different points of the organ itself, determines whether the face is placed backwards or forwards.

As soon as a necessity for instrumental interference appears, two questions of some importance will naturally offer themselves to our mind: the first is, whether we shall call in the assistance of another practitioner, to advise us by his counsel, to aid us in the operation, and to divide with us the responsibility of the case; and the second, whether we shall apprise the patient of the necessity of such help, and obtain her sanction and approval. So far as the first question is concerned, narrow policy might perhaps whisper to us, that we should not unnecessarily throw our characters into the hands of a neighbouring, probably a rival, and perhaps not very friendly, practitioner. We may be led to argue, that we are giving him an undue superiority; that he may, perhaps, be tempted to take advantage of the confidence we repose in him, to worm himself into the good graces of our patient; that he may blazon it abroad he was consulted in a case so difficult, that we were incompetent to its management; and that to his judgment and dexterity the safety of the patient was to be attributed. A selfish and narrow-minded feeling might prompt us to reason thus; but I should hope there are few men in the profession (notwithstanding the vituperations continually hurled at the general body by some portion of the medical press), who would be guilty of such a breach of professional etiquette, as implied in this suspicion.

But let us even look at the darkest point of the picture: we will suppose it probable that the person we consult will take advantage of our confidence, and endeavour to supplant us by specious misrepresentation: still I would recommend you to act on the same principle; and, strong in your own acquirements, in the integrity of your intentions, and the propriety of your conduct, to disregard the ill-natured aspersions which envy or malice may circulate to your discredit: for there is such a comfort in the division of responsibility,

such a consolation in knowing, if the case turns out ill, that you have not acted entirely on your own judgment, but that another party has sanctioned the means employed, and that all has been done which foresight could suggest; that you would be unnecessarily adding to your own anxiety if you allowed any petty jealousy to prevent your availing yourselves of the opportunity offered;—provided, indeed, the loss of time which must elapse would not endanger the woman's safety.

The second question will be more easily disposed of. I presume that no operation in what is miscalled *pure surgery*, is undertaken without the concurrence of the patient; and I do not know why we should place the obstetric branch of the science on a different footing in this repect from surgery in general. Many reasons would induce us to inform our patient of the necessity of relief being afforded her, and the propriety of the means we are about to adopt. If instruments are had recourse to surreptitiously, they must be employed at a great disadvantage; since we cannot, under these circumstances, direct the position and general management of the patient with sufficient accuracy: again, should it be subsequently discovered that artificial delivery has been practised, it will with great reason be presumed that the instruments were used for our own convenience, and not for her benefit; and should an unfavourable termination occur, we shall be most justly censured. Independently of these reasons, we have no object in concealing our intentions; for we generally find her quite ready to submit to our opinion, resigned to the necessity of the operation, and most willing to avail herself of those means of relief which we have it in our power to apply. Nay, more; we shall often find it more difficult to resist the importunate entreaties both of the woman and her friends to terminate the case, than to persuade them of the necessity, when that necessity exists.

Having, then, called in the advice and assistance of a medical friend, having concluded with him that the patient's safety requires that instrumental delivery should be had recourse to, and that the case is fitted for the use of the forceps; and having obtained her sanction, let us sit down calmly and quietly by the bed-side, and determine most correctly the position of the head, if we have not learned it before.

Eight situations of the head.—There are eight situations of the head under which the forceps are available. The first is, where it has fully made its turn, with the face into the hollow of the sacrum, the occiput lying behind the symphisis pubis, or impinging on the upper margin of the arch, with the right ear towards the right ilium, and the left ear to the left side,—offering itself, indeed, at the outlet of the pelvis, in the position most favourable for its exit. The second is, where the head has passed the brim, and come down into the pelvis diagonally, with the face towards the right sacro-iliac synchondrosis, the occiput to the left groin, the right ear under the right groin, and the left ear before the left sacro-iliac synchondrosis. The third is, where the head offers itself just in an opposite direction to the last, with the face looking backwards to the left sacro-iliac synchondrosis, the occiput forwards behind the right groin, the right ear against the right sacro-iliac synchondrosis, and the left ear behind the left groin. The fourth is with the face looking directly towards the right ilium, the occiput to the left, the right ear behind the pubes, the left ear against the hollow of the sacrum. The fifth with the face to the left ilium, the occiput to the right, the left ear behind the pubes, and the right looking towards the hollow of the sacrum. The sixth is, where the face has offered itself anteriorly, has passed down diagonally, looking to one or other groin, and has eventually been thrown behind the symphisis pubis, the occiput having turned into the hollow of the sacrum, the right ear looking towards the left ilium, and the left ear towards the right ilium. The seventh case is where the head has also cleared the brim, with the face directed forward, but where the turn just described has not taken place, the face looking to the right groin, the occiput to the left sacro-iliac synchondrosis, the right ear to the left groin, and the left ear to the right sacro-iliac synchondrosis. The eighth and last case is just the reverse of this again—namely, where the face comes down to the left groin, the occiput to the right sacro-iliac synchondrosis, the right ear towards the left sacro-iliac synchondrosis, and the left ear behind the right groin. In the two last situations the natural inclination of the head is to turn, with the face under the arch of the pubes.

When the head is placed in any one of these situations, and the symptoms require it, we feel ourselves warranted in attempting to deliver by the short forceps—provided the os uteri is fully dilated—if we can feel an ear distinctly, if there is sufficient space in the bony passages for the head to emerge, and if the soft parts are sufficiently dilated to admit of its exit without suffering serious injury.

Before the forceps are introduced, the state of the bladder and rectum must be particularly attended to. Whether urine is detected by the hand or not, a catheter should be introduced, that we may assure ourselves of the organ being perfectly

empty; and if any difficulty occurs in the insertion of the common instrument, a flexible tube should be employed. We must also ascertain that the rectum be not loaded with fæces; and if so, it may be relieved by a simple enema. It is not so necessary to insist on clearing out the bowel as on the complete evacuation of the bladder; and, indeed, when the child's head is fully occupying the pelvic cavity, the stricture produced by it is so great, that it is with extreme difficulty a clyster can be thrown up; and even when injected, the fluid only partially returns; so that we shall generally be foiled in our intention of emptying the lower intestines. It is my practice *always* to introduce the catheter, but not to administer an enema unless an accumulation of fæces in the rectum be evident to the finger.

Mode of applying the forceps.—We will take the most easy case first, as illustrative of the mode in which the forceps are to be applied: assuming that the face is in the hollow of the sacrum, the vertex presenting, and the perineum somewhat distended.

It is not necessary that we should accoutre ourselves in any particular dress, or even take off our coat, for this operation; but it is desirable that we should turn up our coat sleeve, unbutton the wristband of our shirt, and free the fore-arm as much possible from any ligature which dress might produce.

Having warmed the instrument, by placing it in a basin of hot water, so as to bring it as nearly as possible to the temperature of the woman's body, it must be greased with a little lard or other simple unctuous substance, and introduced as I now show you; the patient being placed in the most convenient situation. It is a matter of great consequence for the success of our operation, that we should put the patient in a proper position,—for many failures have occurred for want of this very simple caution.

Still lying, then, in the common obstetric position, namely, on her left side; she must be brought so close to the edge of the bed, that the nates may project somewhat over, the knees must be drawn up towards the abdomen, and the feet placed against the bedpost, or supported by an assistant. The object in bringing the patient so near the edge is, that the handle of that instrument applied over the uppermost ear, may be lowered, and its point easily introduced. If we attempt to operate while she remains in the middle of the bed, it will be impossible to depress the handle sufficiently; and the point cannot be introduced unless the blade be carried up within the sacrum, and then turned forwards over the ear; by which a circular sweep of a portion of the pelvis is made, and the maternal structures might be endangered. It is to prevent the necessity of removing the patient at all, that some practitioners have adapted a hinge in the shank, and others prefer a handle attached to the blade by a screw. The woman being placed in the position described by the machine, two fingers of the left hand, previously anointed, must be carried over the uppermost ear, which is generally the one most easily distinguishable. One blade of the instrument is then to be taken in the right hand; being gently poised between three fingers and the thumb, its handle must be lowered, so that the point may slip up towards the pelvic brim, between the fingers and the head, (being directed over the ear by the fingers, which act as the guide,) and insinuated upwards by a gently waving or wriggling kind of motion. In the introduction the point must be kept constantly in contact with the fœtal head: the attempt must be made in the interval of pain, and desisted from should uterine contraction occur. On its being so fully introduced, that the groove for the lock projects slightly beyond the external parts, it must be preserved in that situation by the little finger and thumb of the left hand, or by an assistant, and we must proceed to pass up the second. This must be introduced in a similar manner to the first, being directed on the fingers previously inserted; and if they are both properly applied, the groove of one blade will fall into the groove of the other, so that they will lock together without difficulty or exertion; and nothing is left us to do but make extraction. If it should happen, however, as will often be the case, that when the blades are both introduced, they are not perfectly opposite to each other, and consequently do not lock easily, we must not wrench them round, in order to make them fit,—for by so doing we shall bruise the woman's parts,—but we must withdraw the one last introduced, and pass it up in a different direction. We had better introduce it two or three times than lock the blades by force.

Another point to be attended to in the application of the instrument is, that we should take care to introduce the blades, so that the grooves to form the lock should be internal in respect to each other,—for if this be overlooked, it is impossible to fix them, unless each handle be wrenched completely round, or one be withdrawn. On closing the lock, we must be particular that none of the soft parts be pinched, and especially that none of the hairs are entangled within the grooves.

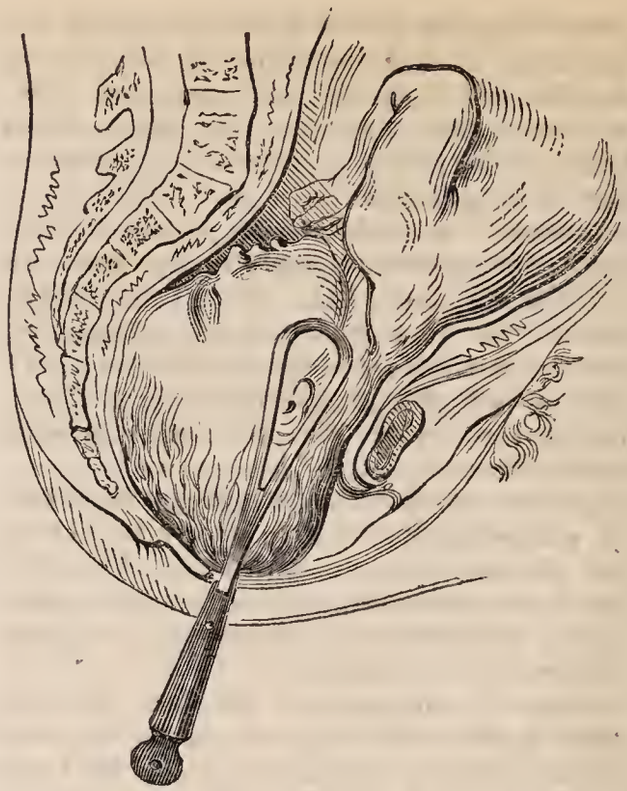
Our next indication is to extract; and we must do this with a regular, slow, waving, pendulum-like sweep from handle to handle, keeping the instrument back to

the perineum as closely as we can. We extract with the right hand, while we support the perineum with the left. If there be pains, we take advantage of them, and act while they continue, resting in the interval of uterine contraction; and we then relieve the child's head from pressure, by opening the lock. If there be no pains, we imitate nature, by working for two or three minutes together, and then relax in our exertions for the same period, taking care during the interval to guard the lock by the fingers, so that the blades shall not slip. In the course of a short time, we shall find that the head makes some advance,—that the perineum becomes more distended, and at last the vertex will appear externally. The direction of our power is then to be in some degree changed, and we must follow the axis of the pelvic outlet. We no longer keep the handles close to the perineum, but turn them rather forwards, and upward towards the abdomen; and, by a continuance of the same pendulum kind of action, the forehead will emerge, and eventually the face and chin; during the passage of which, the perineum will demand our especial protection.

In most of Smellie's plates you may observe the handles of the forceps tied together by a tape; and this practice is still adopted by many. I disapprove of such a ligature, because the hand possesses quite sufficient power to make the requisite compression; and because, if the pressure be continued uninterruptedly, the child's life must be placed in great jeopardy; and for these reasons I never myself employ it.

Cautiously and tenderly must this iron instrument be used. We must recollect that no sensation can be communicated to the operator's hand of any injury that may be done to the woman; and we must remember that one injudicious thrust, one forcible attempt at introduction, one violent effort in extraction, may bruise, may lacerate, may destroy. Bearing in mind, however, the kind of case in which it is useful and admissible;—bearing in mind the principle on which it ought to be employed;—recollecting that it is a lever of the first kind;—that the iron blades have no feeling, and cannot communicate to our sensations a knowledge of the injury we may do, we are not likely to fall into any grave error in its application or its use.

Mode of applying the forceps, when the head has entered the pelvis, before making its turn.—But it is not always in the kind of case of which I have been speaking that we are compelled to apply the forceps; we may have to use them when the head has entered the pelvis, before it has made its turn with the face into the hollow of the sacrum,



The annexed cut represents the forceps applied when the head has made its turn, with the face into the hollow of the sacrum. The tumefaction of the scalp at the vertex, owing to the collapse of the bones from pressure, is very well defined.

—when it is lying diagonally with the face to the right or left sacro-iliac synchondrosis, or laterally to the right or left ilium. It is very evident that in this case, although the head may be sufficiently low to enable us easily to embrace the ear, still it cannot be expelled or extracted, until it turns into a more favourable situation for its exit. We have already demonstrated that the short diameter of the outlet of the pelvis is from side to side, and the long diameter from the fore to the back part, which is just the reverse of the brim; and,—inasmuch as the long diameter of the head is in the direction of the short diameter of the outlet, and nature will not effect the necessary turn,—we must perform it for her before extraction can be accomplished. This is then a more complicated case than the one just described: the symptoms are the same—the reason why we should employ instruments are the same—but the mode of using them somewhat varies. It is probable that in this case we shall not be able to feel the ear which is placed posteriorly, but that towards the pubes may be detected easily enough, and this is all that is necessary for our purpose: because, if we distinguish one ear, that will become an index to the other; and if we pass a blade over it, and make the second blade a perfect antagonist to the one first introduced, both must be properly adjusted. Having, then, introduced the forceps with the cautions and gentleness before directed, the same

pendulum-like sweep must be used for extraction; but, independently of our extractive effort downwards, we must make a slow rotary motion, the wrist being directed outwards or inwards, in regard as the face lies to the right or left side, so as to throw it into the hollow of the sacrum; by which means we convert the case into one of the first kind. We can generally make this turn without any great difficulty; but before we attempt it, it is indispensable that we should have accurately learned to which side the face was originally directed.

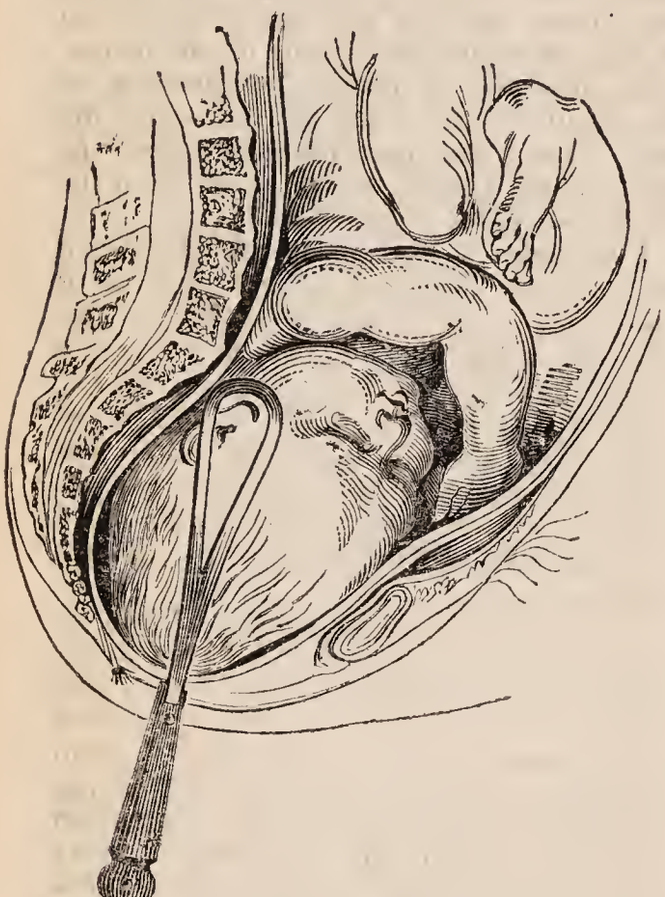
When the face is towards the right side, our object being to turn it into the hollow of the sacrum, the motion of the wrist must be inwards, or that of semi-pronation; but when it is towards the left side, it must then be directed outwards, in the mode of semi-supination. But you may ask, how we are to know when the face is in the hollow of the sacrum? This knowledge may be very easily acquired. I would recommend you to employ an instrument with strongly-marked rivets in the handles; and when the blades are over the ears, the face will not be in the hollow of the sacrum, unless these rivets are in an exact line with the *tubera ischii*. When the rivets are in this situation, you may begin to extract, and not attempt to make any farther turn.

It is in the application of the instrument under this diagonal position of the head that the straight forceps are preferable to those with a lateral curve, in the hands of a young operator. The curved instrument of Levret and Osborn possesses a right and a left hand blade, and requires to be adapted to the head so that the convex edge should look towards the face, and be directed along the concavity of the sacrum, when the head has made its turn and is passing outwards; and it requires no little consideration, so to adjust the blades that the convexity should fit into the curve of that bone. If the concave edge should be directed backwards, the points projecting beyond the child's head will rub against the posterior part of the pelvis, and most probably produce injury. It is by no means impossible that this mistake in the introduction of the blades may occur to one unpractised in operative midwifery; and while such a *possibility* exists, it is much better to have recourse to those means which are least likely to do harm. The advocates for the use of the curved instrument allege that it embraces the head by more points of contact than the straight. This may be true; but even granting their position, that superiority would not counterbalance the other inconvenience attendant on their adoption.

Occiput in the hollow of the sacrum.—In the sixth case,—where the face has come forward, and the head has made a turn, with the occiput into the hollow of the sacrum, and the face behind the pubes, it has not adapted itself to the pelvis in the most fortunate position; and although the woman may have had children before, still it is very probable that it will not pass by the unaided efforts of nature. Our indication here is evidently to extract the child as it lies, although the situation is not the most favourable that could be chosen. I should presume no one would think of turning the face in the hollow of the sacrum before extracting, because nature has already accomplished the greater part of the difficulty—that of bringing the long diameter of the head into a line with the long diameter of the pelvis. The same care is requisite in the introduction of the forceps in this case as in others; but in extracting, the handles must be kept farther back towards the perineum, because the face will require a greater sweep to clear the pubes, than if the occiput were forward: the head does not adapt itself so fortunately to the passages; the bones do not overlap each other so completely; its general figure does not become so conoid, and consequently considerably more room is required for its transit.

Face towards either groin.—We will now consider one of the two last cases, where the face is towards the right groin. This situation of the head,—as the others,—may be learned by the position of the ear and attention to its figure. But in making our examinations for this purpose, we must be careful not to double the flap upon itself, otherwise we may be lamentably deceived. The instrument must be introduced over the ears, in the same manner as before. In this case we have the choice of two methods by which to extract the head—we may either bring the face under the pubes, making a quarter turn of the half pelvis, or we may make a three-quarter turn, and throw it into the hollow of the sacrum. Of these modes I should certainly prefer that in which there is the least turn to be made—namely, with the face under the pubes, provided it could be effected; because we are less likely to do injury to the mother, and also to the child. If we make a three-quarter turn, we may injure the mother's parts by bruising, and perhaps by laceration; and we might even destroy the child: for if its body be strongly embraced by the contracted uterus, and do not follow the extensive turn which we cause the head to make, we must infallibly twist its neck to a great extent; and we might dislocate the vertebræ, to the destruction of its life. But although I recommend you to endeavour to bring the

child with its face forward, still if it will not pass in that direction, rather than have recourse to the horrible expedient of craniotomy, I would advise you to change your method of acting, and turn the face into the hollow of the sacrum. I have frequently effected this alteration in position, though it is usually attended with much difficulty.



The forceps are here represented as applied when the face is situated towards the right groin. The tumor on the scalp is well displayed also in this cut.

Here, again, we remark the superiority of the straight over the laterally-curved forceps; for it is evident, if we apply a curved instrument while the head is in this diagonal situation, the convex edge must be towards the occiput, and the concave towards the face. If, then, we should fail in bringing the face under the pubes, and endeavour to direct it backwards, the blades must be withdrawn and re-adjusted, to prevent the point of the instrument rubbing against the structures at the posterior part of the pelvis; since it would project considerably beyond the forehead. As the straight blades, on the contrary, are perfectly similar in form, the rotation may be accomplished without in the least disturbing their position.

SYMPTOMS INDICATING THE PROPRIETY OF EMPLOYING THE FORCEPS.

It is right that we should now consider *seriatim* the symptoms which would induce us to employ the forceps.

Some practitioners of repute,—as I before stated,—deduce their rule altogether from time, and say, that when the patient has been twenty-four hours in strong labour from the period at which the membranes broke, we are warranted in having recourse to the short forceps, provided we can employ them without injury. Though by no means of universal application, this rule is not to be despised: nevertheless, it must be received with much limitation; because some women will bear up against the fatigue of labour for twenty-four hours with less exhaustion of the constitutional powers than others will sustain in six. Only a few days ago I was called to a patient in a state of depression, from which she never recovered, although not more than six hours had elapsed since the rupture of the membranes. I delivered her under the worst symptoms of exhaustion, such as cold extremities and dark vomiting. She had been in health previously to the accession of labour; there had been no hæmorrhage or laceration, nor any cause for her depression, except the fatigue consequent on great exertion. In this case death would probably have taken place long before the twenty-four hours had expired. The converse of this position holds equally good; for in some cases we should not be warranted in having recourse to instruments, although the twenty-four hours had passed; because the system will have suffered comparatively in a trifling degree.

Others tell us that we are not to pay any attention to time, but simply look to the symptoms present. This, to a certain extent, is also true; but the parts will not sustain pressure for a continued length of time without suffering injury. I have already mentioned, that if the head have been impacted for four hours without advance and recession, I think we are warranted in delivering, merely for the purpose of preserving the soft structures uninjured.

Drs. Hunter, Denman, and Osborn, trusted cases of labour almost entirely to nature. Osborn, in stating the symptoms requiring the use of forceps, says, "All the powers of life are exhausted; all capacity for further exertion is at an end; and the mind as much depressed as the body—they would at length both sink together under the influence of such continued but unavailing struggles, unless rescued from it by means of art." Here we recognize a complete wreck of the powers of life; and I perfectly agree with Professor Burns, that if such a state be allowed to take place, the exertions of art will in general prove as unavailing as the struggles of nature;—if *all capacity* for further exertion is at an end, we can scarcely expect the system to rally. Den-

man also says, "As long as the efforts of the mother continue with any degree of vigour, there is always reason to hope that they will ultimately accomplish the effect of expelling the child without any artificial assistance; in which case the use of the forceps is not required." Again, he says, "A practical rule has been formed, that the head of the child shall have rested for *six hours* as low as the perineum, before the forceps are applied, although the pains should have altogether ceased during that time;" so that if the head have been on the perineum two hours, and the woman be sinking from exhaustion, according to this rule, he would allow four hours more to elapse before he would think of having recourse to the forceps.

We must take these recommendations of Hunter, Osborn, and Denman, however, with some limitation, and recollect that they lived at an age when instrumental interference was frequently had recourse to unnecessarily; that nature was seldom or never allowed to accomplish her object; but the hand was constantly thrust into the vagina and uterus, to dilate the parts;—instruments were employed to extract the child;—and the rudest means were used to bring away the placenta. A most beneficial object, then, was gained by the recommendations of these great men, and the strong language in which they clothed their instructions: a great revolution was gradually effected in the practice of the age, and obstetricians were taught to rely more implicitly on the powers and beneficence of nature. But however useful it might have been during the lapse of the last century to paint in glowing colours the dangers of instrumental interference, and the all-efficient agency of nature, the cautions then inculcated are fortunately, in a great measure, inapplicable to the present time.

We should suppose, from reading the terrible accounts of instrumental dangers met with in some authors, that if a woman was delivered by nature, no injury to the soft parts could possibly happen; but that if instruments were used, it was necessary that some violence should be inflicted. Every practical man, however, knows that the reverse of this is the fact; for many cases occur in which, although the patient has been delivered without instrumental aid, such a degree of pressure has been made by the firm head of the child remaining for a long time in one position, as to produce sloughing of the parts, and subsequent death. Again, I maintain that if the forceps are properly and tenderly applied, and used in a legitimate case, extraction by them does not produce more pressure or pain than would have occurred in the same case had it been left to nature. There may, indeed, perhaps, be a slight aggravation of suffering during the use of the instruments, but that pain is comparatively speedily terminated, and the aggregate quantity is less than the patient would have undergone, had she been trusted to her own powers. It has been said that the hard steel must produce more pressure than the child's head. This, perhaps, may be true to a certain extent; but it must be remembered here, also, that the pressure does not last so long, and that the closing of the instrument produces a partial compression of the head, which must in the same degree relieve the maternal structures; the space thus gained being more than the thickness of the double blades.

I consider it the most difficult part of my duty to lay down rules for the use of the forceps, universally applicable. The principal evils that we have to fear, are a sinking of the patient's strength by exhaustion; laceration of the uterus or vagina; such a contusion of the vagina and perineum as to produce subsequent inflammation, suppuration, or sloughing; and inflammation of the uterus, from excessive action.

With these evils before us, I should recommend you to steer a middle course between the two orders of practitioners just mentioned—to deduce your indications partly from time, but principally from symptoms; and to take care that the patient's strength is not so far exhausted before aid is given as to render recovery hopeless; for surely that man who allows death to steal on by slow degrees, through his own ignorance, timidity, or supineness, is at least equally culpable with him who employs harmless means rather earlier than absolutely necessary, with the honest intention of relieving his patient from present suffering, removing her out of the chance of extensive injuries, or snatching her from threatened dissolution. It must not be supposed, however, that I am an advocate for the early employment of instrumental means, although of a character to do no injury. I merely wish to state my conviction that such assistance had better be rendered, *rather before* it is actually called for, than be delayed till it be *rather too late.*

The rule which I offer for your guidance is taken from a number of circumstances in combination. *First,* we must attend to the previous history of the patient. If she have hitherto been in good health, and is well-formed, she is so much more likely to bring her child into the world without assistance; if, however, she have been confined to her bed for any length of time, we should expect the powers of the system might not be sufficient for the end proposed; though this observation by no means applies universally, for in the last

stages of the most debilitating diseases—such as dropsy and phthisis—the labour is usually terminated naturally. Again, if the patient has had children before, we should expect that this may be born also; unless, indeed, the head be very large, or strongly ossified, or wrongly placed, or hydrocephalic.

Secondly, we must look to the duration of the labour. This is generally attended to by the patient and her friends (who are, of course, unable to form a judgment by symptoms), more than any other circumstance. It is certainly a good general rule to consider, that if the labour has lasted more than twenty-four hours from the rupture of the membranes, there is a great probability that instruments will be required; and that if the head has been impacted four hours, the soft parts must be much endangered.

Thirdly, we must regard the progress of the labour. If the head advances at all, and be not impacted, provided the strength and spirits are good, there is no need to interfere; but if no progress have been made for a number of hours, and especially if impaction should have existed for four hours, then,—provided an ear can be felt,—and the parts are not so rigid as to endanger laceration, we are justified in employing the forceps.

Fourthly, we must consider the remaining strength. Women often suppose they are sinking, and will be earnest in their declarations that they have not strength left to go through their labour, when their power is unimpaired; although there may be a feeling of weariness. There is no word so much abused in the lying-in room as *exhaustion*. The patient will often assure us she is perfectly exhausted, when the uterus is acting with undiminished energy; the solicitous friends will echo the same sentiment, while she is walking about the room, leaning on her nurse's arm. Exhaustion is accompanied and known by a very quick pulse; if it be under 100 beats in a minute, there is seldom occasion for apprehension; but if it have gradually mounted to 120, 130, or 140, our suspicions should be awakened to the probability of approaching exhaustion. It is also known by the pains gradually subsiding in frequency, strength, and duration. We must not confound with this state the sudden suspension of uterine action, which we sometimes observe in the progress of the most natural labour, and which we can seldom account for; the pulse, tongue, countenance, and spirits, remaining good and unaltered:—when the pains decline from a continuance of exertion, there are other accompanying symptoms which indicate distress.

Another proof of commencing exhaustion is a peculiar olive-coloured green discharge from the vagina; and this symptom usually is not sufficiently attended to. I seldom or never saw a case in which exhaustion was approaching, that was not accompanied by this peculiar uterine discharge. Sometimes there is merely a stain on the linen, at others it flows away in large quantity. It possesses a faint and unpleasant odour, though not in the least putrid. This discharge has been looked upon as meconium mixed with the liquor amnii; and its appearance has been considered as a sure test of the death of the child, because it is generally supposed, that when the head is presenting, the meconium would not appear externally unless it had been voided from the bowels in the last death-struggle. I am persuaded, however, that it is not meconium; and I have known many children born alive, who had been declared dead from this erroneous impression. I believe it consists in an altered secretion from the surface of the uterus, consequent upon great exertion and long-continued action; and look upon it as one of the first indications of exhaustion.

Again: exhaustion is known by the breathing being hurried, by the countenance becoming anxious, the eyes dull and sunk in their sockets, the appearance around them dark, and the cheeks exceedingly pale, sallow, or flushed in patches. The face, indeed, generally assumes much of the character given to it in low fever.

The tongue will also guide us in our opinion. If the mouth, indeed, is moist and clean, there cannot be much fever; but if the tongue become loaded with a white fur, fever is present; or if it be coated with a dry brown sordes, that is one indication of commencing exhaustion.

Vomiting also occurs in consequence of exhaustion from great exertion, as it accompanies depression of the powers in the last stage of fever. The matter ejected from the stomach is of the same character in both cases; it is blackish, or has a coffee-ground appearance, and it may be fetid. We must discriminate this vomiting,—as I have before remarked,—from that which occurs at the commencement of labour, as a consequence of the opening of the os uteri; nor is it probable that we shall mistake it.

Shivering is another symptom of exhaustion, and it also indicates great local injury either to the pelvic structures, from pressure, or to the uterus itself.

Coldness of the extremities, accompanied with clammy perspiration, is a very unfavourable sign indeed: if there be cold sweats over the legs, arms, and neck, we may consider that the patient is in imminent danger.

If delirium be present, we may be perfectly satisfied that there is some mis-

chief going on either in the uterus, the pelvis, or the head; and such a case would require extreme care. We may, perhaps, be inclined to bleed. Delivery will often at once relieve this distressing symptom. Another state of mental aberration is that of low muttering. The woman lies quiet, appears to be talking constantly and rapidly, but her articulation is imperfect; and delivery affords the only chance of saving life under this state. We generally find these two last symptoms follow long-continued wakefulness and want of sleep; which, of itself, is one of the many indications of exhausted powers.

The *fifth* indication is the state of the passages. If these are moist, soft, cool, and not tender—if we can pass our finger all round the head easily, we may be sure that there is no impaction, and we need not deliver for their sake; but if the parts become dry, hot, swollen, and painful, so that the patient can scarcely bear the least touch upon them, then there must be injurious pressure, and, — to prevent sloughing,—we must terminate the labour, though there should be but few of the general symptoms that I have mentioned; but it is seldom, indeed, that local injury exists in any part of the body without the whole system sympathizing. If the patient be robust, we may bleed from the arm; if we cannot venture to take blood, we may use fomentations, and endeavour, by local and general means, to remove the inflammation: and if the case be adapted for it, we may have recourse to the use of the short forceps. The longer the pressure is continued, the worse will generally the case become.

Finally, the state of the uterine tumor becomes an indication for delivery. If the abdomen be not tender on pressure, there is no inflammation; but if the application of the hand can scarcely be borne, we have every reason to fear the approach of diseased action. There is no question that inflammation of the uterus itself may be produced by a long continuance of its excessive exertion; but as this is usually accompanied by diminished power in the general system, it is seldom to be removed by bleeding; under such a state, indeed, it would not be always safe to abstract blood freely: delivery offers the only rational method of relief.

Summary of symptoms.— If, then, the pains are subsiding gradually, or have entirely disappeared,—if the strength is failing, the spirits sinking, the countenance becoming anxious,—if the pulse be 120, 130, or 140, in the minute,—the tongue coated with a white slime, or dry, brown, and raspy,—if the patient be vomiting a dark coffee-ground-like matter,—if there have been two or three rigors,—if, on pressing the abdomen, there is great tenderness of the uterus,—if there be green discharge, with heat and tumefaction of the vagina—if there be preternatural soreness of the vulva—if the head have been locked for four hours, and made no progress for six or eight hours,—if there be hurried breathing, delirium, or coldness of the extremities,—then we are at any rate warranted in having recourse to the forceps, even if the labour have not lasted the limited period of twenty-four hours: and we should be acting injudiciously to allow the case to proceed until the last-named symptoms appear, without relief being offered.

But so long as the uterus is acting with energy, the strength and spirits good, the countenance natural and cheerful, the pulse under 100, the tongue and mouth moist and clean—so long as there is no vomiting nor rigors, nor heat, swelling or tenderness of parts; no green discharge, no pain on pressing the abdomen—so long as the head retreats in the absence and advances in the presence of pain—so long as there is any progress in the labour from hour to hour—so long there can be no necessity for instrumental aid; although the case may have lasted beyond the specified limit.

CLINICAL OBSERVATIONS
ON
CATARACT.

By Baron Dupuytren.

From the "Leçons Orales," published periodically, under the Baron's inspection.

Different kinds of Cataract.

Eleven cases of cataract have recently been operated upon in the Hôtel Dieu, by the method to which I usually give the preference—depression; and I am induced to take this opportunity of offering some general remarks upon the subject. Like many other diseases, cataract has been divided into a certain number of species. *Simple* cataract consists in mere opacity of the crystalline lens; and another species, which is nearly as commonly met with as this, is that which arises from opacity of the crystalloid membrane: it is denominated *membranous* cataract. This kind is particularly frequent among infants, with some of whom it is congenital. Saunders has found it present in twenty-one instances out of forty-five. It is in this case most usually complete—very rarely incomplete. Adults are affected with it commonly as a consequence of blows, bruises, or punctures, received on the ball of the eye. It is also observed in persons of scrofulous habit, and in those cases where extraction has been effected without tak-

ing eare to remove the eapsule. In its incomplete form, it has various appearances. The *central* kind is the most common,—so called by Saunders, as its seat is chiefly in the centre of the crystalloid capsule. When in children it makes its first appearanee after birth, it is recognized by a prominent, opaque, and pearly point in the middle of the capsule. This point proeeeds downwards, and radiates in filaments, as it approaches the eireumference; nor is vision immediately lost. This species of the eomplaint is always aecompanied by a eonvulsive movement of the eyes, which, as it were, turn on their axis, for the purpose of presenting to the light suceessive transparent portions of the membrane. The eyelids, and sometimes the head itself, are affected with a similar movement, apparently for the same purpose.

Next after this variety eomes, in point of frequency, the *milky, soft, pulpy* cataract. The crystalline, in this instanee, is very soft in its whole structure: sometimes the softness is eonfined to a part of it; but it is often also eonverted altogether into a white, milky, opaque liquid.

The crystalloid membrane and the lens itself are both liable to become incrusted with a quantity of phosphate of lime, and to aequire a bony hardness, which is clearly perceived when the instrument comes in eontact with the organ. In this case we have cataraet by *ossification*. Finally, according to some authors, the malady puts on occasionally that appearance which they call *black* cataract. This should be perfectly distinct from amaurosis, and discerni ble by manifest signs—sometimes refleeting several eolours at onee, sometimes brownish, sometimes grecnish, and again radiating with white striæ from the blaek ground, and in every case aecompanied by a great mobility of the iris. M. Delpeeh, among others, has denied the existence of this speeies; and I must eonfess I have never myself met with a real case of *black* cataract. Pelletan and Giraud once thought they had found a ease of the kind; and requested me to examine it; but it seemed to me to be merely an amaurosis. However, as MM. P. and G. persisted in their opinion, and entreated me to operate on their patient, I did so, and extracted a crystalline, which was *perfectly sound*. The operation was not attended by any aceident, but the patient still remained in darkness, the retina being paralysed.

It would seem that cataract sometimes results from *hereditary disposition*. The following is a case which appears to put the fact beyond a doubt.

Case.—*Several members of a family affected with the complaint.*—An aged lady came one day to the consultation, accompanied by some of her family. She was considerably above 60 when her sight began to be troubled. In the course of eighteen months both crystallines had become opaque. Depression was practised on one of them, which restored the faculty of vision; nor was it subsequently disturbed. At above 90 she could still see very well: the other cataract was not meddled with. Her daughter, at the age of 28, began to complain of weakness of sight, and was soon unable to guide herself in her walks, yet she could distinguish day from night: the pupils were moveable and the globes sound. I operated on her when she was 30, and by the same method used with the mother. It proved to be attended with equal success. For ten years after she suffered no inconvenience from vision of this side, so that she was tempted to request an operation for the other eye. The journals of the day were full of the praises of a certain oculist, and to him she applied. He performed it by extraction. But, as often happens with this method of treating cataract, the consequences were unfavourable: inflammation, with much pain, set in; the cornea became opaque, and the patient lost her eye; although the other, which had been operated on by depression, remained undisturbed in all its functions.

The son of this lady, aged 17, had two cataracts, which were treated with depression at the Hôtel Dieu, and the patient did well. There was also another grandson of the old lady, who was brought to me, with the crystallines beginning to be opaque; and afterwards a grand-daughter, who could only see objects through a cloud—the precursory symptom of opacity of the lens. Here, then, we had the grandmother, daughter, and three grandchildren, all affected with cataract. The case cannot but be considered as very remarkable.

I have operated in a great number of cases of congenital cataract, and it is worth observing, that I have never met with any of those miraculous circumstances so much dwelt upon by some authors. I have never found that persons on whom I was instrumental in bestowing vision, made those marvellous remarks on the distance, form, and colour of objects, the recital of which has given rise to such disquisitions on the part of the philosophers of mind. On the contrary, I have almost always observed that the blind by cataract, whether the malady was congenital or of very long standing, having been accustomed to live with the aid of four senses only, were generally embarrassed by the addition of the new one: they found it difficult to combine its action with that of the others, and, in many instances, shewed such an indolence in availing themselves of it, that I have been sometimes obliged to

deprive them of the use of one or more of their other senses, in order that they might be induced to exercise that of vision. Thus I have stopped up the ears of a child who used to guide himself by sounds and by the impressions received through his hands, which he constantly thrust out before him, like tentacula.

But the difficulties are sometimes so serious, that I have often failed in restoring vision, though the eye to all appearance has been perfectly treated. There might have been in these cases disease of the optic nerves, either at their decussation or at their origin; or, perhaps, as in members which have long been paralysed and atrophied by inaction, the parts could not resume those powers to which they had so long been unaccustomed.

Remarks on treatment, and the different modes of operating.

With regard to the different modes of operating for cataract, depression is the one to which I am decidedly partial: to extraction I very rarely have recourse, and only in cases where it is manifestly indicated, as, for instance, where the crystalline or its membrane has undergone such a change as to render its absorption impossible. It is a most improper precept given by some surgeons, to depress in every instance, or, indeed, to practise any one mode of operating invariably in every case; for we cannot, *à priori*, determine which operation ought to be preferred. We cannot in surgery, any more than in medicine, invariably adopt the same means in order to effect the same end: thus, in regard to cataract, we must take into consideration the age of the subject, and various circumstances connected with the shape and size of the organ affected; we must look to the degree of energy of the absorbent system; and perhaps it may be said in general, that depression is most suited for children, extraction for old age. In early life the vital functions are in all their vigour; absorption of the lens commences the very moment the part loses its living properties; and, besides, at this period the crystalline is never so hard as in more advanced age, and consequently not so refractory to the process of absorption. In old people it is just the reverse, and the lens is often of remarkable hardness. I have found the crystalline perfectly unabsorbed in aged patients, who had been operated on by depression above two years before their deaths. There are, however, some circumstances which tend to render depression the more desirable mode of treatment for both the young and old. Children are not very manageable, and it is difficult to prevent them from raising their hands during the operation, so that when we proceed to extract, there is danger of the discharge of the vitreous humour. In old men, the eye is usually deeply sunk in the orbit, or perhaps the globe itself is not well developed; in which case extraction is particularly difficult. Among people of all ages there are individuals in whom, owing to an aberration in the movement, or in the shape or relative proportions of this organ, this method is precarious, and by no means easy; other persons, again, are subject to rapid and convulsive motions of the eye; and, in short, it may be observed, that when an individual had been deprived of sight for a time, he loses the habit of fixing his eye, along with the habit of seeing with it, the movements of the globe being no longer obedient to his will.

But whatever mode of proceeding we adopt as to the operation itself, it is highly necessary that we take some preliminary means of precaution. We must have a special regard to the state of the atmosphere and the prevalent medical constitution; also the habit of body of the patient must be thoroughly understood. There are some circumstances connected with this point which often render it advisable to defer the operation: the patient may labour under rheumatism, or pulmonary catarrh, or disorder of the stomach, bowels, &c. If rheumatism be present, the operation may cause a metastasis to the head, and a very serious ophthalmia may be the consequence. A cough may bring the blood too much to the head; and if depression have been practised, there may be danger of the cataract rising again from the efforts made in coughing. Complaint of the stomach, again, may cause similar disturbances; and we must recollect, besides, the strong sympathy between the stomach and the eyes. Moreover, if we operate while the stomach is out of order, we shall have to observe a tedious dietetic after-treatment, to which we may not find children and old people very amenable. The presence of piles is another obstacle which must be attended to: the application of leeches to the anus, in this case, is strongly to be recommended, both for its local and revulsive effects.

The immediate preparations which I generally prescribe are some baths, lavements, emollient drinks, and general or local bleeding, according to circumstances. A little castor oil may occasionally be administered with advantage; and if the eye be very irritable or convulsive, we should accustom it to surgical manœuvres resembling the operation. If we mean to proceed by keratonyxis, we should drop in between the eyelids a little of the solution of the extract of belladonna, or of cherry-laurel water, for the purpose of producing a dilatation of the pupil.

In the great majority of cases I prefer

keeping the patient in his bed, when he is undergoing the operation: it may be rather an unfavourable method for extraction, but for couching the advantage of such a position is obvious. Should syncope occur, an embarrassing accident for the operator, it must be greatly alleviated by the recumbent posture.

After the operation the eyes must be carefully bandaged, and the patient kept in darkness. Low diet and perfect repose must be enjoined. If there be signs of congestion about the head, venesection is advisable; and it must be repeated, if pains of the head or eyes continue. If vomiting supervene, as is almost invariably the case with children, an anodyne draught is prescribed, and after that, if necessary, Seltzer water, or the *potion de Rivière.*

When the patient is affected with double cataract, it is preferable to operate on only one eye at first, and not to touch the other till the first is cured. Experience has shown the propriety of this proceeding, while the laws of physiology are strongly in its favour. Two simultaneous operations must necessarily be much more serious for the patient than one, and the consequent inflammation may be attended with accidents much less manageable for the practitioner. It is also worthy of notice, that this inflammation very rarely is equally regular in both eyes: it may be very severe in one of them, and even produce complete disorganization, while the other is but slightly affected. This, however, is no more than is commonly observed in the simultaneous occurrence of inflammation in organs which are double.

Complications of Cataract.

Let me now direct your attention to the complications which attend on cataract. The chief of these, and by far the most common, is contraction of the pupil. This affection is sometimes carried to such an extent, that the aperture shall scarcely admit the blade of the couching needle. It is most frequently observed in persons of a scrofulous habit, and it is connected with an inflammation of the retina, distinguished by a thickening and redness of the latter. Taken in time, it yields to antiphlogistics, and the application of a little extract of belladonna. Another complication is the adhesion of the crystalline capsule to the back of the iris, or of the lens to the membrane that contains it, or of the iris to the ciliary circle, &c.; all which maladies are, in general, only the results of inflammation of the iris.

We should, however, be on our guard when we meet with an appearance of this kind: for it often happens that the inflammation of the iris is not primary, but sympathetic, with an inflamed state of the retina,—a very common occurrence, as indeed scarcely a week passes that we have not examples of it in our wards, or at the public consultation. Scrofulous children are most frequently affected with it, and the nature of the malady can be recognized while yet the little patients are at a distance from us: they approach with unsteady steps, with their hands up to their faces to keep away the light; and when brought to the window they turn away abruptly, covering their eyes closely with their hands, and the more they are desired to remove them, the more strongly they persist in the action; they resist their removal: attempt to open the eyelids, and they struggle violently; they cry loudly if you succeed, but still the eye is turned upward, so as to cover the transparent cornea. It is quite a hemeraphobia! Now can we call this an inflammation of the iris? Surely not exactly; for this malady constantly exists in a high degree, without giving rise to any such phenomenon. We must, in short, admit it to be a true phlegmasia of the retina—that nervous membrane of exquisite sensibility, which has such delicate functions to perform in receiving and transmitting the impressions of light. Inflammation of the part reacts on the iris, and causes that contraction of the pupil which we might easily mistake for a symptom of iritis.

There is another complication of cataract, which we ought by no means to pass over without noticing: it is paralysis of the retina, which often renders the operation completely nugatory. In some individuals we may observe certain peculiarities which strongly tend to establish, if not an absolute certainty, at least a high presumption, that they are affected with this paralysis. A young man, of about 28 years of age, who is at present a patient in this hospital, is affected with accidental membranous cataract—that is to say, he has had a contusion on the anterior part of the globe of the eye. The capsule is not wholly altered; several points of its surface, as well as the lens itself, are perfectly transparent; yet his sight is gone—he cannot distinguish day from night. An aged woman, on whom I operated in December last, presents us with a contrary example. Although her lens was entirely opaque, she still had the power of vision in some small degree; she could distinguish light from darkness. Now, whence the difference—what is the reason that this young man is deprived of all perception of luminous rays? You will all agree that there must be something more in the case than the mere existence of cataract; and, indeed, there is but little room to doubt that the cause of the phenomenon is simply paralysis of the retina. Little hope, therefore, remains that we should be able

to re-establish his powers of vision. Yet the patient wishes us to operate, notwithstanding. The desire of getting rid of a deformity is, no doubt, a strong motive with him. I have often met with persons (particularly of the other sex) who, under similar circumstances, requested to have the operation performed, although they knew well they should not recover their sight: they were induced, however, to do so through a desire of being freed from an unsightly appearance: with one of them an intended marriage was in question; and I have yielded sometimes to importunity. No accident occurred; but such motives are in general too frivolous, when a man can live without inconvenience with a cataract in his eye. We should therefore endeavour to persuade our patient to forego the operation.

It sometimes happens, however cleverly the operation of couching may have been performed, that the lens rises again, and interferes with vision. Coughing, ill-regulated movements on the part of the patient, and numerous other causes, may lead to the occurrence of this accident.

It is always a mortifying occurrence for the patient, who, after having recovered his sight, again finds himself deprived of it. In such cases, if the cataract had been depressed *en masse*, we might take either of two courses,—leave it as it is, that it may be absorbed; or, what is better, as the absorption may be tedious, depress it once more, just as if it had never been meddled with. Cases have occurred in which it has been necessary to repeat the operation four times; and it may be observed, that the repetition seems to diminish the risk of ill consequences to the patient.

There have been instances also of the lens rising after a certain time—not wholly, but in part, the remainder of it having either been absorbed, or become embedded in the vitreous humour. An old man, on whom I operated not long since, presented us with an example of this variety. Cataract may also re-appear behind the pupil, even after the lens have been broken up; in this case, the parts become re-united; but this sort of cataracts, called *cataracts by agglomeration*, are in general more easily absorbed than when the lens remains whole. And when the process fortunately takes place, it is interesting to observe the pleasure with which the patients are affected; they, in fact, seem to have joy in this progressive improvement, in a higher degree than melancholy as the disease advanced. Sometimes, however, the *debris* are obstinate, and interfere for a long time with vision; in this case, depression is obviously to be recommended.

After the operation, it may be observed that short-sighted persons (myopes) have more correct vision than in their natural state; while the long-sighted (presbytes) are more embarrassed than before the cataract formed. The latter require a lens to be placed before the eye, as a substitute for the crystalline; but the use of such artificial means ought not to be allowed them till some time after the operation has been performed, otherwise the intensity of the impressions produced will inflame the eye, and defeat the object of all that has been accomplished.

It is a remarkable circumstance that the crystalline, which is so steady in its proper place, when in a healthy condition, becomes subject to displacement when it has lost its transparency. Sometimes it passes altogether into the anterior or posterior chamber of the eye—more rarely into the former; and, again, it is found only partly detached and floating, so as incompletely to obstruct the pupil. It is by displacement of this nature it has sometimes happened that patients with cataract have suddenly recovered their sight, after an abrupt movement, or a blow inflicted on the head or the eye: and there are cases on record which show that there are individuals who have the extraordinary power of removing at pleasure, by the simple influence of their will, the affected crystalline from one chamber of the eye to the other; a very remarkable example of this you will find in M. Demours' work on the Diseases of the Eye.

All the pathological varieties of cataract which I have now brought to your notice, may, as you can readily conceive, require different modifications in the treatment, manual or medical, according to their several natures and causes. If the contraction of the pupil be the result of acute inflammation, antiphlogistics, the application of leeches to the angle of the eye, cupping the temples, and especially bleeding in the feet, will serve to remove it; but if the affection be of a chronic kind, and there be no symptoms of inflammation present, these methods will be of no use: we should then find it beneficial to rub in belladonna, or (better still) to apply the cherry laurel-water, in order to dilate the pupil, and thus to facilitate the operation.

Experience enables me to observe, that in cataracts complicated with considerable contraction of the pupil, the capsule of the lens frequently adheres to the hinder surface of the iris. If the case be sufficiently recent, the lymph not being yet in a condensed form, belladonna will be found to be very useful; for the iris, acting under its influence, interferes with the recent agglutination, and leaves little more for the operator to do. But if the lymph be already organized, the needle must be brought between the two membranes, and their union must be dissolved before de-

pression is praetised. There is also great differenee in the manner in which the iris adheres to the ciliary eirele in different individuals: in some, it is so extensive and intimate, that great difficulty is experieneed when we would separate them, and form an artificial pupil; in others, the iris tears rather than be disengaged; whilst in other instanees it gives way to the slightest foree.

When the lens, whether affeeted with eataraet or not, beeomes displaeed, the general rule with me is, if it give rise to no inflammatory aetion, to leave it to itself—but to operate as soon as signs of inflammation are noticed: in faet, if the crystalline be not opaque, nothing is gained by its extraction; and if it be, nature has undertaken to depress it. When the lens has become embedded in the vitreous humour, there is evidently nothing to be done but to leave it as it is, for the circumstanees are just those of depression, and this part is in the proper state for the action of the absorbents. Lastly, when the crystalline lens has obtained an entranee into the anterior chamber of the eye, the operation is simple and easy. In the generality of eases, a small ineision of the cornea may be made, when extraction with the needle can easily be performed.

Unique operation.—In one ease on whieh I operated, in 1819, I ventured to adopt a new method. My patient was a man aged 34: the lens, affeeted with eataract, got into the anterior chamber, as it seemed, on the head being bent suddenly downwards. There was much inflammation present: vcnesection, baths, and purgatives, were had recourse to; after whieh I proceeded to operate. I introduced the needle through the sclerotic eoat, at about two lines distance from the transparent eornea —passed it through the posterior chamber into the anterior, and there transfixed the lens. I then drew it back into the posterior ehamber, in the lower part of which I held it for a time before I withdrew the needle. The man was immediately restored to sight, got well without any accident, and left the hospital in six days after.

THE DELIVERY OF A CHILD,

Having a Second Imperfect Fœtus contained in the side of its Scalp and Face.

By G. Bury, Esq., of Farnham,
Member of the Royal College of Surgeons, and Member and Licentiate of the Society of Apothecaries in London.

On the 21st of October, 1833, I was called in by one of the midwives of this town to see the wife of a sawyer, in labour with her second child. She was a healthy young woman (her appearance was that of rude health), and in her twenty-second year. At eleven o'clock in the forenoon I obeyed my summons, and learned that the pains had been strong for thirteen hours, and the membranes ruptured about six. As the midwife had felt something unusual about the head of the child, which had not latterly descended, notwithstanding the brisk contractions of the uterus, further aid was naturally desired by her, as well as by the patient and friends.

On making my examination I found the child's face presented to the pubes, and the whole scalp very considerably distended, apparently with blood; the tumefaction being universally soft, but firm and inelastic. This I then considered to be what is commonly denominated "tumor of the scalp" in the fœtus, a frequent cause of delay in parturition, and, together with the position of the head, fairly to explain the reason of the protracted duration of the labour. One ear could be plainly felt by the finger; and seeing circumstances thus situated, I did not judge it necessary to make any painful efforts to the patient, by introducing the entire hand into the vagina for the purpose of searching for the other. I advised all parties to exercise their patience; and after informing the midwife of what I had ascertained, requested her to send for me again if the case did not proceed to her satisfaction.

At six o'clock in the evening my attendance was solicited in haste, with a message to bring my instruments, as no progress in labour had been made. Such proved to be the fact, for the head was lying, as in the morning, at the brim of the pelvis, not having in the least degree descended under the incessant, and what may be called even violent, contraction of the uterus. The scalp was somewhat more tumefied; the woman's strength was beginning to give way; the pulse was at 96, full and firm; the external parts were tender, and also slightly swollen. It was now evident that some serious obstacle to delivery existed, and that the powers of nature were unavailing; and therefore I determined on performing artificial delivery without loss of time.

I first endeavoured, with the aid of my hand, to turn the face of the child

into the concavity of the sacrum, but was not able to do it, seemingly baffled by the strong uterine action, which repelled the face and occiput into their former situations as soon as they were brought to correspond to the lateral parts of the pelvis. The latter object, however, was far from being perfectly accomplished, wherefore, thinking the face or forehead might be lodged on, or impacted behind, the pubes, I next attempted to extricate it with the vectis; but this effort was alike unsuccessful, not being equal to effect the least descent of the head. The forceps were then resorted to, and although from some cause, at the time not discoverable, the blades were incapable of being locked, there being an interval of nearly an inch between the two, some little advance appeared to be made by the traction, which was employed with a fair purchase. For nearly half an hour I exerted considerable force with this instrument, perhaps more than I had ever used before, and was at length obliged to relinquish it, from its total inadequacy.

When applying the forceps I did not satisfactorily make out both ears (respecting which I was not particular, since there had been no pulsation in either of the fontanelles from the first period of my attendance this evening, whence I concluded the life of the fœtus was extinct), and consequently I suspected that some unnatural formation of the child existed, to account for the difficulties of the case. But I felt that no time was to be sacrificed in conjectures concerning the cause of the obstruction; the expulsive efforts were so strong, as to fearfully excite in my mind a dread of the worst effects, and termination of forcible and unproductive pains.

I now, therefore, resolved to introduce my hand into the uterus, with the intention of turning the child, and simultaneously detecting whether any impediment was appertaining to the latter. The right hand being passed over part of the face instantly came in contact with what I might have regarded as the fœtal abdomen in a state of ascites. On this the uterus was closely contracted, apparently wasting its power. Having got my hand some way beyond, I reached one foot after great difficulty, and encountered still greater in bringing it down between the (supposed) tumid abdomen and the very forcibly contracted uterus. I conveyed it into the vagina, but could not carry it any farther. The presence of the head, and the contiguity of the other part of the child, entirely prevented me from retaining the foot even in this situation, when I tried to reintroduce my hand into the womb to gain the other extremity. I was, therefore, constrained to abandon all hopes of getting down both legs, and to be content with one only. With this one foot I made pretty strong attempts; but they were quite fruitless, as I could not get it to descend even to the external orifice.

Being thus frustrated in each mode of delivery, and there being now some obvious source of impediment in the child, the destruction of the latter presented itself as the last and indispensable means to be resorted to. The woman's strength was much exhausted, and the pains continued dreadfully violent, and without the intermission of scarcely a minute. It was now half-past seven o'clock, and doubtless the action of the uterus was greatly increased by my efforts: I accordingly repaired to my house for the instruments further necessary, which occupied only a few minutes; and soon after re-entering the room, just as I was about to use the perforator, I observed the patient to be seized with a convulsion. I instantly opened a vein in each arm, and allowed the blood to flow, whilst I was breaking up the contents of the cranium. After the loss of about three pints of blood, which latterly became thin and florid, sensibility returned, and, I am happy to add, was unimpaired. Meanwhile, I had dispatched a messenger for my colleague, Mr. Clark, whose assistance I was necessarily most anxious for, as my own strength was greatly reduced, and there was the most urgent demand for promptitude. The bystanders were all alarmed at the onset of the convulsion; and those who have ever witnessed such scenes, and had such frightful cases to deal with, well know the relief felt by the mind on the arrival of a brother practitioner. The perforating instrument entered by the anterior fontanelle; and after evacuating a large portion of cerebral matter by the opening, I fixed the blunt hook against the frontal bone. A considerable quantity of blood escaped in the fluid state, on cutting through the scalp, but not much on breaking down the interior of the head. Tolerably strong traction was employed

with the hook, placed as above, but the head was still immoveable: I consequently resolved on yet further destroying the skull; and having made a long incision with the perforator, extending in front to the nose, and posteriorly along the whole sagittal suture, extracted two central portions of the ossa parietalia, and compressed the cranium, now thoroughly emptied, into as small a compass as the force of my fingers could effect. Mr. Clark, after this, tried to accomplish the descent of the head with his hand, but was unable to alter its position in any important degree. He then fixed the hook on the temporal bone very firmly, but even this tenure gave way under the unsuccessful exertions of each of us, though it allowed us to exercise very strong extractive power. During all this time, the enlargement above mentioned, and referred to the abdomen of the child, was somewhat more readily tangible; and as I could not distinguish, owing to its bulk, either the child's neck or chest, I imagined that the belly was very much distended with dropsy, and that its most dependent portion was thus pressed downwards by the uterus. This enlargement also seeming to be the cause of the obstruction to delivery, I was inclined to perforate it as well as the head; but my colleague advising that I should first endeavour to bring down the feet, I again had recourse to this plan.

After great labour, though not quite equal to that experienced in my former trial to turn, I succeeded in getting one foot close to the orifice of the vagina, where I secured it by applying a ligature around the ankle. The more difficult task now remained, to find the other lower extremity, and place it in the same position; this, however, was ultimately done; but the foot was not carried equally low with the other into the vagina, so as to enable me to pull with both of them. In the course of a little time this difficulty was got over; for Mr. Clark fortunately passed a tape around the ankle, high up into the vagina, as it was, by which we gained all the purchase that could be desired. A steady force being then employed by us, each drawing with one ligature, the evolution of the fœtus was quickly felt, in a remarkably manifest manner. The inferior extremities, the abdomen and thorax, were delivered forthwith. After extricating the arms, we found some obstacle to the passing of the head; but it did not delay the completion of the labour more than half a minute. The lower limbs, belly, chest, and arms of the child were perfect and well formed; therefore, during the temporary period of obstruction, endured by the head in clearing the outlet, our expectations were raised to behold some abnormal formation of, or growth from the head or face. The sequel of my paper will, I trust, fully explain the nature of the impediment to parturition, and at the same time disclose the cause of our former fruitless efforts to relieve the sufferings of our patient.

The child being removed, the placenta was discovered in the vagina, and easily abstracted by the hand.

The termination of the delivery was not completed before nine o'clock P.M. Our patient was necessarily reduced to an extreme degree of exhaustion, but her intellect and sensibility were unimpaired, and there was good uterine contraction. Her pulse was at 130, and the countenance blanched and elongated, presenting a strong contrast with the florid and plump aspect of the morning. A full opiate was given, and fomentation applied to the abdomen. Diet to consist solely of gruel and tea.

Next morning Mr. Clark and myself visited her together, and were grieved to witness the actual commencement of peritonitis. The appearance of the countenance betrayed exhaustion in a manner more marked than last evening: the lips had lost their florid hue, and the whole face was extremely pallid; in a word, the look was death-like, having that peculiarity in the expression of the features which is generally noticed in the latter stages of puerperal disease ending fatally. She was lying on her back, with the thighs drawn upwards to the abdomen, and respiration was rapid and painful. Pulse 144, thready, and no hardness to be detected in it, probably depending on the copious bloodletting to relieve the convulsive paroxysm. The after-pains were very severe, but there was constant pain, of course somewhat less, with the greatest tenderness evinced on pressure, over the whole abdomen.

Leeches were applied to the belly, which was directed to be afterwards constantly stuped. General bleeding was obviously inadmissible, and we could do no more than place only a

feeble hope in the antiphlogistic powers of mercury. Two grains of calomel were therefore prescribed, with three of James's powder, to be taken every two hours; and the bowels were moved by a gentle aperient draught. The diet was necessarily the same.

A short interval of ease, though not of freedom from pain, followed the use of the leeches, by means of which a considerable quantity of blood was locally taken away.

On the following day, nevertheless, the pain was more acute than before, and was especially complained of in both hips. The leeches were re-applied, and the fomenting and calomel ordered to be continued.

The objects of this communication would not be served by my relating at any length the particulars of the treatment of the case. I regret to say that, in despite of the remedies administered, as above seen, actively and with regularity, and the incessant and vigilant attention of the medical attendants, the poor woman gradually sank, the progress of the disease seemingly unarrested, till death took place in the morning of the 26th, five days and a half after the birth of the child.

On the 23d she suffered very severe pain in the head, but it did not last long. This was the sole cerebral symptom observed by us, as consequent to the convulsion. Up to the last hour of existence, most intense agony persisted in the whole abdominal cavity; and that constantly referred to the hips was yet more excruciating. No effect of mercurial action was at any period perceptible in the mouth. A postmortem examination was not allowed.

Front view of the tumor entire, shewing plainly the two pouches, or different portions, of which it consisted. There was a deep partition, or septum, between these two parts, formed by bone and cartilage, as described in the text. At the apex of the larger pouch, the cuticle was thinner than elsewhere; some abrasions of this part are shewn, and were produced probably by the fingers of the operator, or the feet of the child in the act of turning *.

It now devolves upon me to lay before the reader the most interesting part of my paper, in giving an account of what proved to be the obstacle to the natural completion of labour in the foregoing history.

Attached to the right side of the head and face of the child, and entirely enveloped in the scalp and skin of the face, and in a prolongation from the former, where its region normally terminates, was a tumor of most peculiar form and character, even in outward appearance. In size, it might be said to be nearly double that of a natural fœtal head, though its form was irregular, and bearing no similitude to the head of a fœtus. I state that it amounted almost to twice the magnitude of the head of a full-grown child, in order that it may be at once understood what an insuperable obstacle to parturition nature

* We selected the above from a set of drawings sent to us by Mr. B. along with his paper. The style in which those drawings are executed is highly creditable to the artist, Mr. F. G. Delamotte, son of the professor of drawing, Military College, Sandhurst.—Ed. Gaz.

had to contend with: it might be truly represented as trebled.

To the touch the mass was generally soft, from superficial fluid, similar to the mollified state of a bladder which has contained ice in its cavity for some time, or to the abdomen of a fœtus in a state of ascites.

It will be remembered, that in the foregoing description of the difficulties encountered during the period of labour, I have confessed that for a while I imagined this mass to be a dropsical belly, but I afterwards relinquished this idea, from being unable to discern with the fingers either the neck or thorax of the child below the supposed ascites, and, consequently, before the hand came in collision with the same; indeed, these latter parts could not be made out at all in the neighbourhood of the soft swelling, in consequence of their being altogether covered by it in front, as the view in the engraving will indicate. In two or three spots some solid bodies could be distinctly felt, which Mr. Clark and I agreed were like the limbs of a small fœtus. Hence our suspicions were strongly raised as to the nature of the interior and the contents of the mass, but it would be vanity to say that we *expected* to find what the dissection laid before our eyes.

An incision being made into the posterior part of the mass, where it was in proximity to the neck of the child, about four ounces of a thin yellowish serum, stained with blood, escaped on the division of the skin and a membrane immediately subjacent, which possessed the exact appearance of the chorion of the placenta. The fluid might likewise be well compared to liquor amnii. The harder parts previously felt, were now, of course, more manifest to the touch; and the incision being extended, and the whole tegumental covering reflected forwards upon the face, our curiosity was gratified not a little by exposing to view the very singular phenomenon of a second fœtus. The condition of this second child, confined in so limited a sphere for carrying on vitality, must have unavoidably been imperfect; and such it eventually proved to be. The whole mass consisted of two pouches (seen in the engraving); the less one in front, and occupying the cheek only of the child, and the larger behind, and containing the amnial fluid and the principal members of the fœtus. These pouches communicated by a narrow channel, which was arched over in a most curious manner by a dense cartilaginous septum, situated between the two, and in the course of the line exhibiting their separation in the drawing. This septum was found to be continued from a very remarkable prolongation of the temporal angle of the os frontis, having a smooth and polished surface, like cartilage, and constituting a kind of boundary to the growth of the mass laterally and anteriorly, as will hereafter be shewn more fully. The channel of communication just alluded to, was filled by a small part of the unformed fœtus, and was capable of admitting the passage of my finger. Thin shreds of transparent and perfectly organized membrane were here and there plainly to be traced on the exterior of the mass, and the membrane had exactly the appearance and physical character of chorion. Two imperfect limbs of a fœtus were readily recognized, and a third was less distinct. A portion of scalp covered with hair, and part of the cranium, were very manifest. In different situations there were several imperfect viscera, in their present state resembling small bladders, or transparent sacs, and, in one instance, strongly like an hydatid. The interior of the mass seemed firm, but its contents were not explored by the scalpel. The whole was embedded on the lateral part of the head and face of the infant, its deepest attachment being in the site of the temporal fossa, whence it was not, of course, separated. Here, most probably, I conceive, its nutritious vessels were transmitted from the temporal vein and artery of the child. The connexions of the mass to the parts whereon it lay were slight, so as to be detached by the gentlest exercise of the finger, excepting over the temporal pit, as above specified.

A most singular process from the external angle of the frontal bone was discovered, passing forwards and downwards upon the face between the two pouches, nearly an inch and a half long: on this part the fœtus lay, and by it was prevented from encroaching further on the face. At the distal extremity of this process, the cartilaginous septum before spoken of might be described as taking its origin. An equally curious process from the os temporis was also found, arched from behind forwards and inwards, taking its direction under

the horizontal ramus of the lower-jaw of the child, and corresponding in purpose with the former from the os frontis.

The lower jaw-bone of the child was perfect, as were, indeed, all the bones of the face and head. In short, the child itself was full grown and well formed; and had there been room for the passing of the head and tumor enclosed, or means known by which the delivery of them entire could have been accomplished, I can, without difficulty, believe that the extra uterine existence of the child would not have been annihilated by this extraordinary adventitious growth; nay, perhaps it would have received but little influence from this super-addition, since no parts essential to life were involved with its structure.

Though cases of malformation, similar to the above, are to be met with in authors, they must be admitted to be rare in practice. One or two specimens, bearing some analogy, have, I believe, within the last few years, been deposited in one of the anatomical collections in London. This preparation I have sent to my friend and former preceptor, Mr. W. J. Wickham, surgeon to the Winchester Hospital, to stand in juxta-position with a very singular modification of the far-famed "Siamese Twins."

ON THE FUNCTIONS OF NERVES.

To the Editor of the Medical Gazette.

Sir,

In consequence of the discussion upon the functions of the fifth pair of nerves, which has occupied a place in a few of the last numbers of the Gazette, and which does not appear to have made much progress towards a settlement of the question, I am induced to offer a few observations, which may have the effect of causing both parties to view the subject in a new light.

In the first place, it is to be observed that the faith of both parties is firm in the theory of Sir C. Bell respecting respiration. This theory rests upon the supposition that all the respiratory nerves arise from a distinct portion of the superior part of the spinal marrow. Now no one can look at the anatomical disposition of this part, and of the nerves connected with it, without being convinced of one of the two following circumstances: first, either that there is no distinct "tractus respiratorius;" or, second, that if there is, the nerves which Sir C. Bell calls respiratory, do not arise from it; for instance, the portio dura belongs to the anterior column, the nervus accessorius to the posterior, while the fifth and phrenic are like all the spinal nerves, in having what is termed a double root, one belonging to each column, which being the case, they do not belong to any separate "tractus." In either case, therefore, the theory of Sir Charles Bell is founded in a most obvious error. If, then, the points in the present dispute are branches and ramifications of error, where will the discussion lead?

A very slight attention to the motions performed by the muscles of respiration is sufficient to convince us that they are in constant action by night as well as by day, and that they are at all times liable to have their action increased by the will. (It should here be observed, that the will has in no case the power of preventing the action of a muscle, its power being simply that of calling any muscle subject to its influence into action, and of increasing that of some which are always acting.) As there are many other muscles whose action is of a similar nature, the respiratory muscles form only part of an extensive class whose action is alike, though their functions are different. This being the case, it is evident that the cause of the peculiarity of the action of this class of muscles is to be found in some general law.

The circumstances to which I am anxious to direct the attention of the parties interested in this question, are these: 1st, All purely voluntary muscles receive filaments from the anterior columns of the spinal marrow. 2d, All purely involuntary muscles receive filaments from the sympathetic ganglions. 3d, Those which are mixed, *i. e.* always acting involuntarily, yet at all times subject to the influence of volition, among which are those of respiration, are supplied both by anterior and by sympathetic filaments. 4th, Filaments from the posterior columns are common to all. Now volition and sensation are properties which are only exercised occasionally; they are also exactly opposite in their nature, the one being an

active, the other a passive property: there is, therefore, no necessary connexion between them; for although the mind is always conscious of the completion of an action, on account of the posterior nerves with which all parts are provided, a person may perceive an external impression without being necessarily induced by it to perform any act of volition. As these properties are thus proved to be independent of each other, every thing relating to them would be performed as well if the nerves were every where separate, consequently no explanation of the cause of their union can be found in any supposed necessity of connexion between volition and sensation.

The real cause of their union is to be found in a function which cannot be performed by any nerve separately; this is the secernent function (in this term should be comprehended not merely the formation of bile, milk, saliva, &c., but the whole process of growth, nutrition, and repair of every part), which, since during health it is constantly being performed, requires the agency of some power whose operation is as uninterrupted while the body is at rest, as when it is in motion. It must also be independent both of volition and sensation, which are only acting, or acted upon, at intervals. The importance of this function will be immediately perceived, when it is recollected that, besides a means of calling muscles into action, and a means of conveying to the mind a knowledge of the completion of the action, it is no less essential that they should be maintained in a *state fit to act*. It has been elsewhere shewn*, that for the performance of this, which is a part of the secernent function, posterior nerves are indispensable, and that it is on this account that whatever mode of action muscles may possess, posterior nerves are common to all. It is also shewn in the work referred to, that no single class of nerves can perform this function, the union of two of an opposite nature being requisite; and that as a nerve of voluntary motion is as opposite in its nature to a nerve of sensation as one of involuntary motion, it is immaterial as far as regards the maintenance of the secernent function whether a voluntary or an involuntary nerve be united to a nerve of sensation; the growth of muscle, the deposition of fat, the secretion of fluids in the joints and bursæ, being as well performed in the limbs as the growth of the heart in the chest, or the secretion of gastric juice in the stomach. It is also immaterial as regards this function, whether involuntary as well as voluntary nerves, are united to nerves of sensation, because the difference which can result from this variety will be in the mode of action performed by the muscle so supplied, the secernent function being every where performed by another function, which cannot be carried on unless nerves of an opposite character are united.

My object in making these observations has been to shew, that much more information on the functions of the nerves generally, is requisite for the settlement of the question respecting those of the fifth pair, than can be obtained either from the theory or experiments of Sir Charles Bell; and I hope they may be the means of directing the inquiries of those who wish to follow up the subject, in a proper channel.

I have the honour to remain, sir,
Your obedient servant,
JAMES WM. EARLE.

14, Old Broad-Street,
May 19, 1834.

* A New Exposition of the Functions of the Nerves.

PATHOLOGY OF THE NERVOUS SYSTEM.

To the Editor of the Medical Gazette.

SIR,

IF I have any hesitation in answering the "Queries" which your correspondent has proposed, it arises wholly from the fear that we may be discussing subjects on which your readers have no remaining doubt; for I confess that, to me, the subject is so interesting and full of curious matter, and the mode of Mr. Walker's treating it is so liberal and unexceptionable, that I can only have pleasure in answering him: yet I must say, that the questions which he puts are such as to embrace nearly the whole physiology and anatomy of this department!

The subject which was first proposed for discussion by Mr. Walker was this—Do the ophthalmic branches of the fifth pair possess any power over the mus-

eles?—are they motor nerves, as well as nerves of sensation? He maintains that they have both these functions; and such a view agrees with a theory which he entertains of certain motions of the eyelids and of the iris being controlled by these nerves.

Without recurring to the same arguments which I brought forward in my former letter, to controvert this opinion respecting the ophthalmic branches, I may be allowed to remind Mr. Walker of a principle which bears directly upon the question at issue: it is a principle which, in attending the lectures in Great Windmill-street, he must have repeatedly heard announced, and dwelt upon with much earnestness for its importance, and very copiously illustrated; I mean—that a nerve cannot be endowed with the two distinct offices of sensation and motion, unless it have two distinct roots. In relating his original experiments on the roots of the spinal nerves, so far back as twenty-three years ago*, Sir C. Bell held the same argument; viz. that the root or nerve which transmits the mandate of the will *outwards* to the muscles, cannot carry a sensation *inwards* to the sensorium. There must be, for this purpose, two nerves or two roots, each having a distinct connexion with the brain. This is the fundamental principle on which this gentleman's researches, with regard to the functions of the fifth pair and of the spinal nerves, have been prosecuted. It is allowable to infer that it was this idea of the improbability of one nerve conferring these two incongruous powers, that induced him to commence his experiments: the principle was, at all events, satisfactorily confirmed by the experiments which he made on the anterior and posterior roots of the spinal nerves; and it was subsequently corroborated to the fullest extent by his investigations of the functions of the fifth pair. Now it is to be regretted that in every one of Mr. Walker's communications, treating of the functions of the fifth pair, he has omitted to take notice of this principle. If he had founded his arguments upon the structure and distribution of the roots of this nerve, he would have perceived that it is not all the branches indiscriminately, but those only which are composed of its two roots commingled, that possess the double properties of motion and sensation.

The fifth pair is distinguished by this remarkable anatomical character, that the root which confers the power of motion is extremely small, while that which bestows sensation is about five times the size. From this it follows, that the whole of the branches prolonged from the common nerve cannot be endowed with these two properties, but that it will be only those which are formed by a combination of the fibrils of the lesser root with those of the greater root, that will have motion as well as sensation. It was the knowledge of this fact which led Sir C. Bell, in his original paper, presented to the Royal Society*, to express the opinion that the fifth pair is not universally, and in all its branches, a compound nerve; it was this consideration which made him refrain from designating it by the extended title (as Mr. Walker erroneously does for him), of the nerve of motion and sensation, and induced him to use the more restricted term, "nerve of mastication and sensation." He applied this name to the fifth, in order to point out distinctly that it was only to the muscles engaged in mastication that the motor root of the fifth was distributed. All the extracts from Sir C. Bell's and Mr. John Shaw's writings, which Mr. Walker has adduced to prove that the fifth is a nerve of motion, refer exclusively and solely to the actions of feeding. Neither of these gentlemen whom he has quoted ever conceived that the fifth possessed any motor influence unconnected with the organ of mastication; indeed, the exclusiveness of the distribution of the motor part of the fifth to this organ was one of the arguments urged in favour of classifying this nerve apart from the portio dura and certain others, and including it in the "original or symmetrical system," common to all classes of animals.

When Sir C. Bell bestowed the name "nerve of mastication," upon the fifth, can it be conceived possible that he ever contemplated that its branches were destined to give power to the muscles of the forehead, or to those of the eye-brow, or to those of the eye-lids, or to those of the nostrils? None of these parts have the most remote connexion with the organ for chewing the food. As to the ophthalmic branches, where they come

* See Preface to Bell on the "Nervous System," 4to. 1830. p. xiv.

* 1821.

out from the orbit to the forehead, eye-brow, and eye-lids—that is to say, the very nerves which Mr. Walker conceives may be motor nerves, Sir. C. Bell made experiments for the express purpose of demonstrating, that they exercise no influence whatever over the muscles, and that they are simply nerves of sensation. The muscles situated there, as well as all the muscles of the face, are supplied by the portio dura; and it is from this nerve that they derive their only power of motion, voluntary as well as involuntary; for I may remark here, that Sir C. Bell never attempted to draw any distinction between the voluntary and involuntary actions of the muscles of the face, or conceived, as Mr. Walker would appear to indicate, that these two different kinds of power were regulated by the different nerves of the face. The only distinction which he at any time dwelt upon was concerning the muscles of the *lips;* and his object was to determine how far these were under the control of the fifth during the actions of *mastication;* for he had demonstrated that they were principally influenced in their motions by the portio dura. All his illustrations of the functions of the portio dura evinced most clearly that he considered it capable of conferring both voluntary and involuntary power over the muscles. The example of the coachman with injury of his portio dura, who could not whistle to his horses, or of the person continuing to breathe while insensible in apoplexy, or in sleep, might remind him of this; but the fact is placed beyond a doubt when we consider that he represented this nerve as the *only one* which could bestow motion to all the muscles of the face, excepting merely the muscles of the lips, which he considered had two sources of nervous power.

It is to be regretted that, in treating of the muscular branches of the fifth pair, Mr. Walker should not have consulted the latest edition of Sir C. Bell's works, to ascertain what are his opinions on this subject. The extracts which he has made, in his letter, are from the earliest accounts of these researches. There was an error in the first experiments, which were intended to solve the question concerning the relative influence of the portio dura, and of the motor part of the fifth, over the muscles of the lips. Sir C. Bell had originally supposed that, during the act of feeding, the muscles of the mouth were brought into action by the branches which are distributed to them from the infra-orbitary nerve; but it was discovered that there was an error in this. Certain experimenters, first abroad and then near home, in the course of repeating his experiments on the nerves of the face, just after they were published, stumbled—and it was by mere chance — upon this mistake. They shewed, however, that the infra-orbitary branch of the fifth had no power over the muscles of the lips. As this happened to be the only nerve on which Sir C. Bell had thought it necessary to make an experiment to confirm his opinion that the fifth was in part a motor nerve, the consequence of making this correction was to disprove, as far as experiments went, the truth of his position, that this was a double nerve: it was left to be inferred, since all the branches experimented on were merely nerves of sensation, that it was, on the contrary, a single nerve. So little acquainted were these experimenters, to whom I allude, with the anatomy of the roots of the fifth pair, or with the principle which made Sir C. Bell attach importance to its having two roots, that when they had thus swept away his only *experimental* proof of this being a motor as well as a sensitive nerve, they ceased to do any thing more. It was my brother, the late Mr. John Shaw, who had frequently, ere this time, repeated the experiments on the roots of the spinal nerves*, and knew what were the grounds on which Sir C. Bell had founded his opinion, when he pronounced the fifth to be the nerve of mastication and sensation, that gave the first confirmation of it by experiment. He was aware that the statement as to the functions of the nerve was not made from the result of experiments performed directly upon the fifth itself, but that the opinion was suggested from the previous investigations respecting the roots of the spinal nerves and the resemblance of the fifth pair to these nerves. Accordingly he examined, but with greater accuracy than before, the distribution of the double roots of the fifth nerve. He found them uniting together in the third division alone; and selecting this nerve, where it emerges from the skull through the foramen ovale, he performed those

* See the account of his experiments performed before the students in Great Windmill-Street, in March 1821; that is, several months previous to Sir Charles Bell's original paper.

decisive experiments upon it which established that the deductions from the anatomy were correct. It was demonstrated, that as the fifth pair, like the spinal nerves, had double roots, so it had double functions; but that in its character of being a moter nerve, it was restricted, as originally stated by Sir C. Bell, to the muscles employed in mastication*. Thus, although it was necessary to make an alteration, in describing the functions of the individual branches about the mouth, from the statements in the original paper, there was no occasion to change the first designation of the fifth nerve—viz. that it is the nerve of sensation and mastication.

If Sir C. Bell had consulted books of anatomy somewhat more carefully, before he experimented on the infra-orbitary nerve, and had seen that no filaments whatever of the lesser root joined the second division, but that all the branches were derived simply from the one root—viz. the ganglionic—we cannot for a moment doubt, knowing the principle on which he proceeded, that he would never have looked for the lips receiving *compound* properties from it. The subjoined engraving is accurately copied from Prochaska; and it shews that although the anterior root approaches close to the second division, it does not transmit any fibrils to it: it is incorporated altogether with the third or inferior maxillary division. Immediately subsequent to the experiments of which I have been speaking, on the third division of the fifth, great interest was naturally attached to the minute anatomy of the roots of the fifth pair; and it was found that their course and distribution were most accurately given by numerous anatomists. Although many other authorities might be referred to, yet if the reader has occasion to consult the original papers on the structure of the brain in Reil's Archives of Physiology, he will find in vol. xi. (1815) a very admirable account of this nerve by Niemeyer, illustrated with numerous correct drawings, to shew the course of its roots. This is an anatomical paper which has met with very undeserved neglect.

When Mr. Walker examines this drawing of the roots of the fifth pair, he will perceive that the ophthalmic division arises exclusively from the large ganglionic root; and if he accede to the principle of one root bestowing only one function, he will cease to entertain the opinion that its branches can be nerves of motion as well as of sensation. When M. Magendie was for the first time made to confess that he had been anticipated in the experiments on the roots of the spinal nerves, which he had announced as original, it is somewhat curious that he immediately published the account of another series of experiments, of which the object was to invalidate, if not to completely nullify, the results that had been formerly obtained by Sir C. Bell: their tendency was to remove all confidence in the correctness of the principle, that a nerve of sensation requires to have a different origin from a nerve of motion. He pretended that his second set of experiments demonstrated that the anterior root was endowed with the compound properties of motion and sensation, and that the posterior root possessed likewise both sensation and motion*. It is to be observed, that he advanced to these conclusions while confining himself to experiments made in the spinal canal of living animals, after breaking up the bones—for the most part, while employing galvanism as his means of stimulating the nerves—and without trying what would be the result if he prosecuted the same refined inquiries on the fifth. Now it is these alleged observations of M. Magendie which can alone give the slightest apparent support to Mr. Walker, for supposing that the ophthalmic branches of the fifth, arising from a single root, can possess two such opposite kinds of functions as sensation and motion. But before Mr. Walker puts confidence in the authority of that experimental physiologist, I beg him to read a most interesting Memoir, by M. Muller, of Bonn, in a late number of the Annales des Sciences Naturelles; where he refutes, in a satisfactory and perfectly conclusive manner, these statements of the Frenchman; where he follows M. Magendie into his own ground, and proves that his opposition to Sir C. Bell's original account of the spinal nerves is altogether unsupported by experiment, and that his principle remains still valid in its full extent; that, in short, the anterior root is for motion alone, and the posterior root for sensation alone †.

* For the first account of these experiments, see the Med. and Phys. Journ. Oct. 1822.

* Journ. de Physiol. Oct. 1822.

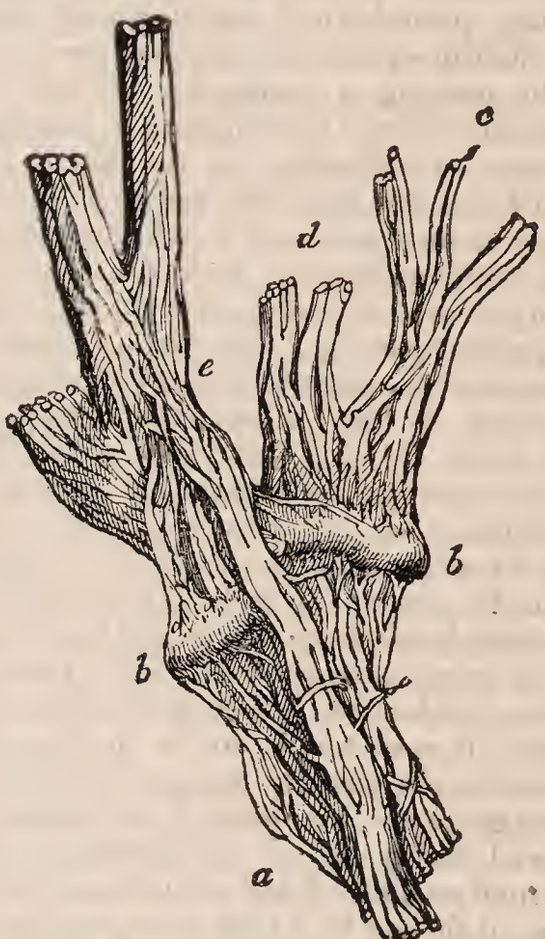
† It is to be accounted a very strange circumstance that we never see a notice of the functions of the spinal nerves, in any work of anatomy, without finding Sir C. Bell's name and that of

Mr. Walker puts the question—"If the fifth have no influence over the muscles of the face and forehead, why is every one of these muscles supplied with branches of this nerve? Do the muscles—*e. g.* those of the eye-ball, or the muscles generally—receive nerves for any other purpose than that of bestowing upon them the power of motion?" This is truly an interesting subject to which Mr. Walker adverts; for it is a very remarkable circumstance that filaments of nerves, derived directly and exclusively from the ganglionic—that is, the sensitive root of the fifth—are profusely distributed to the muscles; yet, if we stimulate these nerves in experiment, we cannot make the muscles contract. I regret that it is impossible for me to offer a full answer to this question, owing to the length to which I have already encroached on your space. I can only state, that it was the observation of this anatomical fact which led Sir C. Bell to entertain the views which he holds on the "muscular sense," and to compose the paper on the "Nervous Circle which connects the Voluntary Muscles with the Brain," that is to be found in his work on the "Nervous System." If Mr. Walker has not read this last paper, I beg to recommend it for his perusal, as tending to remove the difficulties which he may have on the subject in question. He will find the same views, I may also mention, more copiously illustrated, in the Bridgewater Treatise on the Hand; where the author treats of the combination of the sense of touch with the sense of muscular action, and where he applies the same course of reasoning to the sense of vision, in its connexion with the motions of the eye-ball.—I have the honour to be,

Your obedient servant,

ALEX. SHAW.

May 19, 1834.



a, the two roots; *b*, the Gasserian ganglion; *c*, the ophthalmic division; *d*, the second division, of which the infraorbitary is a branch; *e*, the inferior maxillary division.

M. Magendie linked together. Never did two writers hold more opposite opinions than these, as to the functions of the roots of the spinal nerves. Sir C. Bell has maintained, from the first, that sensation and motion are inconsistent qualities in the same root, or nerve; and that they are never united, unless the nerve be compound in its structure, and one of the roots have a ganglion formed upon it. M. Magendie, on the contrary, asserts that both roots of the spinal nerves are capable of bestowing either of these two properties; though he acknowledges that they do not possess them in the same proportion or intensity: "L'isolement des deux proprietés dans chacun des ordres de racines n'est donc pas absolu."—(An. de Syst. Nerv. Magendie et Desmoulins.) "Ils semblent établir que le sentiment n'est pas exclusivement dans les racines posterieures, non plus que le mouvement dans les anterieures."—(J. de Phys. Oct. 1822.) These are the conclusions to which he arrives after pretending to shew that, by stimulating the posterior roots, he can excite muscular contractions, and by stimulating the anterior roots he can make the animal howl. As there is this wide diversity of opinion between these two gentlemen, in reference to the spinal nerves, so there is the same in regard to the fifth. Sir C. Bell has always represented it as analogous to the former set of nerves: M. Magendie considers it, with its two roots, to be the nerve of the five organs of the senses! I am led to make this remark about the complete want of agreement in the opinions of these two physiologists, from observing, with surprise, that Dr. Roget, in his Bridgewater Treatise, recently published, has followed the usual fashion of joining their names together, as if they were in perfect harmony upon a point respecting which they entertain the most diametrically opposite opinions.

PATHOLOGICAL APPEARANCES OF SEVEN CASES OF INJURY OF THE SHOULDER-JOINT; *With Remarks.*

To the Editor of the Medical Gazette.

SIR,

I BEG to inclose you an account of the pathological appearances observed in seven cases of the shoulder-joint, which appear to have suffered from severe injuries at some former period. I am sorry I am unable to give any previous history of the different cases; the whole

of them occurred in bodies brought to my theatre for dissection.

I have the preparations in my possession, and they shall be accessible to any gentleman who may wish to see them.

If you esteem this communication worthy a place in your valuable journal, I shall be obliged by its early insertion.

Case I.—In the body of a man brought to the dissecting-room, under the old system of violation of the grave, in the month of February 1832, the following pathological condition of the left shoulder-joint was observed:—

On making a transverse section in the centre of the deltoid muscle, for the purpose of reflecting it, the bursa situated beneath was observed to be much larger than usual, very much thickened, and communicating with the general cavity of the shoulder-joint by a large irregular opening. On further examination, it was noticed that the tendinous insertion of the subscapularis muscle had been entirely torn away from the lesser tubercle; the supra spinatus, infra spinatus, and the teres minor muscles, had likewise been completely detached from the greater tubercle. The tendon of the long head of the biceps had been torn away from the upper part of the glenoid cavity, and entirely withdrawn from the joint: it was found to be firmly attached to the anterior margin of the bicipital groove. The head of the humerus moved freely in all directions on the glenoid surface of the scapula, and the size of the cavity of the joint was much increased, from the extensive laceration of the capsular ligament; it included the whole of the neck of the bone and both tubercles. The appearance of thickening of the capsule below would seem to indicate that it had likewise suffered laceration in this situation at the time of the injury. A small portion of the outer margin of the glenoid cavity had been fractured off, and, with the under surface of the acromion process, and the tubercles of the humerus, were partially covered with portions of enamel-like or porcelain secretion; and numerous bands of organized fibro-ligamentous substance extended across the cavity of the joint in different directions. There was a fracture of the humeral extremity of the clavicle, which extended into its articulation with the acromion.

Case II.—Mary B * * * *, æt. 30, died of consumption in St. George's Workhouse, and was removed to the theatre for dissection, in November 1832, under the new regulations provided by the Anatomical Bill.

The left shoulder-joint presented the following appearances:—The bursa beneath the deltoid muscle communicated, by a large irregular opening, with the general cavity of the joint: the tendon of the subscapularis muscle was partially torn from the lesser tubercle of the humerus, but the insertions of the supra and infra spinatus muscles and the teres minor, remained perfect. The round tendon of the long head of the biceps muscle was ruptured, leaving a portion, about half an inch in length, attached to the upper part of the glenoid surface: the lower portion of the tendon had been drawn from the cavity of the joint, and lay firmly attached to the margin of the bicipital groove. The ruptured extremities of the tendon were perfectly smooth and rounded, and the superior portion had become much flattened: small bands of fibro-ligamentous structure were observed, but none of that peculiar enamel-like secretion noticed particularly in the preceding case.

Case III.—Ann D * * *, æt. 38, died January 1, 1833: removed from St. George's Workhouse, for the purpose of dissection, under the new regulations.

The following account of the appearances of the right shoulder-joint is extracted from the notes entered at the time, in the dissecting-room journal *, by Mr. G. Knox, who dissected the extremity. On removing the deltoid muscle, the head of the humerus came into view, presenting a larger surface of bone than usual: on further examination, it was found that the tendon of the subscapularis muscle had been partially torn away from the lesser tubercle, and the original insertions of the supra spinatus, infra spinatus, and teres minor muscles, had been completely separated from the greater tubercle. The tendon of the long head of the biceps had also been

* We have been in the habit for some time past of keeping a dissecting-room journal, in which every thing is entered that occurs out of the regular course, or differs from the natural appearance; the consequence has been, that we have already collected a few interesting examples of varieties in the distribution of arteries and nerves, the absence of certain muscles, &c. If this plan were generally adopted in the dissecting-rooms in London, in the course of every session, a very curious and valuable collection might be made; the most interesting examples of which might be selected, and annually published in one of the medical periodicals.

torn from its origin, and had become attached to the upper part of the bicipital groove.

The under surface of the acromion process was found hardened by the friction of the head of the humerus, and covered by a peculiar enamel-like secretion. The capsular ligament appeared unusually thick at the lower part, which gave rise to the idea it had been lacerated at the time of the injury, and had become re-united by the effusion of coagulable lymph.

Cases IV. and V.—Catherine S***, æt. 56, died February 1833; was removed from St. George's workhouse, under the new regulations, with a medical certificate signed "sudden death." She was a short, stout, muscular subject, and, upon inquiry, proved to have been a hard-working woman at the wash-tub up to the time of her death. The shoulder-joints presented the following appearances:—

In the right shoulder.—The bursa beneath the deltoid muscle communicated with the general cavity of the joint, by a jagged irregular opening about the size of a half-crown. The tendon of the subscapularis muscle was torn from the lesser tubercle, and the tendon of the supra-spinatus muscle detached from the greater tubercle; both having become united with the common capsule. The tendon of the long head of the biceps had been torn from the upper part of the glenoid cavity, withdrawn from the joint, and found to be firmly attached by ligamentous structure to the margin of the bicipital groove. There were a number of small exostoses on the tubercles, covered with the enamel-like secretion, which corresponded to a similar appearance on the under surface of the acromion process.

There was an oblique fracture of the acromion process of the scapula, which had separated about an inch of its expanded extremity; it had not united by bone, but had formed an artificial joint through the medium of cartilage, and was further strengthened by a fibro-ligamentous capsule.

The appearance of the biceps muscle was very remarkable, and first directed the attention to the condition of the joint; the portion of the belly of the muscle appertaining to the long head, was remarkably short, and the short head, unusually developed, appeared in great measure to supply the place of the other.

In the left shoulder.—On dividing the deltoid muscle, the bursa at its under surface was found very much thickened and enlarged, and an opening observed which communicated with the articulation. On further examination, this opening was found to be caused by a partial separation and detachment of the supra-spinatus and subscapularis muscles, from the larger and lesser tubercles; the surfaces from whence they had been torn being within the capsular ligament. The inner surface of the capsule presented a very rough fibrous appearance, occasioned by the portions of the lacerated tendons; the synovial membrane presenting small villous productions, the result, apparently, of organized lymph.

The tendon of the biceps was wanting in the joint, having been torn through and divided into a number of fibres, which were attached to the upper part of the bicipital groove; small bony exostoses had been thrown out on the tubercles, and the cartilaginous surfaces of the humerus and scapula were here and there covered by small patches of the enamel-like secretion.

The acromion process of the scapula had been fractured precisely in the same situation as that of the opposite side, and formed a similar artificial joint.

Cases VI. and VII.—Thomas K***, æt. 40, died April 1834; removed from the Islington Infirmary, with a medical certificate signed "Consumption."

In the right shoulder.—The bursa beneath the deltoid muscle communicated by an irregular opening with the general cavity of the joint. The tendon of the subscapularis muscle was entirely detached from the lesser tubercle, and the fibres of the muscle itself were drawn downwards from the ventes of the bone, presenting a small cavity beneath, lined by an irregular fibro-ligamentous structure, and communicating with the articulation. The tendon of the supra-spinatus muscle was torn from the greater tubercle, the infra-spinatus and teres minor muscles remained attached; but the muscles appear, at some former time, to have suffered severe tension.

The tendon of the long head of the biceps muscle was not separated from its origin, but displaced from the groove, and lay loose in the inner part of the cavity of the joint; it is expanded, and bears evidence of having been subjected to pressure and friction; one surface,

which corresponds to the head of the bone, is smooth and polished, the other presents a bundle of silvery cords, which may be spread out upon the finger three quarters of an inch in width; the bicipital groove is nearly filled with a fibro-ligamentous substance, similar in structure to numerous small bands, which extend across the joint in different directions, from one point of the synovial membrane to another.

The capsular ligament had been much stretched, and will readily allow the head of the humerus to be displaced under the coracoid process of the scapula, resting upon the inner margin of the glenoid cavity. There is no distinct evidence of the capsule having been ruptured in any other situation than that already mentioned, communicating above with the bursa beneath the deltoid muscle. There were small bony deposits about the tubercles of the humerus, which were within the general capsule, and here and there small patches of that peculiar porcelain secretion.

In the left shoulder, the bursa beneath the deltoid muscle was found very large, and its parietes thickened, but it did not communicate with the general cavity of the joint, being separated by a thick layer of lymph. The capsular ligament was perfect, but very capacious, and apparently thicker than natural. The head of the humerus moves very freely in its socket, and may be easily drawn beneath the inferior margin of the glenoid cavity. The tendon of the subscapularis muscle is torn from the lesser tubercle, and the fibres are drawn from a considerable part of the venter of the bone. The tendons of the supra spinatus, infra spinatus, and the teres minor muscles, remain attached to the greater tubercle, but their fibres have evidently been, at some former time, very much stretched.

The tendon of the long head of the biceps, as in the preceding case, was permanently displaced from the bicipital groove, and lay at the inner and lower part of the joint, playing over a smooth part of the lesser tubercle; one surface is perfectly smooth and glistening, the other is a flattened band of silvery fibres. There are a number of fibro-ligamentous bands of organized lymph stretching across from one point of the capsule to another. The bicipital groove is nearly obliterated by the same structure, and portions of ossific matter have been deposited.

Remarks.—There are several points of extreme interest in the foregoing cases, connected with the pathological condition of the shoulder-joint consequent upon severe injuries, which present themselves for consideration; and I conceive might be attended with a very useful lesson, if we are careful to avoid all hasty opinions, and only select such practical inferences which may be essential in accounting for many circumstances attending accidents of the shoulder-joint.

It may assist us in forming a right judgment of the case, point out the most rational method of treatment, and enable us to prevent many bad consequences which frequently follow injuries of this important articulation.

The first point which suggests itself is, that a severe blow, strain, or dislocation, is more frequently accompanied with severe local injury of the muscles and tendons, in the immediate neighbourhood of a joint, than we should be otherwise inclined to suppose.

I have formed this opinion from the circumstance, that the seven cases of severe injuries of the shoulder, described above, occurred in the comparative small number of dissections, not exceeding forty individuals; and I think I may therefore safely infer, that these effects consequent upon severe accidents more frequently happen than are suspected, and from inadvertency or other causes are entirely overlooked in the ordinary dissection of bodies.

It is a curious fact, that in two instances both joints of the same individual should have presented nearly similar appearances, and that the same cause should probably have produced exactly similar results.

The frequency, in these cases, of the rupture, or tearing away from its origin, of the tendon of the long head of the biceps muscle, and its subsequent withdrawal from the joint into the bicipital groove, and its complete and permanent displacement in two instances, are facts in themselves of great practical importance.

The separation of the tendon of the subscapularis muscle from the lesser tubercle (excepting in one case), where it was only partially torn away, may be looked upon as the common result of the accident which produced these appearances; not so, however, with the tendons of the supra and infra spinatus, and the teres minor muscles, from the

greater tubercle, which appear to be more uncertain. In two instances all three tendons were torn away; in three cases only the tendon of the supra spinatus; and in the remaining two cases, the whole of the tendons preserved their natural attachments.

The capsular ligament appears in all cases to have been extensively lacerated at the upper part, and to have communicated with the bursa beneath the deltoid muscle, with only one exception, which hardly deserves to be excluded, from the very evident thickening, which implies that a communication had existed, but that union had afterwards taken place. In only two instances could any thing like a laceration be detected in the lower part of the capsule; and even in these it could only be suspected from the thickness of the capsule in this part. In all the cases the capacity of the general capsule was much increased, and included within it more of the upper part of the humerus than natural. In two cases it distinctly allowed the head of the bone to be displaced from the glenoid surface of the scapula without the least difficulty.

The fibro-ligamentous bands must be considered as the result of the organization of fibrine thrown out during the inflammatory stage consequent upon the first effects of the injury. The enamel-like or porcelain secretion may probably be traced to a similar cause, or looked upon as one of the resources of nature, to prevent parts subjected to unusual friction from being materially injured by such a process. In all the cases (excepting case II.) the joints presented more or less of the fibro-ligamentous bands, and the enamel-like secretion immediately decides the question, which might otherwise have arisen as to the probable data of the injuries.

It is likely that the first case was an example of the effects that may be expected to follow the dislocation into the axilla, with the addition of a rupture from its origin of the round tendon of the biceps muscle, which, according to the opinion of Sir Astley Cooper, is by no means to be considered a common circumstance, or generally attendant on this accident. It would seem to have been produced by a severe blow on the top of the shoulder, from the appearance of injury and fracture of the humeral extremity of the clavicle.

The second case is probably one shewing the effects of partial dislocation, in which the head of the humerus is drawn forwards against the coracoid process of the scapula, but quickly slips back again into its natural socket. It is an example of a rupture of the round tendon of the biceps muscle, instead of the tendon being torn away from its origin.

The third case, I am inclined to think, has been a dislocation into the axilla or on the dorsum of the scapula: it exhibited the most serious injuries; the whole of the tendons of the muscles were torn away from their attachments.

The fourth and fifth cases occurred in the same individual. I am at a loss to say in what manner the bone had been forced to produce the appearances observed, but I am inclined to think it probable that they are both examples of the dislocation upwards. I find that Sir Astley Cooper says, in his valuable work on Dislocations, "It has been supposed that a dislocation of the os humeri upwards might occur, but it is obvious it could only happen under fracture of the acromion: it is an accident I have never seen." In strength of the opinion I venture to offer in these examples, I find that the humerus may be readily displaced upwards when the acromion is fractured; so that the head of the bone rests on the superior margin of the glenoid surface, but immediately returns to its natural situation.

It has been suggested, that the accident might have happened from the individual falling down stairs while the arms were half extended, or by any violence that would tend to throw the head of the bone forcibly upwards. It is a remarkable fact, that the appearances in one shoulder-joint should be nearly a counterpart of the appearances in the other; they would seem to have been produced at the same time, and both to have been followed by severe inflammatory symptoms, as evinced by the fibro-ligamentous bands and the secretion of enamel on the processes of bone. The motion between the fractured ends of the acromion was considerable, and each surface is invested with a layer of cartilage.

The sixth and seventh cases likewise occurred in the same subject. The appearances in the right shoulder, I think, clearly indicate that it is an example of the effects of a dislocation under the pectoral muscle. The appearances of the left shoulder result, perhaps, from a dislocation under the pectoral muscle,

or into the axilla. The curious coincidence, that both the long tendons of the biceps muscle should have remained displaced, and that both should present the same remarkable character, are facts worthy of being remembered; and the question occurred to me, whether the pain and inconvenience a patient feels in some cases for a considerable time after the reduction of a dislocated arm, may not be owing to the long tendon of the biceps muscle having been displaced from its groove, which would not only give pain by stretching that muscle, but very considerably affect the action of the fore-arm? In this case I should think it right to give the arm a gentle rotatory motion after reduction, that the parts might thereby be properly replaced and adjusted to their wonted situations.

There are a few other observations I might take the liberty of offering, if I had not already extended my communication to a very considerable length: I shall therefore conclude by assuring you that I have been induced to draw up the account of these cases in the hope and expectation that they might lead to some practical remarks from others who may have had opportunities of witnessing similar appearances resulting from severe injuries of the shoulder-joint.

It will afford me great pleasure at any time to exhibit the preparations which are in my possession, to any gentleman who may be desirous of seeing them.—I remain, sir,

Your obedient servant,

JOHN GREGORY SMITH.

Hunterian Theatre of Anatomy,
Great Windmill-Street, Haymarket,
May 15, 1834.

ON PERFORATION

OF

STRICTURES OF THE URETHRA.

With Cases.

BY R. A. STAFFORD,

Surgeon to the Mary-le-bone Infirmary, and formerly House Surgeon to St. Bartholomew's Hospital.

[Continued from p. 205.]

CASE XII.—*Stricture, with fistulous openings in the Perineum, and a false passage into the Rectum, perforated.*

Oct. 10th, 1832.—Captain J., a naval officer, æt. 60, gives the following history of his case:—He contracted a stricture at the age of 25, which embittered the greatest portion of his life, having brought on, at different periods, incontinence of urine, fistulous openings in the perineum, and a false passage into the rectum. In the early part of the disease he paid but little attention to it, until incontinence of urine came on, and afterwards urinous abscesses, produced by the urethra having ulcerated behind the stricture, and allowing the urine to escape. He then consulted Mr. Whately, from whom he obtained considerable relief. After Mr. Whately's death the stricture returned, and he then placed himself at different times under the care of other surgeons, but without receiving much benefit from their treatment. He at length, in 1824, consulted Dr. James Arnott, who employed the dilator invented by him, from which he obtained considerable relief, being enabled to pass a large sound into the bladder, and making water in a full-sized stream. By the aid of passing occasional bougies, which kept the stricture open, he continued pretty well until 1831, when, finding that the disease had returned, and that the stricture had nearly closed up again, he attempted to use the dilator himself; in doing so, however, he had the misfortune to burst the urethra, and thus to break a passage into the rectum. Considerable hæmorrhage followed the rupture of the canal, which was succeeded by a very severe fit of illness. Some few months after his recovery, and finding he could only void his urine by drops, he passed a bougie for himself, but was baffled in the attempt: the stricture was impassable. He now consulted a surgeon of the greatest eminence in the City, who found he had a false passage leading into the rectum, and that no instrument whatever could be got into the bladder. Under these circumstances, he advised him to make no further attempt for the present, fearing that the passing of instruments might only add to the mischief already done. He followed this advice; but the power of expelling his urine having greatly diminished, he became highly nervous, and fell into a state of despondency; having before his eyes the constant dread of complete retention, without the hope of relief. As a last resource, he came to London, and placed himself under my care.

On examining him, I found the cicatrices of old fistulous sinuses in the scrotum and perineum, and that one of them still remained open, and allowed a few drops of urine to escape through it. I passed a bougie. Its progress was arrested in the membranous portion of the urethra, and, on passing my finger into the rectum, I could feel an opening which appeared to come out from the urethra immediately

behind the bulb. By pressing on the bougie, I discovered also another false passage, leading on one side of the urethra. Although there was great difficulty in preventing the point of the urethral perforator from getting into one of these false passages, yet, by placing my finger in the rectum, and guiding the point of the instrument, I was enabled to cut in the right canal. I was, however, extremely cautious, and never made more than one or two incisions at most, at a time. After employing the urethral perforator four or five different times, I accomplished the perforation, and left a catheter in the bladder, which remained there for a week or ten days; the size being increased every other day, until it arrived to a No. 12. Steel dilators were then introduced twice in the week, and in six weeks from the time he first came under my care he went home quite well, making water naturally, and being able to pass a large-sized (No. 13) sound into the bladder. The false passage communicating with the rectum healed up, and the urine did not, as it had done before, pass through the one in the scrotum.

Remarks. — A more difficult case could not well have been offered for treatment than the present, on account of the false passages, and the difficulty of preventing the instrument entering into them. It is of the utmost importance that the operator should be certain, when there are false passages, that he is in the right canal, or otherwise, by cutting on in them, he may do incalculable mischief, and run the risk of destroying the patient. In such cases, therefore, the greatest caution is necessary; and more than one or two incisions ought never to be made, on this account, with the urethral perforator at one operation.

It would appear from this, as well as from other cases where it has occurred, that when the urethra is ruptured or torn, it is usually followed by considerable hæmorrhage.

Case XIII.—*Three Strictures treated by the Lancetted Stilette, with a profuse discharge resembling Gonorrhœa.*

1832.—Mr. H., a gentleman residing in Ireland, consulted me under the following circumstances. He had suffered for many years past the most distressing pain and irritation in the urethra, attended by a profuse discharge resembling gonorrhœa; he had the greatest difficulty in passing his urine, and was teazed by a constant inclination to do so; he had taken every description of medicine administered in such diseases, used injections, and had been treated for stricture by the common methods, without ever obtaining the slightest relief. Having been so long harassed from the above causes, he fell into a state of melancholy and nervous irritability, which sometimes almost amounted to insanity. On passing a bougie for him, I found the urethra was diseased, but more particularly in the anterior portion, almost throughout its whole course. There were strictures at two inches, three inches, and four inches from the orifice, and also one in the membranous portion. I operated on the two first at once, and kept the parts open by passing bougies. When the healing process had taken place, and when the contracted parts were enlarged to the natural size of the canal, the third stricture was divided; this also was kept open and enlarged, until the hardened substance of which it was formed was absorbed. The discharge ceased, and the pain and irritability of the urethra subsided. As the stricture in the membranous portion would admit through it a bougie, and as I had reason to believe it was kept up from sympathy with the others, I recommended it to be treated by simple dilatation. It yielded to this method, and my patient returned back to his own country perfectly well.

I have since heard from him; and he informed me, that from having been one of the most miserable men in existence, he is now one of the most happy.

Remarks.—A discharge from the urethra, like gleet, is one of the commonest symptoms of stricture; but a profuse discharge, resembling gonorrhœa, is not so common. It no doubt arose, in this case, from so much disease being present in the anterior part of the urethra, and thus the irritation kept up a constant morbid secretion from the mucous membrane lining the canal. I have seen the same affection occasionally take place in other cases of stricture, and it has usually arisen from some excitement of the part, caused by irregularity of living, drinking more wine than the patient is in the habit of doing, &c. As I have remarked before, and as I have related in other cases, an affection of the mind is not an uncommon attendant on this disease.

Case XIV.—*Stricture, producing retention of urine, perforated.*

In September, 1832, I was sent for to see a gentleman in Wales. When I arrived there I found him labouring under

retention of urine; and, after having applied leeches to the perineum, placed him in a warm bath, and employed the usual remedies, without relief, I at once proceeded to operate. The stricture was only three inches down the canal, and was quite impermeable. By three incisions with the blade of the urethral perforator the stricture was permeated: a catheter was passed, and more than three pints of urine drawn off; it was left in the bladder three days. When I felt that my patient was secure from a return of retention, I left him, requesting him to follow me up to town to complete his cure; he did so: steel dilators were passed, until the stricture was made as large as the natural size of the canal. He has remained well ever since.

Remark.—There is but little doubt that had not the urethral perforator been successfully employed in this case, the bladder must have been punctured.

Case XV.—*Stricture arising from an accident perforated.*

Thomas Clerk, æt. 32, was admitted into the St. Marylebone Infirmary, 8th January, 1833, with a stricture of the urethra. He states that he received a hurt in August 1831, on board a ship, by missing his hold of a rope, when he fell with the whole weight of his body upon the edge of a bucket which was on the deck, and which struck him between the legs, on the perineum. He bled very much at the time, and for some time afterwards, from the urethra, and only a few drops of urine occasionally escaped. He remained in this state for three days, when total retention came on. The usual means were employed, but with no effect, and a catheter was attempted to be passed, but it could not be got into the bladder. He had had complete retention for forty hours, when, having taken a dose of castor oil, he was seized with violent vomiting, and during the effort of straining made at the time, a large quantity of coagulated blood was forced out of the urethra. He now was able to void a few drops of urine, and he had an inclination to do so every ten minutes. From this time he gradually got better, until he could make water in a very small stream, but he found himself much reduced. He continued in this state for about a month, when he consulted an eminent surgeon, under whose care he remained for two months, but no instrument could be introduced through the stricture into the bladder. He gradually got worse, and became so weak and thin, as almost to resemble a skeleton. Incontinence of urine came on, and he had no power over the bladder night nor day. The pain he suffered was so excessive that his mind became affected, and he lost his reason. He now was sent to the sea. His intellect and general health improved, but the stricture remained the same. On his return to London, he took quinine, and bougies were passed for him, but they could not be introduced into the bladder. He went to the sea again, and bathed, but derived no benefit. The incontinence of urine increased, and his sufferings were very great, having constant irritation in the urethra, and great pain in the loins and hips. In this miserable state, by the advice of Mr. Salmon, he came to me, and he got himself admitted into the St. Marylebone Infirmary, under my care.

When I saw him, nothing could be more wretched than his condition. He was extremely emaciated; he had a distressed and anxious countenance; his spirits were depressed to the utmost degree; he was feeble and tremulous; his urine escaped from him drop by drop, into a little leather bucket attached to the penis; his person was offensive both to himself and those around him, from a urinous fœtor; he had no appetite, and his pulse was quick and irritable. The bladder was tender on pressure, and the urine deposited a mucous-purulent secretion. On examining his urethra, I found a stricture situated five inches down the canal, which gave the sensation, when pressed upon, as if the point of the bougie rested against an indurated solid mass.

Jan. 12th.—The urethral perforator was passed down to the obstruction, and the blade of the lancet was thrust out at its point. About half an inch of the stricture was permeated. Only a few drops of blood followed the incisions, and no pain was given. A bougie was passed every day, by which the divided part was kept open.

18th.—The instrument was again employed, and nearly half an inch more of the stricture permeated. The same treatment was followed.

24th.—On this day the obstruction was completely perforated, and a catheter passed into the bladder. During the whole operation, at each time it was performed, it required considerable force to penetrate the indurated stricture. But little pain was given, and no bleeding of any moment resulted.

A catheter was left in the bladder for four days, but he could not bear it longer. Steel dilators were then passed every other day; and when the stricture had entirely disappeared, he was discharged from the hospital.

The general treatment of this case was strictly antiphlogistic. Leeches were fre-

quently applied to the region of the bladder: aperients, opiates, and emollients, were administered as the symptoms required them; and when the inflammation had subsided, tonics were taken with great advantage.

Remarks.—The distressing state to which this poor fellow was reduced was deplorable. The only treatment which could have been adopted for him, excepting perforation, was division of the stricture from the perineum; but it is doubtful whether, in the enfeebled state in which he was, he could have borne so severe an operation. It is most probable, from the great hæmorrhage that ensued after the accident, that the urethra was ruptured.

[To be continued.]

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'Alembert.

Outlines of the Anatomy and Physiology of the Teeth, and their Diseases and Treatment; with Practical Observations on Artificial Teeth. By D. Wemyss Jobson, Member of the College of Surgeons, Dentist in Ordinary to his Majesty, &c.

There is more practical information in this book than we have met with in any other on the teeth. Mr. Jobson, indeed, leaves few parts of his subject untouched, and has generally something valuable or original to say on all.

In the few following extracts, for which we must endeavour to find room, we have taken no pains to be select: we looked through the volume without any prepossession, but soon found ourselves interested with many passages. Most of our readers will recollect the difference of opinion set forth, not long ago, in this journal, relative to the order in which the temporary teeth make their appearance. Dr. Ashburner and Mr. Hunter expressed themselves distinctly on the question. Mr. Jobson speaks as follows:—

"The under teeth almost always perforate the gums before the upper ones; and in both jaws they advance in pairs, one tooth appearing on the right or left side of the centre line, and either accompanied, or in the course of a few days followed, by the corresponding one of the opposite side.

"A central incisor of the under-jaw is generally the first tooth that is obtained. It is usually observed about the sixth or seventh month, and is immediately afterwards followed by its fellow. The corresponding teeth in the upper-jaw make their appearance in the same manner, in the course of a few weeks.

"The under lateral incisors next advance, about the eighth or ninth month, and are soon afterwards followed by the corresponding upper ones. The whole of the incisors are thus usually seen when the child is nine or ten months old.

"The canine teeth, which in arrangement are situated next to the incisors, are so much more deeply placed in the maxillary bones, that they seldom make their appearance until the fifteenth or sixteenth month, and are preceded by the first grinding tooth, which is usually present on both sides, above and below, when the child is one year old.

"The second temporary grinding teeth are considerably later in appearing, and seldom perforate the gums until the end of the second or beginning of the third year. When these teeth are obtained, the temporary set is completed."

With regard to their removal, our author says:—

"About the fifth or sixth year of age, the roots of these teeth are removed by absorption; the dental absorbent vessels being stimulated either by the pressure or by the advance of the bodies of the permanent teeth. When the fangs are thus removed, the teeth, being deprived of their support, become loose, and eventually drop out. When this occurs, their necks have sometimes a splintered appearance, as if the teeth had been broken across; although in general the removal of the fangs is so completely effected by absorption, as to have given rise to the popular but erroneous opinion that the temporary teeth have no roots.

"Occasionally, however, the fangs of several of these teeth seem to escape the action of the absorbents, and these may then remain firm in their sockets for many years. This occurrence will, of course, prevent the corresponding teeth of the permanent set from advancing at the proper period; although they may

afterwards appear when the temporary ones have been removed in the course of time in the usual manner.

"I have no doubt that it is invariably from this occurrence that those anomalies of the appearance of new teeth at an advanced period of life proceed, which are erroneously considered as part of a new set. As an instance of this, I may mention the following case:

"About a year ago, I met with a gentleman, upwards of forty years of age, who still retained some of the temporary incisors of the lower jaw, and the canines of the upper. One of the latter required to be removed, on account of the pain which caries of it gave rise to; and its root was found to be quite entire and sound. I persuaded the gentleman, contrary to his own inclination, to have no artificial tooth put in its place; and the advice has been justified by the event, as he has recently informed me that a new tooth is now making its way through the gum."

It is well known that the occasional existence of a *third* set of teeth is believed rather implicitly by some persons. Mr. Jobson, as we have just seen, rationally and simply accounts for the phenomenon; but so firmly is he persuaded of the truth of his position, that we cannot help quoting his words, while we can scarcely suppress a smile at their force:—"The idea of the existence and advance of a third set of teeth after the second have been removed, is so diametrically opposed to every principle of physiology, that I shall not believe in it until it is substantially proved that the head will again spring from the shoulders after the individual has undergone the ordeal of the *guillotine*."

The remarks on the diseases of the teeth and their treatment, are full and satisfactory; and the suggestions relative to the proper use of the numerous instruments employed in dental surgery, cannot but be valuable to all students and junior practitioners. We will quote a few passages from what our author says respecting extraction of the teeth.

"The instruments that are required for performing extraction, are the Tooth-key, the Forceps, the Elevator, or Punch. An endless variety of form and modification of these instruments are to be found, almost every dentist having conceived it to be necessary to invent something of his own, and in numerous instances been influenced by the vanity of attaching his name to what he thinks proper to term 'improvements,' which, in nine cases out of ten, will be considered to be the reverse, by every one excepting himself.

"The Tooth-key is an old invention, or rather a modification of the ancient and obsolete instrument known by the name of the 'paces;' and although it is now the fashion to despise and abuse it, (especially with those persons, as Mr. Bell well remarks, who secretly resort to its use,) it is, unquestionably, still the safest, and, perhaps, also the most useful of all the instruments that are employed for the extraction of the teeth.

"The remark I have made of the inconvenience and annoyance which are occasioned to the student, by the suggestions of those persons who are so much more industriously engaged in altering and inventing instruments, than in qualifying themselves to use them, can nowhere be better illustrated than in the case of the tooth-key, which has, in many instances, been rendered almost wholly useless by their contrivances. No other example of this need be adduced than that afforded by the additional fulcrum, which was proposed some years ago, with the view of removing the pressure from the affected tooth, to the adjoining sound ones on each side of it. In consequence of this 'improvement,' it generally happened that these teeth were severely injured by the operation; and, I believe, it not unfrequently occurred, that the whole three teeth were turned out instead of one.

"The forceps is also a very ancient instrument, and is perhaps the most original of all, as it may naturally be supposed to have been the implement employed by those 'artists' who formerly took a share in performing this operation."

"The forceps is a far more difficult instrument to use than the tooth-key; although this will not, at first sight, be evident, on looking to their respective constructions, or attending to the mode in which they act; as the forceps removes the teeth perpendicularly from their sockets, and the key always withdraws them, in a greater or less degree, in a lateral direction. But it will soon be discovered on a comparative trial of

the two; and the operator will find, that, in applying the forceps, great caution is necessary, to avoid the danger of breaking the tooth, and considerable experience is required to enable him to use the instrument with facility. But when a command of it is once acquired, it is undoubtedly the most applicable of all those that are employed for the extraction of the teeth.

"The elevator, or punch, is the instrument required for the removal of the roots, when the bodies of the teeth have been destroyed by caries, or broken over, either by accident, or by ineffectual attempts at extraction.

"Many unsuccessful attempts have been made to construct an instrument by which the teeth might in every instance be perpendicularly withdrawn from their sockets. I have seen several contrivances for this purposes, and can give no better idea of their complicated structure, than by comparing them to a steam-engine on a small scale, or to the mysterious machine with which Hogarth decorates the studio of the charlatan in *Mariage à la Mode.* The formidable dimensions and appearance of such an instrument must always prevent it being generally used, or indeed ever being applied to a patient of ordinary delicacy. But even if these insuperable objections could be surmounted, there are many cases in which it could never be employed without increasing the difficulty and pain of the operation. The large grinders of the upper-jaw, for example, from the irregular form of their roots, and the mode in which they diverge, could scarcely ever be removed from their sockets in a perpendicular direction, without extensive fracture of the alveolar processes."

But we must recollect our limits, and leave the reader who is interested with what specimens we have here been enabled to insert, to follow up his acquaintance with the author in the volume itself. We regret that we have no room for noticing many excellent hints given in that part which treats of artificial teeth; we cannot, however, close our remarks without once more commending the candour and ability which are displayed throughout the work.

The Anatomy and Surgery of Inguinal and Femoral Hernia, illustrated by Plates coloured from nature, and interspersed with Practical Remarks. By E. W. Tuson, F. L. S., &c. Churchill, 1834.

The merit of this work consists in the execution of the plates; these have moveable parts, representing the layers of the different textures as they successively present themselves, and are so arranged as to shew the mechanism both of femoral and inguinal hernia in a very clear and satisfactory manner. They are thus calculated to assist in a remarkable degree the labour of the student; and though such plans ought never to take the place of actual dissection, yet the facilities they afford in conducting the dissections advantageously, and in impressing the relative position of the parts on the memory, are altogether unquestionable. A good digest of the most important facts connected with the subject is embodied in the text, and the whole is well deserving of encouragement.

MEDICAL GAZETTE.

Saturday, May 24, 1834.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

Cicero.

THE LATE ARCTIC EXPEDITION.

A report has recently been laid before the House of Commons by the select committee appointed to inquire into the merits and services of Captain John Ross in his late expedition to the Arctic Seas. We have perused the document with much interest, and have seen so many things in it which we thought might serve to excite, if not to satisfy, the curiosity of the medical reader, that we resolved to devote a short space in our journal to a notice of its contents. It is indeed one of the privileges of our profession, that it touches almost all the concerns of human nature in some point or other; and

in the temporary calm of medical politics, arising from the protracted labours of one Parliamentary Committee, perhaps it may not be amiss to direct our attention for a few moments to what has been done by another, concerning a subject of so attractive a nature generally, and having, besides, an especial reference to some topics of medical science, from its involving certain physiological facts of no small importance.

There is always something captivating in the story of labours passed and difficulties overcome: nor is the narrative of Captain Ross (so far, at least, as we can gather it from the minutes of his evidence) by any means destitute of attractions of that sort. Quite independently of the scientific importance of the late expedition, it is perhaps one of the most striking features of its history, that of the three-and-twenty individuals who composed the adventurous band,—who spent four years and a half in their undertaking (inclusive of four successive winters), in the endurance of hardships such as have fallen to the lot of few persons for so lengthened a period,—all returned, with the exception of three, two of whom died natural deaths, as they no doubt would have done had they remained at home, and one alone perished from the severity of the climate and other influences. This fact of itself should give a prominent degree of interest to the circumstances of the expedition; but we must enter a little more into detail.

Having sailed from England in May, 1829, the party made for Prince Regent's Inlet as directly as possible, that being the route by which the attempt was this time to be made to determine the question of a north-west passage. They reached the Inlet in August, and were soon in a track hitherto unexplored. It will be remembered that it was here that Captain Parry was obliged to turn back on his last voyage in search of a north-west passage in 1824-5, having lost one of his vessels (the Fury) on the western side of the Inlet. We will not follow Captain Ross in his progress into the Gulph of Boothia, nor pretend to describe the continent and isthmus, and the large number of islands, rivers, and lakes, which he is said to have discovered, for we have not the means of tracing him satisfactorily; nor indeed is it our purpose to give a regular summary of the voyage: we hasten to notice some of the circumstances which are more in our way. The first winter was spent in comparative ease, in Felix Harbour; but another and another followed—those, namely, that were spent in Sheriff's and Victory Harbours—which were amply sufficient to put their endurance to the test. In January 1831, the thermometer sometimes stood as low as sixty degrees and a half below zero, or ninety-two degrees and a half below the freezing point of Fahrenheit; and in those two winters of 1830 and 1831 generally it may be observed, that so intense was the cold, that the thermometer was frequently as low as eighty, and occasionally at eighty-five degrees below the freezing point!

At the last-mentioned station they were obliged to abandon their ship; and as their only chance of reaching a place of shelter, in the prospect of spending another year in these regions, they determined, in May 1832, when driven nearly to the last extremity, to proceed over ice and frozen snow, a distance of 200 miles, between the 70th and 74th degrees of latitude, to the place where the Fury had been wrecked, but where they had reason to expect they should find the provisions which had been saved from that vessel. This distance of 200 miles, which seems to have been made nearly 300, in the anxiety of the party to leave nothing important unexamined, was effected under circumstances of extreme suffering; however, by an economy next to starvation, and by the

most cautious management, they succeeded in attaining their object. We learn some extraordinary facts relative to this part of their history from Captain Ross's evidence. The manner of their bivouac, it seems, was this. The party, which then consisted of twenty-one, was divided into three companies of seven each, under the care of an officer. Three trenches were dug at night in the snow, each large enough to contain seven people; every man was provided with a sack made of blanketting, and sometimes lined with fur; into this he got at night, and stowed himself along with his six companions, in the trench, where though laid on the frozen snow, yet by their mutual warmth they contrived to pass some hours in sleep tolerably enough. These sacks were secured so as not to allow any of their limbs to be drawn out at night; and even the face was provided with a covering necessary to protect it from frost-bite.

The cold on this journey was sometimes at thirty-two below zero, or sixty-four degrees below freezing of Fahrenheit; and to add to their suffering, worn down as they must have been by the hardships of three winters, the fatigue of dragging themselves along was not a little aggravated by their being burdened with their tents, their collections of specimens, their fuel, and what provisions remained to them. By the time they reached Fury Beach they were reduced, even on their scant allowance, to their last day's provisions; and we may very well conceive the dreadful alternative, had they reached the spot where they hoped to find the means of sustenance, and been disappointed. Such a shock, in the necessarily depressed state of their minds, must have been attended with the most deplorable consequences.

To the spirit and energy of Commander James Ross, on this occasion as on several others, the people were much indebted. Taking with him two of the strongest of the party, he pushed on to the place where the stores of the Fury were expected to be found. "I advanced," he says in his evidence, "to see whether they were there or not; for had the party gone there without finding the provisions, the whole, or nearly the whole, of them must have perished." To the experience, indeed, and the indefatigable activity of this gentleman, the success of the late expedition (so far as it has been successful) must be mainly attributed. There is, we believe, no other individual who has had such ample opportunities of acquaintance with the Arctic and Polar seas: he has now been out in six voyages to the northern and north-eastern districts, beginning with his uncle's first in 1818, and he has spent fourteen summers and eight winters in those inhospitable regions of the globe. The attainments, moreover, of Commander Ross, as a man of science, are of a very high order, and, we are glad to perceive, are appreciated in the proper quarters.

Soon after reaching Fury Beach, it was attempted to cross the inlet, and to get into Lancaster Sound, where there might be a chance of being relieved from their perilous circumstances, perhaps, by some whaler: but the attempt was unsuccessful; the ice remained unbroken during the whole of that summer; and their only chance of surviving now was to make the best arrangements they could for spending the winter at the Beach. The history of this winter is without a parallel in the annals of navigation; for all who have ever been reduced to such straits after the wreck of their vessel, have miserably perished. Sir Hugh Willoughby's story is well known: himself and crew all perished *;

* "In these fell regions, in Arzina caught,
And to the stony deep his idle ship
Immediate seal'd, he with his hapless crew,
Each full exerted at his several task,
Froze into statues; to the cordage glued
The sailor, and the pilot to the helm."

THOMSON; *Winter.*

and the Russian party at Spitzbergen were also found dead from cold. Here, then, we have a general fact of very high importance to all who are interested in ascertaining the powers and resources of the animal economy: it is demonstrative, at least, of the great improvements made in recent times in the proper management of such difficulties. We cannot but be struck when we read the narrative of the comparative immunity of Captain Ross's party from the appaling consequences of spending a *fourth* winter in such forlorn circumstances, and recollect the frightful mortality that has ensued from a single winter's hardships, in modern times, in Europe. Not to recur to the celebrated retreat of the French from Moscow,—the fact is on record that Charles the Twelfth lost 2000 men by cold, during a siege in the winter of 1709.

The preservative means employed by Captain Ross and his companions chiefly consisted in adopting a proper mode of ventilation, and in protecting their shelter by a thorough coating of frozen snow. Their habitation, which was constructed of a frame of spars, 32 feet by 16, covered with canvas, was coated with snow from four to seven feet thick, which, being saturated with water when the temperature was 15 deg. below zero, immediately took the consistency of ice; "and thus," says Captain Ross, "we actually became the inhabitants of an iceberg during one of the most severe winters hitherto recorded." Of the importance of a thorough system of ventilation in such circumstances, in order to prevent that accumulation of heat and moisture which would be so much to be apprehended in a surrounding atmosphere so exceedingly cold, we must at once be convinced; of its absolute necessity we must be made aware, by a passage from Parry's account of his third voyage. "The want of a constant supply of warmth," says Captain Parry, "is here immediately followed by a condensation of all the moisture, whether from the breath, victuals, or other sources, into abundant drops of water, very rapidly forming on all colder parts of the deck. A still lower temperature modifies, and perhaps improves, the annoyance, by converting it into ice; which again an occasional increase of warmth dissolves by converting it into water. Not only is a moist atmosphere thus continually kept up, but it is rendered foul by the want of that *ventilation* which warmth alone can furnish. With an apartment in this state, the men's clothes and bedding are continually in a moist and unwholesome condition, generating a deleterious air, which there is no circulation to carry off; and whenever these circumstances combine for any length of time together, so surely may the scurvy (to say nothing of other diseases) be confidently expected."

Another very important preservative was the perfect abstinence of the whole party (through sad necessity, indeed, for the most part) from every thing in the shape of spirituous liquor, or stimulating beverage of any kind. They were in this "temperate" condition for the last fifteen months; and to this Captain Ross seems, not improperly, to attribute much of the safety of the crew. They had cocoa, indeed, till within the last six months, and "coffee of burnt peas;" but latterly their drink was entirely confined to water and lime-juice. The water, too, by the way (which added not a little to their discomfort), had always to be procured for immediate use by melting it from ice; a process which rendered it necessary for them never to be unprovided with fuel.

Having spent eleven dreary months in this way, at Fury Beach, they were at length, in July 1833, enabled to leave the spot; and, carrying with them three sick, who were unable to

walk, they, in the course of eight days, reached their boats. They were, however, unable to cross Prince Regent's Inlet till the middle of the following month; when, after proceeding eastward for a considerable distance, along the beach and across some minor inlets, they were picked up at last, to their indescribable joy, by the Isabella, of Hull. They were reduced at this time to the most miserable allowance; and their three invalids must have died in a fortnight, had they not been providentially saved by the whaler: the whole party, indeed, "were nearly exhausted by fatigue, hunger, and exposure in the boats."

Such are the general facts which serve to form some sort of outline of the proceedings of this expedition,—an expedition, be it recollected, almost of a private nature,—nearly the whole of the expenses incurred by it being defrayed by the munificence of a private individual*. There were accordingly none of those comparative luxuries here enjoyed, which solaced the navigators in former enterprises equipped by Government: yet the most abundant provision against difficulties and dangers could scarcely, perhaps, have procured more satisfactory results — considering the route by which it was previously arranged that the party should proceed.

The maintenance of discipline during the expedition was equally creditable to all parties. Some slight circumstances of insubordination, indeed, at one period made their appearance; but this is scarcely to be wondered at; and they were so trifling as to require only a little firmness on the part of the Captain, to restore tranquillity again.

We look forward with some impatience for the detailed narrative of the enterprise which is in preparation, by Captain Ross and his nephew. We should hope that the surgeon of the expedition has not neglected to preserve ample notes of the many facts in physiology, pathology, and natural history, which must have come under his observation. From the letter of Capt. Ross to the Admiralty, we learn that Mr. M'Diarmid acquitted himself in a very superior manner: he is said to have been "successful in every amputation and operation which he performed, and particularly so in his treatment of the sick;" and in consequence of this recommendation, we are glad to perceive that he has since obtained an appointment in his Majesty's service. The mention of this surgical and medical practice, conducted as it was under such peculiar circumstances, suggests to us many points on which we would gladly make inquiry; but we must at present hasten to conclude.

In the preceding notice we have abstained altogether from touching on the important advantages which, it is said, have been secured to science by the late arctic expedition. We have purposely omitted to allude to the benefits derived from it, more particularly to geography, meteorology, and magnetism,—or that modification of electricity, so called; though we might readily have extended our remarks by offering our readers a popular account of what has been done in these several respects. With regard to the great object of discovering a north-west passage, we might have dwelt on the importance of the effective search which has been made by the route of Prince Regent's Inlet, and the result of which seems to be perfectly—we might almost say *satisfactorily*—negative. We might, above all, indulge ourselves, with no little gratification, in noticing the circumstance of the *actual discovery of the magnetic pole*—that remarkable point on the surface of our globe where the dip is 90°,

* Felix Booth, Esq.; to whose "modest public spirit and rare munificence" the Parliamentary Committee "cannot forbear offering the tribute of their admiration and respect."

and the horizontal needle is destitute of the power of traversing to any particular point. But we are rather willing to confine ourselves, as we have done, to what we consider the more legitimate business of our vocation: we turn our eyes more especially to the contemplation of what benefits may possibly result from the enterprise to medical science; and we cannot help thinking, if the details have been properly recorded and preserved, that several branches of medicine will have been materially benefitted by the observations made on this expedition. Human life, as we have seen, has been preserved, in the most untoward circumstances, by a system of management the details of which it would be highly interesting and important to know. And in this point of view alone, independently of the general interest attaching to it, we have thought it desirable to bring the subject before our readers.

BIRMINGHAM *versus* BRISTOL.

We published, a few weeks ago, a memorial from the medical officers of the hospital at Bristol to the Parliamentary Committee, requesting that their pupils should be admitted as candidates for examination at the College of Surgeons, without having attended at all the practice of any hospital in London. This document the parties sent to various provincial towns and elsewhere, praying their brethren to support them. The medical officers of the Birmingham Hospital have expressed their determination not to co-operate in any such cause, as they look upon the short attendance required at a London hospital as a most important part of the pupil's education, and think that the College has behaved with sufficient liberality towards the provincial hospitals in limiting the metropolitan attendance to six months. They are also of opinion that an injury has been done to the profession by that regulation of the Apothecaries which dispenses with the necessity of any attendance in London.

ROYAL INSTITUTION.

Friday, May 9, 1834.

DALTON'S DISCOVERIES AND RESEARCHES.

It having been announced on the preceding evening of meeting that the venerable and distinguished philosopher of Manchester, Dr. Dalton, was to give an account of his discovery of the Atomic theory, the theatre was completely filled with members and visitors anxious for so interesting a communication. Nor was the general expectation disappointed: Dr. Dalton not only explained what were his earliest views on the subject of the intimate constitution of chemical substances, but related what might be considered as a sort of history of his own mind, from the date of his primary researches in philosophy up to nearly the present time. In early life, he said, he was pretty well grounded in mathematics and the principles of mechanical philosophy, which are so necessary for those who would make a progress in natural science. It was in meteorological researches that he was at first principally engaged. While living at Kendal, in Westmoreland, about the year 1787, his attention was much attracted by certain appearances of the aurora borealis: these he began regularly to note down: then added other observations: and in short, from that time to this, he has been in the habit of keeping various scientific journals. In the year 1793, he published a volume of Meteorological Essays, in which he gave a special account of one of the most remarkable phenomena he ever witnessed: it was that of the sudden illumination of the heavens one evening by a large dazzling ball of light, which proceeded with great rapidity from north to south, leaving behind it as it were a number of rods of light, all verging nearly vertically to a point which he had reason to suppose was the magnetic north. On this phenomenon he dwelt with peculiar animation. The volume just mentioned contained also some remarks regarding the mode in which vapour exists in air.

By a set of experiments with an air pump, to which he had access, he satisfied himself that steam or vapour is only in accidental connexion with the atmosphere, or that it is not necessarily present as a constituent of air. He performed

some experiments here, in the Royal Institution, in the year 1804, illustrative of this subject; and he placed beyond a doubt the possibility of the existence of vapour with all its ordinary properties in a Torricellian vacuum. For one essay, which was published about this time in the Manchester Transactions (Vol. 5, Part 2) he took much credit to himself: it was that in which he proved that cold was produced by the rarefaction of air—not by the change of pressure; and his reasoning was founded on the observation of the fact, that on the abstraction of air under the receiver the temperature was suddenly lowered 3°, while by its admission it was raised 3°. He also noticed other facts which engaged his attention about the same time; among others, the formation of a slight cloud under the receiver when two or three smart strokes have been given with the pump: this he accounted for by the sudden cooling of the vapour which was present.

About the year 1805, he and Dr. Henry, of Manchester, were engaged, but independently of each other, in ascertaining the quantity of gases contained in water. It was soon known that different gases are contained in water in different proportions; while the quantity of common air that may be contained in the same fluid is directly proportional to the pressure employed upon it. Dr. Dalton was chiefly occupied with carbonic acid gas; and among other deductions and results arrived at, he found out the reason why heat does not ascend and accumulate in the higher regions of the atmosphere. He now conceived the idea that elastic fluids are composed of distinct particles, with an atmosphere of heat around each: and in some of his earliest experiments to find the relative weights of the atoms of two elastic fluids in a state of combination, he ascertained that a pound of hydrogen required seven pounds of oxygen to form eight pounds of water,—whence he assumed the relative weights of hydrogen and oxygen to be as 1 to 7. He then proceeded to explain his reasons for assuming that the atoms united in this instance, one atom to one, and not one to seven, as some objectors suggested: and he drew his principal arguments from what takes place in the combination of the constituents of salts, and especially in the union of the metals with oxygen. It was in this very theatre that he first demonstrated with satisfaction to himself (in consequence of the excellent diagrams which he found here) his views of the chemical constitution of bodies: and some of these demonstrations he now repeated, with regard to the several oxides and acids formed by the union of carbon and oxygen, and azote and oxygen. The great obstacle, he thought, to arriving at the doctrine of definite proportions sooner, arose from the bad method adopted by chemists in taking notes of their analyses: they almost always proceeded to state how many parts of the constituents they found in 100 of the compound; whereas, if they had taken the quantity of oxygen at 100, they could not but have been struck with the remarkable analogies in their registered results. Finally, he expressed his opinion, that chemists did wrong in neglecting to note the *number* of atoms, as well as the atomic weights, in compounds which they analysed; for without this, he said, they only did half their work.

In conclusion, Dr. Dalton reverted to some of his meteorological speculations. He formed a theory many years ago relative to the phenomena of thunder and lightning,—or more particularly with reference to the mode in which the sounds proceed from the successive explosions of thunder; and in 1819 he had an opportunity of verifying it in a very striking manner in the neighbourhood of Stockport. But he found since, that he had been anticipated on this subject by Beccaria, as appeared from Priestley's History of Electricity. The meteor which he observed in 1783, Dr. Dalton calculated to be 80 miles high, and the noise which accompanied it, as it proceeded along in its rapid course, seemed to identify it with electric fire,—its rushing sound, indeed, readily admitted the same principle of explanation.

The learned lecturer was heard throughout with the greatest attention, and both at the commencement and conclusion was greeted with loud applause.

CLINICAL LECTURE

ON

DISEASES OF THE HIP-JOINT,

Delivered at the Middlesex Hospital,

BY SIR CHARLES BELL.

You follow a lecture delivered as part of a regular course of surgery with great ease; the subject is laid down, and systematically pursued. If it treat of disease, it is described, its origin investigated, its termination, its consequences, narrated, and then its treatment. In all this there is no distraction; no difficulty occurs to you; the practice of your profession appears very simple; and thus you are de-

ceived into the belief that you know your duties, and you discover your defects only amidst the painful doubts and difficulties of practice. What is it, let me ask, that distinguishes one practitioner from another in eminence? It is not so much peculiarity of practice, as being able to distinguish the disease, or the nature of the accident; it is in this, principally, that correct professional knowledge is invaluable.

But now, in going round the house, you see a variety of accidents and diseases of the hip-joint. The difficulty which you will have in practice hereafter, is not to say what shall be done in a certain case, but to ascertain the nature of the case. The diagnosis is the grand difficulty; and I would require no better proof to give to any person of the interest that I take in you, and the desire I feel of being useful to you, than that I should return to this subject of the hip-joint; for, be assured, there is none that you will find more difficult to investigate, or a greater source of anxiety and mortification to you.

In the first place, is not this neutral ground? Are not the surgeons of this hospital often called into the physicians' wards? — how often do diseases which have nothing to do with the hip or thigh, appear to be affections of those parts!

Let this be the first head which you are to inquire into—*pain in the hip*, and *debility of the extremity*, when there is no disease there. You are aware that pain, arising from disease of the great sciatic nerve, as it passes near the hip-joint, and disease produced by disorder in the viscera of the abdomen or pelvis affecting that nerve, must be very apt to be mistaken for inflammation of the hip, involving the same nerve. So that whenever you find a patient with pain in the hip, the very first thing you have to inquire into, is, whether this be not symptomatic of internal disease; and I would remind you, that it is not the more formidable disease of the viscera which produces this affection of the nerve, but rather disordered function.

The next thing you have to consider, is the lesser degree of pain in the hip, which still proceeds from disorder in the lower part of the canal; for example, accumulation in the colon will produce pain in the hip, which may be mistaken for disease in the joint. The nerves which go to the internal part have branches which run externally over the ilium; and the nerves which pass over the ilium to the hip, and those which pass anteriorly to the groin, are often affected by disorders within—disorders of the bladder, of the kidneys, and of the rectum. Sometimes a patient will merely have piles; he will not tell you of them, but complain of these pains in the hip, thus concealing the cause of the sympathetic pain. So again, a man may labour under stricture; the stricture affects the bladder; the affection of the bladder will be shewn by stiffness and pain in the loins and the hip. You cure the man of his stricture; and when the bladder dilates freely, and the kidney regains its functions, he will get rid of what he has called his *lumbago*, and the stiffness in the hip-joint; and so far from walking double, or stooping and being unable to rise again, he is free from pain, and capable of free motions in the hip and loins.

Let me repeat, then, that whenever you come to examine diseases of the hip, the first thing suggested to you is, whether the affection be actually seated there, or whether it be a pain symptomatic of internal disorder.

The next head for your consideration is one that must have occurred to you from what you have seen in this hospital—I allude to hysterical affections. You have just observed how a patient lies with disease of the hip; and do you not remember a young woman lying precisely in that position, in whom there was great difficulty to discover whether there was actual disease or not, in whom the pelvis was pitched obliquely, as if there were disease in the hip? But then there arose a class of symptoms which pointed to the right source—a singular contraction and retraction of the leg, so that the knee was bent almost to the bursting of the ligaments, and the foot turned in so extraordinary a manner, that the great toe lay close to the anus! The retraction was so powerful, that we naturally apprehended that the ligaments of the knee-joint must be destroyed. It proved to be a case of hysteria; and what was extraordinary, was the resemblance it had in every feature to the disease of the hip-joint. I think there is now a young woman in Handel's ward with disease of the knee-joint, in which there comes on suddenly an extraordinary distortion of the foot, in consequence of the flexors and the tibialis anticus acting together. This also we must class as hysterical, although, I confess to you, this is not quite satisfactory to my mind; it is more like a cover for ignorance than a solution of the difficulty. It is certainly well worthy your observation, that this contraction, which on this occasion we have seen occurring before our eyes, is that condition of the foot which we often see as an obscure and permanent defect, where the peronei muscles are rendered paralytic, and the tibialis anticus, flexor pollicis, and flexor digitorum, are spasmodically power-

ful. It is that condition of the foot which is found in children at birth, causing the club-foot.

I will not dwell upon the next point, which is psoas abscess. You must take care, however, to distinguish psoas abscess from disease of the hip-joint; and you have the more difficulty, because both diseases belong to the same condition of the general system—a strumous habit. You have visited several cases with me, when I have not concealed the difficulty that I have had in saying whether the complaint was in the hip, or in the loins and the course of the psoas muscle.

Among the somewhat puzzling varieties of scrofulous disease of the hip which I have pointed out to you to-day, the little girl who lies opposite the door in Handel's ward is not the least. Her right leg is swollen from the hip to the toe, and not with such œdema as you see in some other cases accompanying violent inflammation. It is firm, and the cutaneous veins are turgid and blue. By and by there is presented in the left groin an internal abscess, to all appearance a formidable lumbar abscess. This has been treated, punctured and closed, and punctured again; and I think, greatly to my relief, that this child will survive. To what, then, should we attribute this appearance of hip-joint disease? I think to glandular enlargement within the pelvis, and to pressure upon the iliac vein; but we may entertain a surmise that this swelling is owing to the inflammation and choking of the vein.

The next case is one of great interest, because it is of such frequent occurrence—I mean the blighted limb. The simple fact, as it occurs, is this:—A young person, or a child, has some affection of its bowels, some disorder of the intestinal canal; it may be at the period of teething or weaning, and it is marked by green purging. There is an immediate effect upon the limb; for a time it does not grow, and it remains the limb of an infant in the body of a growing youth; or it grows so slowly as not to keep pace with the other limb. Often it is noticed after the boy has been some time absent from his parents: perhaps the boy has been sent to school; and when he has come home, after an absence of some months, in running across the drawing-room to his mother, she says, "Child, you are lame!" He insists upon it "that there is no lameness; he has played at cricket with the other boys, and he has no pain." But the mother is alarmed, and calls you in to see what is the matter. When you lay the boy down on the carpet, and measure the limbs, you find that there is one really shorter than the other; and when you take a cord, and measure the circumference of the knee and ankle, you find that the affected side is smaller in every part. Then placing him before you, you find a flatness in the hip, not indicative of inflammation of the hip, and wasting of the gluteal muscles from that cause, but proceeding from a defect of growth, or a want of perfection in the gluteal muscles of the hip.

Now it is a sad mistake to put this child under treatment for inflammation of the hip,—to make an issue in the hip under the idea that there is hip disease, when, in fact, it is a state of the limb arising from some strange and unascertained condition of the nervous system; so that either no part of the limb grows, or it does not grow in due proportion to the rest of the body; and consequently, while other parts are developing themselves, and acquiring a manly character, this remains the limb of a boy.

These remarks have been preparatory, and with a view to show you the importance and the difficulty of the subject; we now come to actual diseases of the hip-joint.

The first point upon which I shall touch is one which, if you are not upon your guard, may injure your character in practice, and of course give you great vexation. A man is perhaps thrown from the top of a coach; you are called to him,—you examine him carefully,—you adopt all the means for ascertaining whether the pain and lameness, consequent upon the accident, arise from dislocation; you use all the means by which you distinguish fracture of the neck of the bone from dislocation; and you satisfy yourself that the limb is of the proper length,—that there is no crepitus,—that, in fact, there is neither dislocation nor fracture. We will pass over months of suffering and confinement; and let us suppose that the patient is now beginning to creep about. Another practitioner sees him,—talks to him, and says at once, "How your case has been mistaken! How is it possible that they should not have seen that that leg is shorter?" He then tells him that he has a dislocation, and that the head of the bone has been so long out of the socket, that it is irrecoverable. Are you aware, then, what has happened? Are you aware that this medical person, not to say *friend*, may be mistaken? It may not be jealousy; it may not be ill temper; it may be purely a mistake. How, then, does the mistake arise? That is at least the most generous interpretation.

Now there are two circumstances that you will please to remember. When a person falls upon the great trochanter, and the head of the bone is thrust

in upon the acetabulum, and inflammation slowly arises in the joint,— I say there are two reasons why, when the patient begins to walk, the leg not only appears, but really is, in a slight degree shorter. The first is, the wasting of the head of the femur and the neck of the bone: for, by this inflammatory process, it sometimes happens that instead of the head of the bone neatly corresponding with the natural depth of the acetabulum, a change takes place in both surfaces; the socket expands, and the head of the femur is moulded to a broader shape, with extended margin and shorter cervex: at all events, the head and neck of the bone are partly absorbed and shortened. There is a check in the motion of the thigh bone; it becomes limited; and if you consider the obliquity in the neck of the bone, you will see why there is a degree of shortening. The next thing to be considered is, that while there is this inflammation, and these changes are proofs that there must have been pretty active inflammation going on in the hip-joint, the pelvis is tilted up, attended by a peculiar oblique position of the spine; and these two circumstances combining, during all the time that the patient is confined to his bed, no wonder that when he rises, and puts his foot to the ground, that leg is shorter than the other: and if your new friend and consultant does not ascertain the actual position of the pelvis, and measure the degree of obliquity, he will say at once the heel does not touch the ground; the leg is short, the case has been mistaken!

I trust that this is not too tedious a description of what may occur in your practice without any fault of your own. I mean, by this statement, to advise you to be circumspect when called to an accident of this kind,—to explain to some person of good understanding, or of some authority, the nature and probable effects of the injury, that he may observe the care with which you measure and attend to the position and relation of the parts; and a few words said in anticipation only adds to the confidence your friends place in you; whereas the same explanation coming as an apology, answers no purpose at all.

I shall not enter into the history of these complaints, but point out to you some of the distinguishing symptoms.

In respect, then, to diseases of the hip, I am sure you have seen enough to convince you that it is a most important subject. You see how many cases are brought into this hospital of scrofulous disease in the hip-joint; and the first thing you should notice is, that there is acute and chronic inflammation of this joint,—that the acute form very often terminates in the chronic; and that there is a curious character in both varieties, a deceptive appearance, to which I have already alluded in speaking of the *pitch* or *tilt* of the pelvis.

We shall pass over the account of the disease rapidly. You see its progress; you see that the inflammation in the hip-joint produces abscesses exterior to the joint; you see that these abscesses are sometimes very extensive, — that they either open spontaneously, or are opened by the surgeon,—that hectic fever follows, —that the patient is reduced, and dies hectic. Sometimes, however, the patient surviving the attack, there is caries of the bones, absorption of the head and neck of the femur, and, not unfrequently, absorption of the bottom of the acetabulum; so that I have found the head, or rather the narrow absorbed neck of the bone, actually passed through into the pelvis, and irritating the bladder and parts within, by its motion. What, then, is the termination of this case? There is generally a serious injury to the joint as a mechanical apparatus, and very often the disease terminates by a sort of imperfect anchylosis; that is to say, the head of the bone and the neck are so far absorbed, that the trochanter touches the margin of the acetabulum: this limits the motion; and as soon as the motion is thus limited, the great source of irritation is removed, and the patient is cured, with a contracted stiff limb, which causes him to move most awkwardly; and sometimes the thigh stands directly out before him, in a very unseemly manner.

But the symptom which I shall most press upon your attention is the pitch of the pelvis; and I must say, for the hundredth time, that the anatomy and surgery ought to go together: it is impossible for a teacher to separate them. I very well remember, that while teaching anatomy with my late excellent colleague, Mr. Wilson, when he heard how I was engaged, he would say, "I keep my surgery to the end of the course:" but it is not possible to separate the two departments; for either you must have the anatomical demonstration before you, in order to exemplify the principles of surgery, or you must introduce the practice of surgery in explanation of the importance of the demonstration.

Now for an example with regard to the thigh-bone. It is not possible to give that degree of interest to the subject, and at the same time to afford that easy and ready explanation, while treating it, without a demonstration of the parts before us. How easy and pleasant it is to explain all this subject when you have the skeleton before you, and the dissection of the psoas and iliacus internus muscles! It is the action of these two muscles which explains almost every

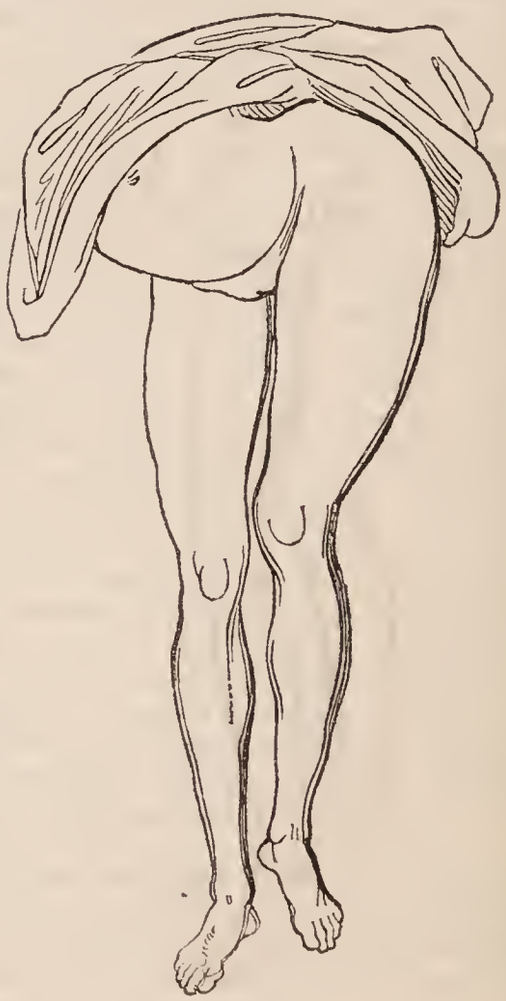
thing that seems at first obscure in regard to the position of the limb in the hip disease. The iliacus internus and psoas magnus on the fore part, and the glutei on the back part, are balanced; when, however, you stand, nature intends that the muscles which are descending from the pelvis, and from the spine on the fore part, and going into the trochanter minor, should sustain the whole weight of the body: they are powerful for that object; the body hangs back upon them; they are the muscles that throw forward the body, and that raise the thigh-bone. You have seen how it happens in amputation, that the moment you cut off the weight of the limb that is acting against the muscles, the stump immediately cocks up; and precisely the same occurrence takes place when the bone is fractured; for the femur is held in its position against the action of the iliacus internus and psoas magnus by the weight of the limb. As you lie in bed, it is the weight of the limb that acts against these muscles; but when the bone is broken, the counteracting power is gone, and these muscles raise up the upper part of the bone: so that in my collection I had twelve specimens, placed in a frame together, of fracture of the thigh-bone, and every one of them more or less, but some of them in a marked degree, were united with the upper portion sticking in front of the lower portion.

Now, whether you have to place the stump in amputation, or to set the limb in fracture of the thigh-bone, you have to counteract the action of these two muscles; so that you raise the body a little, or raise the limb. You cannot by any force bring down the fractured extremity of the bone; you cannot apply force to an inflamed and injured part; but even if there were neither pain nor inflammation, you could not apply your compress and bandage with sufficient power to press down the upper part of the thigh. What can you do, then? The only resource is to bring the inferior part up to the level of the upper part; and that you can easily effect.

Again, to bring the action of these muscles in explanation of our present subject. You remember how closely the tendons of the psoas magnus and iliacus internus are connected with the ligaments of the joint. Conceive the ligamentous apparatus within, to be inflamed; can the patient in that inflamed condition stretch out the limb? No; because then the muscles and their tendons press on the inflamed parts. In order, therefore, to relieve the inflamed joint from the pressure of these muscles, the patient lies with the limb bent, with the knee raised towards the abdomen. The next thing to be noticed is, that all the motions of the limb are attended with more or less pressure of the head of the femur upon the acetabulum. The individual lies in bed in the position I now place myself [the Professor here threw the one thigh over the other, so that the weight of the lower part of the limb should tend to lift the head of the femur out of the socket], and then the whole weight of the lower part of the limb and of the femur is poised upon the other thigh, as upon a fulcrum, and the lower part of the limb being heaviest, the head of the bone is thus raised, as it were, out of the acetabulum; in other words, this position saves the joint from the painful pressure of the head of the bone.

Thus you perceive the reason of the peculiar position of a person lying in bed with an inflamed joint.

When you have some difficulty in ascertaining whether the disease be in the hip-joint or not, you look to the position of the foot; you find the heel is short of the other; you conclude that there cannot be a shortening of the leg so rapidly; you then take a cord, and place it across from one anterior superior spinous process of the ilium to another, and then you find that the pelvis is pitched; that it is oblique. Why? By the process of reasoning I have offered you, you see why: it is to relieve the inflamed parts, and thus the obliquity of the pelvis comes to



be an indication of inflamed hip joint. This obliquity will go on increasing until you shall see the body of the child actually begins at an angle of 45° with the thigh: the limbs may be lying straight in bed, and the body thrown altogether over; so that in acute inflammation of the hip joint dislocation will actually take place.

Do you understand what is meant by the French term *dislocation consécutive*? They must have mistaken the disease: I have no doubt of it. Here is a drawing [see preceding page] where you would scarcely suppose, without due study, that such a shortening of the limb could have taken place without dislocation. It is the case of "Ann Palmer, æt. six, late in Northumberland ward. She went out last Tuesday, after having been seven months in the hospital. The case was simply this: she sprained the hip in jumping, some time previous to her admission. She was treated with issues, and is now well enough to be taken home, though not entirely free from pain. The pelvis is much tilted; so that when she is made to stand, she cannot touch the ground with the foot of the affected side without bending the other knee; so much longer does the sound leg appear to be than the diseased one. The spine is much distorted from the position in which she has lain so long."

The next case is that of "William Maxy, æt. five, admitted into Northumberland ward, January 14th. The position is remarkable. The pelvis is very much tilted; he lies on the sound side; the knee is straight. The diseased leg is apparently much shorter than the other, and the knee of it low, so as to raise the head of the femur out from the acetabulum."

Another case is that of James Dixon, æt. six years and a half, admitted into Northumberland ward on the 23d of July. The position of this poor child is exactly the demonstration that I wanted, in order to show how one leg is thrown over the other, to give weight to the lower part of the limb, and to raise the head of the bone out of the acetabulum.

I now put into your hands a card containing a sketch of the position of Dixon;

and on the reverse you will find another figure representing George Newman, late in Hertford Ward—now an out-patient.

[This case and figure being analogous to some of the others, we have not thought it necessary to insert it here.]

Here is another case [presenting a sketch], that of Emma Panzetta, æt. 11, admitted April 4th. "She has long been unable to lift the left foot, but shuffled along; and when she sat, sat on the corner of the chair, resting on the sound hip."

Now it is really necessary that you should understand these cases, otherwise you will fall into a careless habit, and consider that the position is all accident. Be careful, not only in physiology but in surgery, to avoid saying that an occurrence is accidental; it is a lazy, indifferent way of looking to a subject. This position is not accident; there is a cause for it, and it is your business, as intelligent students, to understand the cause.

The question, then, is, how does it happen that in hip-joint disease the limb is generally shortened, but sometimes lengthened? The cause is the same in both; they seek the position of ease: that position in which the patient suffers least pressure to the inflamed parts, and experiences least pain; it is that which induces him to lie in the particular manner. If the patient be taken due care of, and put to bed early in the disease, the leg will be shorter; but if the disease be of a more chronic nature, so that the person is permitted to go about, the leg will be longer; for see what takes place. Suppose the person is walking with an inflamed hip, it is quite clear that the weight of the body must be thrown on the other hip, in order to relieve the affected side; and when he moves along, he dare not bring the diseased hip exactly under the centre of the body, but he pushes it forward whilst he bears on the other limb. This gives rise to a curve in the spine. When you lay him down and measure him, the spine being curved, that diseased leg appears longer; and this is owing to the position of the pelvis, which is poised differently: the diseased side is depressed, instead of being elevated, as in the other case. Then, again, if he be sitting, he will sit upon the side that is free from disease; and here also is a thrusting down of that side of the pelvis; and then, on laying him down and comparing, you find that the heel of the sound leg touches the calf of the lame one. This is still owing to the position of the pelvis; for the spine will accommodate itself to the pelvis, and the latter, therefore, will be permanently tilted in a different direction to that I before described, and there will be all the appearance of a long leg instead of a short one, accompanying this same disease of inflammation of the hip.

I conceive it to be of great importance that you should look on these things, not as accidents, but matters on which you ought to reason. You should take up all the facts, and then reason upon them; by which you will acquire that real knowledge that will prevent you being led into mistake in forming the diagnosis.

FRACTURE OF THE NECK OF THE THIGH-BONE.

We now come to consider fracture of the neck of the femur, as distinguished from other accidents to the hip. Let me advise you, whenever you go to visit a patient who has received an injury, not immediately to interpose your hand; because, as soon as you have thrown off the clothes, looked at the limb, and begun to handle it, the relatives most anxiously contemplate your countenance, to know your opinion, and you seem to hesitate; which is giving needless anxiety to them, and doing no justice to yourselves. Reasonably you cannot yet have formed an opinion, which seems to them strange; and so they form an idea that you are not quick, or decided. It is a merit with sensible men of your own profession, that you hesitate—that you take time calmly to form an opinion; but this is not what society admires in the medical attendant: there is a manner which they call decision, and which is mighty captivating. Be above all pretension of manner, but do yourselves justice. I would, then, advise you to keep the parts quiet, and, before you throw down the coverlet, to endeavour to discover how the accident happened; because, being anatomists, and knowing how a certain force operates, you will be able at once to form something more than a mere guess or conjecture of what has actually occurred. For example, if a per-

son (and more especially an old woman) should have been coming down stairs, and thought she was at a landing-place when she was not, and so has come down the last step suddenly—and if from that moment she falls lame, and has great pain—you have no difficulty in ascertaining, before you look at the limb at all, that the force has been on the neck of the thigh-bone—that there has been a force in that direction which is apt to snap the neck of the bone. The circumstance of the patient being a female increases the probability, and also her age, inasmuch as the bones become extremely brittle by age and disuse.

When you undertake to examine the limb, the moment you have thrown off the clothes you look to the pelvis, and you see that that lies right; then you see that the limb is actually short, that the toe is turned out, and you state your decisive opinion that the bone is broken—not dislocated. You then put your hand to it, and you find that it moves easily in all directions—that there is no stiffness, no sudden spasmodic difficulty in moving it; you draw it down, and it is drawn back again; there is pain, but no let or hindrance besides to your turning it. There is no crepitus; and what is the reason of this? Why, in common fracture, do you at once find the jar of a broken bone, and not in this case? Because of the retraction of the limb. The nearer the fracture is to the superior end of the bone, the greater is the retraction. What have you to do, to ascertain the crepitus? You must make a person draw down the limb, so as to bring the broken portions into contact; and now, upon moving them, you find the crepitus, and the case is decided. But sometimes it happens, when the neck of the bone is broken within the capsule, that there is very little retraction in the limb, and therefore there is hesitation and difficulty in forming a decided opinion; and you say, if the bone were broken it would be retracted. Suppose, however, that the narrative of the accident and the complaints of the patient are such as to induce you to believe that the injury has fallen on the cervix, and yet there is no shortening of the limb, or other appearance of fracture, I recommend you to get the patient out of bed, to get her to stand upon her two legs, and then to raise the sound leg, and try to stand upon the one which is injured. She cannot do it; there is immediately a sinking down, and if you hold your hand upon the head of the femur you feel such a jar that no hesitation is left in your mind on the subject. You do this to avoid a very awkward occurrence. It happens sometimes, that the broken ends correspond so that they resist the retraction until some change of position takes place; then, by and by, on the second or third day, you find the limb greatly shortened, and the signs of fracture of the neck complete; and the wonder is that you could not have discovered this at first. I must not, then, say that there is no difficulty in these cases; for, on the contrary, I declare, that even to the most experienced practitioner, this is always a source of great anxiety. There is nothing more difficult to distinguish than the exact nature of this case; and with all your knowledge of anatomy, and the just application of the principles of surgery, it is a miserable business—that is to say, you will be blamed unless you have taken proper precaution to explain the peculiar nature of the case early. Are you aware that this is a case which stands, as it were, by itself? It is one of the exceptions to the general rule of fractures. When a bone is broken in ordinary cases, parts are ruptured, vessels throw out their blood, inflammation arises, consolidation succeeds, the parts are united by tough and inflamed cellular membrane; then comes a deposition, which you may call *callus* if you please, and an union almost of cartilage; and lastly, bone is formed in the cartilage. Can this process of union take place when the fracture is in the capsule of the joint? It cannot; the circumstances necessary to a proper union are wanting; on which subject we might take up an hour, perhaps, with some advantage.

When you are called, then, to a case of this kind, it is necessary that you should have some sensible person with you, connected with the family, or intermediate between you; and let them see that you understand the case—that you anticipate the difficulty — that the patient cannot be cured, and that there must be a shortening of the limb. After all, these are the cases that are so apt to injure the practitioner in the country. The patient has a fracture high up in the thigh-bone — there is a shortening of the limb, or the neck of the bone is absorbed; he is lame, he is thrown out of his occupation, ever idle, ever wandering about, every hobbling from one side to the other. On the market-day, if you see him in a large crowd, you can distinguish him from other persons by the motion of his head; and he is always lamenting that his case was not understood, or that he was so badly treated by the doctor.

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EXTRACTS FROM FOREIGN JOURNALS.

PLEURITIC EFFUSION IN BOTH SIDES OF THE CHEST SUCCESSIVELY — DOUBLE PARACENTESIS THORACIS—RECOVERY.

A YOUNG Arab had suffered for some time from pain in the cavity of the chest, with impediment in breathing, and sense of suffocation. I examined the chest. The left side was much enlarged, the ribs separated, and the muscles distended in the intercostal spaces. I perceived that there was fluid within, which caused this projection, I therefore made a puncture; when there issued several pints of serous pus, with albuminous floceuli. A small mesh was then introduced between the lips of the wound, to keep it pervious. The opening was thus maintained for several days, and the cure appeared complete, without any thing else having been done. In a month after, the right side (which had appeared to be sound when the operation was performed) had acquired a size nearly equal to that of the left side before it was punctured. Paracentesis of the right side was now performed, when a quantity of purulent matter, of the same nature as before, escaped. The respiration became free in a few days; the sense of suffocation disappeared, and he got quite well.—*Clot-Bey; in the Gazette des Hôpitaux.*

ABSCESS, CAUSED BY A CANINE TOOTH LODGED IN THE MAXILLARY SINUS, CURED BY ITS REMOVAL.

A young Mussulman of Alexandria had a considerable swelling of the right cheek, the nose being pushed to the other side. Within the lip was a fistulous opening, as large as a quill, and occupying the canine fossa. A sound introduced into the sinus through this opening enabled me to ascertain that there was no fungus. I then detached a portion of the enlarged sinus from below, by means of a gouge and mallet. The finger was thus admitted into the cavity, where I found a tooth, which had escaped from its alveolar socket, and become lodged in the sinus, where it acted as a foreign body. I immediately extracted it. In twenty days the parts were healed, and the deformity gone.—*Ibid.*

A MAN WHO CAN MAKE HIMSELF TALLER AT WILL.

At a late meeting of the Academy of Medicine in Paris, M. Velpeau exhibited a man who possesses the very singular power of making himself two inches taller or shorter at will. Standing erect, he can elongate the spine, and contract it again, by moving the sacrum, which plays like a wedge between the bones of the pelvis. He is at present 40 years of age, and had a carriage pass over his body when a child, to the injury received at which time the power of executing this curious manœuvre may be attributed.—*Gazette des Hôpitaux.*

LITERARY INTELLIGENCE.

In the press, An Inquiry into the Nature of Sleep and Death, with a view to ascertain the more immediate Causes of Death, and the better regulation of the means of obviating them; being the concluding part of the Experimental Inquiry into the Laws of the Vital Functions. By A. P. W. Philip, M.D., F.R.S.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *May* 20, 1834.

Abscess	1	Heart, diseased	1
Age and Debility	36	Hernia	2
Apoplexy	4	Hooping-Cough	11
Asthma	18	Inflammation	33
Cancer	1	Bowels & Stomach	4
Childbirth	4	Brain	2
Consumption	51	Lungs and Pleura	4
Constipation of the Bowels	1	Insanity	2
Convulsions	28	Liver, diseased	3
Dentition or Teething	7	Measles	8
Dropsy	15	Mortification	2
Dropsy on the Brain	20	Paralysis	2
Fever	8	Small-Pox	5
Fever, Intermittent, or Ague	1	Spasms	1
Fever, Scarlet	12	Thrush	2
Fever, Typhus	1	Tumor	1
Gout	2	Unknown Causes	12
		Stillborn	16

Decrease of Burials, as compared with the preceding week . . . 125

NOTICES.

Dr. D. Williams's circular to the medical journals, relative to Dr. Kay's work on Asphyxia, is not suited to our pages. Dr. Kay's book has already been reviewed in this journal.

We have received from several quarters accounts of examinations held in different medical schools, both in London and the provinces: some of them, we observe, have been already published in the newspapers; but even had they not, we generally have no room to spare for the insertion of such intelligence.

RECEIVED—An Introductory Lecture on Diseases of the Eye, delivered at the Birmingham Eye Infirmary. By Richard Middlemore, M.R.C.S., &c.

W. WILSON, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MAY 31, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XXXIV.

DIFFICULT LABOUR.—INSTRUMENTS.

VECTIS.

The next instrument to which I shall beg your attention, gentlemen, consists of a single blade, and is called THE VECTIS, —THE LEVER; but to this name I have decided objections, because if employed as a lever, the soft parts of the patient are in great danger of injury; and because the term *vectis*, as used by the ancients, carries with it an idea of force and violence;—that having been an engine used to raise great weights, to wrench open doors, and perform other acts requiring considerable strength. Nor am I singular in this objection; for my friend Dr. Blundell conceives the same idea, and has denominated the instrument THE TRACTOR.

History.—Much uncertainty still hangs over the primary origin of the vectis; and we are without any positive records either regarding the real inventor, or the time at which it was first used. It is well known, however, that Hugh Chamberlin, about the year 1693, twenty-three years after his unfortunate visit to Paris, made also an excursion to Holland, where for some time he practised midwifery; and, having contracted an intimacy with Roonhuysen, he communicated his secret to him, on the receipt of a considerable sum of money. Very soon Ruysch and Boekelman, two other practitioners at Amsterdam, became Roonhuysen's partners in the secret. These surgeons scrupulously adhered to the terms of their agreement with Chamberlin; and only displayed the instrument, and described its use, to those of their pupils,—and that a very limited number,—who gave them a large additional fee for this extra knowledge. Among this number was John de Bruin and Peter Plaatman; and they also, under the same conditions of secresy, instructed Titsing, Boom, and a few others. In a short time the credit of the instrument was raised so high, that the magistrates of the town forbade any of their surgeons to practise midwifery, until they had been examined by its possessors, and instructed in the manner of using it. As it passed into a greater number of hands, its fame became proportionally spread; and in the year 1753, Visscher and Van de Poll,—physicians practising at Amsterdam, — with a praiseworthy generosity, by no means characteristic of the age, bought the secret from Gertrude, the daughter of John de Bruin, and wife of Herman Van der Heide (to whom it had been bequeathed by her father as a legacy), and made it immediately public. The instrument, thus dragged forth from comparative obscurity, about which so many conjectures had been formed, on which the hopes of the Dutch practitioners were so highly raised, and which the profession in general was so anxious to obtain, was found to be a flat, plain, unfenestrated piece of iron, slightly bent at both extremeties into the segment of a large circle. On this discovery many doubted,—as well they might,—whether de Bruin's heir had really allowed the true instrument to pass out of her possession; others, whether Roonhuysen had dealt fairly with his pupils; and others, again, whether Chamberlin had not acted a double part, by having originally sold to the Dutch physicians an instrument, of which he himself practically made no use; because, as it was then well known that Chamberlin's celebrated *arcanum* consisted

of a double blade, it seemed impossible to reconcile this fact with the production of the simple, rude, almost straight piece of iron, which any one might devise and manufacture without being possessed either of much inventive genius or turn for practical mechanics.

Since the fortunate discovery, however, of Chamberlin's original instruments, as I have before informed you, these inconsistencies appear to me easily cleared up; for among them are two simple levers, very similar in size, shape, and design, a drawing of one of which I now present.

Chamberlin's lever—the front and side views. The only difference between this and the other specimen found consists in the one being blunt at the extremity of the curved handle, while this terminates in a sharp point.

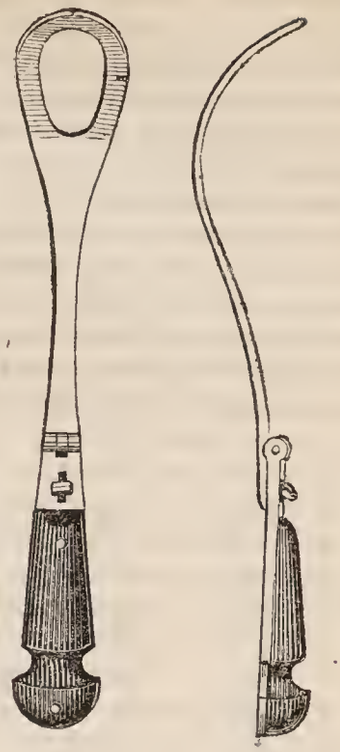
It seems, then, fair to infer that both the forceps and lever—such as they were—were communicated to the Dutch practitioners by Chamberlin; and that, either in consequence of not being able easily to introduce the two blades, or not readily adapting them to each other when they were introduced; or perhaps,—what is equally likely, since it agrees so well with the spirit of the times,—finding themselves enabled to use the one blade with so much greater *secresy*, and less chance of *discovery*, than two: they became habituated to the employment of the vectis, acquired a certain degree of dexterity in its use, considered it the most efficient instrument, and consequently instructed those who applied for such knowledge in its properties, in preference to those of the forceps: and this supposition is strengthened by the fact that Palfyn, of Ghent, in 1720, after many journeys to London and Amsterdam, for the purpose of ascertaining what Chamberlin's celebrated secret really consisted in, (which was known to have passed into the hands of the Dutch physicians,) was enabled, from the information he picked up, to form his *tire tête*, which he presented to the Academy of Sciences at Paris, and which, after some modifications, became the forceps of Dusée and Butter. It was certainly the prevalent belief, in Roonhuysen's time, that the instrument he employed was the forceps; and Levret states that Vander Suam, who lived many years with Roonhuysen, confirmed that idea; so that we may suppose he possessed the forceps, but more frequently used the lever.

If this history be correct, it is plain that we should be in error if we granted to Roonhuysen the honour of having discovered the vectis; for although he made a great change in the general appearance of the instrument, his alteration scarcely affected it at all as a mechanical power; since the curve of his lever, or rather De Bruin's—and I take it for granted they were the same—as delineated by Mulder, is almost a segment of the same circle as that possessed by the instrument found among Chamberlin's collection. All the first vectes, indeed, such as Roonhuysen's, Plaatman's, Boom's, and Morand's, as well as Titsing's *spatula*, have nearly the same curve of greater or less extent; and this also inclines me to believe that Chamberlin's was the original pattern. But after all it is quite possible that Chamberlin took his idea of the vectis found among his papers, from Roonhuysen, during his residence at Amsterdam.

I have said that Roonhuysen's lever consisted of a flat piece of iron, bent into a slight curve at both ends, and he generally employed it covered with soft leather. Titsing fancied he had improved on this plan, by padding the instrument with wool. It has been formed by different persons,—either for the sake of appearance, or from the presumption that such substances were less likely to inflict injury than the harder metal—of wood, horn, ivory, and silver. The vectis of Morand, in 1755, was of ivory; and that of Herbiniaux, in 1782, of the latter material.

As it was generally known among the profession, at the commencement of the last century, that some of the Dutch physicians possessed Roonhuysen's secret, it is not surprising that practitioners, as well in Holland as in other countries, should have endeavoured to ascertain in what it consisted, or to have invented for themselves some means to accomplish the same end. Thus, in 1738, Rigandeaux, being called to a case in which the head was

impacted, procured a common chemist's spatula, a foot in length, and, after having softened the blade in the fire, he bent it into a slight curve, and with it delivered the woman of a live child. Incited by his success, he formed an instrument very similar to Titsing's in shape, though shorter, which he was in the habit of using continually. Like his predeeessors, however, being desirous of reaping more than ordinary pecuniary advantages from his fortunate discovery, it was not until the year 1754—after Visscher and Van de Poll had published descriptions and delineations of De Bruin's instrument—that he made known the form and use of his own. Warrocquier, of Lisle, also, it would seem, fell by chance upon the expedient of delivering by the lever, before that instrument was publicly known; for in 1753, being foiled in his attempts to terminate a labour by Smellie's forceps, at that time but just come into use, he employed one blade as a vectis, and had the satisfaction of bringing into the world a living infant. From that time he discarded the double instrument, and used a single blade, not unlike Titsing's in its fashion; with which, indeed, he afterwards boasted that in twenty-one years he had delivered 1200 women. Long before this time, however—viz. in 1715—Isaac de Bruas, of Midelburg, made an attempt to extract a child when the head presented, with a blunt hook, such as he had been accustomed to use under breech presentations. He succeeded in his object, having slightly bruised the child's head. Correctly judging that the instrument was too thick, and not sufficiently wide for this purpose, he formed a fenestrated vectis, decidedly the best of all those first invented, and which, indeed, approaches nearer than any of them to the form of Lowder's—that variety now in most common use in this country; which I recommend in preference to others, and a specimen of which I now shew you. This instrument is 12 inches long, and seven are engrossed by the blade; which does not spring from the handle in a regular sweep, as we observed with regard to the forceps, but possesses a shank nearly straight for the space of three inches, retiring backwards at a very small angle; the curve then commences, and its point is bent forwards more abruptly; the blade is widest near the point, being one inch seven eighths across; the fenestra is nearly oval, two inches and a quarter long, and an inch and one-eighth wide in the centre. The whole instrument weighs seven ounces. This specimen, you will observe, possesses Saxtorffe's hinge, but this is of no advantage in its use; it is merely for the convenience of carriage.



Front and side views of Lowder's vectis.

Cases in which the vectis is applicable, and mode of using.—All the cases in which the short forceps are applicable, are supposed to be fitted for the use of the vectis; and the symptoms calling for its employment are necessarily also the same. Three modes of using this single blade have been suggested;—either as a lever of the first kind, or as an antagonist to the left hand introduced into the pelvis, or as a simple tractor; which last, indeed, is the only safe method. As I have already said, it was intended to be used as a lever by Chamberlin and Roonhuysen; and that the pubes of the mother should form the fulcrum. I need not insist on the injury likely to result from this mode of acting; I need not call your attention to the important organ, the bladder, placed directly behind the junction of those bones, and the danger which must accrue to it, as well as the surrounding soft structures, from the violence of undue pressure; and I need not warn you against ever adopting Roonhuysen's plan. The injuries inflicted, indeed, must have been frequent and great; and this led Pean, in 1772, to suggest the possibility of delivering by the vectis, without making a fulcrum of the mother's structures. He proposed a practice, which is now sometimes adopted, of grasping the shank of the instrument with the left hand,—the outer edge of the little finger being applied towards the vulva,—making that hand the fulcrum, and pressing the extremity of the blade on the child's head, by raising the handle firmly held in the right. Though not so easy a method of delivery, this is much safer than that first recom-

mended, and, *if used as a lever at all*, the instrument should be employed in this manner.

Another mode praetised was the introduction of the instrument over one side of the head, and the application of three or four fingers of the left hand over the opposite, which were intended to act as an antagonist to the iron blade, and to form a double instrument, similar to the foreeps; for as the latter instrument is to be regarded as a pair of artificial hands, so the veetis, if used in this way, must be looked upon as a substitute for the right hand. But it is evident that, if there be sufficient room in the pelvis to allow of the introduction of three or four fingers over the head, there ean occur very few eases in whieh instrumental assistance is neeessary. This mode of using it was first adopted by De Bruas, in 1755.

In 1783, Dease, of Dublin, gave a new name to the instrument, which much influenced the mode of using it. He called it an *extractor*, and proposed, that on the point being earried fully over the ehild's head, the handle should be grasped tightly, and held firmly, by one hand, while the shank was embraeed by the other, and the movement, that of steady traction downwards, should be given by that hand which embraeed the shank, thus eonverting the instrument into a lever of the third species. He sta'es his opinion, that if used as a lever of the first kind, it must always prove highly dangerous, "retentions of urine being the immediate, and involuntary discharge of urine the lasting eonsequenees of it." In the year prior to that last mentioned, indeed, Herbiniaux, of Brussels, had formed a vectis, which aeted on the same principle; he passed a piece of tape through a transverse eye in the blade, situated just below the curve, by which tape traction was to be made, while the handle was held tight, and steadied. It is curious, however, that this improvement on the instrument of his day was taken from the original vectis which Visscher and Van de Poll bought from De Bruin's daughter; for in the description of that instrument whieh they published, we read that they found a cord fixed round one end, about the middle of the eurve. "This," they state, "we imagine, served no other purpose than to point out the end of the instrument eommonly made use of" (for they were both similar), "or to measure the length of the part introduced." Levret afterwards suggested, that the intention of the cord, for which these physicians eould find no use, was probably to moderate the pressure of the instrument against the pubis. Herbiniaux took the hint; but supposing that, from the situation of the ligature, the mother's parts would be liable to be chafed, he placed it a little distance below the curve.

The mode reeommended by Dease is that now generally adopted, and which I advise you to follow, provided you are inelined to eall in the aid of the veetis. It is scarcely neeessary for me to urge again on your attention, the impropriety of employing any instrument elandestinely, however easily it may be applied, or however adroit we may be in its use; for although Lowder ineulcated the doctrine of coneealment and secresy, and although since his time many men, infeeted with his notions, have frequently had reeourse to the instrument unknown either to the patient or her friends, be assured that such a practice will, in the end, most probably lead to disgrace and bitter self-reproach. In this respect, then, we should act exactly as with the forceps—inform the patient and her friends of the neeessity of assistanee being rendered, and never interfere unless cireumstances demand our aid. Before the introduction of the instrument, the bladder must be emptied by the catheter, and the rectum unloaded also, if requisite. The same posture must be adopted previously recommended: the patient's person must be brought conveniently near to the edge of the bed, and the instrument warmed and greased. Two fingers of the left hand are then to be passed as high as possible within the vagina, over the child's head; the handle of the instrument must be held lightly in the right, and depressed sufficiently low to allow the point to slide up between the fingers and the ehild's head. The same kind of semi-rotatory motion recommended in the application of the forceps, will also faeilitate the introduction of the veetis; and, as with the forceps, if any impediment occur to its easy passage, that must not be overcome by main force, but a different direction must be given to the blade, and the obstacle must be surmounted by gentle insinuation. On the complete introduction of the instrument, the fingers of the left hand are to be withdrawn, and the shank so grasped with that hand, that the little finger shall lie near the os externum, and the first finger surround the junction of the handle with the blade. Thus a firm purehase is obtained; and the whole instrument must be steadied with the right hand, while traction is made with the left. This extractive effort must not be a constant, strong, uninterrupted *pull*, but must consist of a number of short, steady, firm attempts, following eaeh other in tolerably quick suceession; the left hand pressing strongly against the shank under each, so that the point may at the same time compress the head, while the handle remains stationary. Here also, as with the forceps, we work

during the continuance of contraction, and desist in its interval; and should the uterus be inert, we imitate nature, making our traction only occasionally. This instrument is much more likely to lose its hold than the forceps; but as it is easily re-introduced, and as there is no second blade to adjust, that occurrence is of little consequence.

In employing the vectis, then, we shall find it necessary to apply it over different parts of the cranium, and perhaps the face also, successively, in order to relieve the head from its fixed situation, and favour its descent; and we may sometimes feel it necessary to use it one minute as a tractor, and the next as a lever; being, however, most cautious to make the fulcrum of *our own left hand*, as first recommended by Pean.

In the year 1784, Dr. Aitken, of Edinburgh,—a man of great ingenuity and mechanical invention,—having found by experience the difficulty of introducing a blade with the extremity considerably curved, and the uselessness of the instrument if the point was but slightly bent, contrived one that could be introduced straight, and curved *afterwards* to any required angle, by turning a screw at the extremity of the handle, so that it might adapt itself closely to the child's head. This supposed improvement he denominated the *living lever*.

You will observe in the machine before you, with what difficulty the phantom descends under the action of the vectis, although its head is small in comparison to the size of the stuffed pelvis, and you may, from watching my almost unavailing efforts, imagine that it is a power inferior in its value to the forceps.

Relative value of the vectis and forceps.—Much, indeed, has been written, and more said, of the relative value of the vectis and forceps as obstetrical assistants; and the discussion was at one time carried on with no small degree of acrimony on both sides. Some practitioners invariably used the vectis—as Lowder, Dennison, and Sims; others gave the preference to the forceps, among which number were Smellie, Denman, and Osborn: of the present teachers, I believe most are in the habit of employing the latter. The first celebrated, and, perhaps, the most strenuous, of all the advocates in this country, for the vectis as the preferable instrument, was Dr. Bland. His arguments will be found in a paper by him, published in the second volume of the London Medical Communications, in the year 1790. We must, however, receive the recommendations even of practical men on this subject *cum grano salis;* recollecting that early instruction is likely to prejudice every one in favour of any particular instrument, and that a certain degree of acquired dexterity in its use would probably attach him to it, and cause him to recommend it. Thus, then, although one practitioner may wield the forceps, and another the vectis, with the greatest advantage, it by no means follows either that we should be able to administer artificial aid with the same success, or that we should find the superiority of one instrument so decidedly outweigh that of the other, as we might be led to imagine by listening to the doctrines of those practised exclusively in the employment of either. If, however, we can show, by legitimate arguments, that the one instrument possesses a decided advantage over the other, we are bound to use that means which offers the fairest prospect of success; until, indeed, by actual experience, we become convinced of the fallacy of our previous impressions.

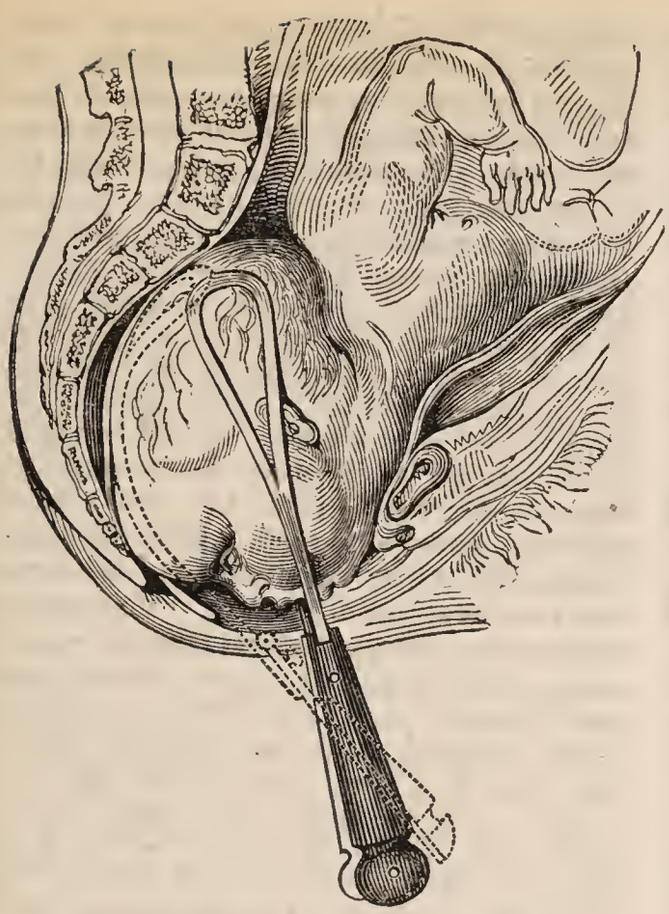
Arguments in favour of the vectis.—The arguments in favour of the vectis are,—*first*, that there being but one blade, it is more easily applied; and that, as the greatest difficulty in introducing the forceps consists in adjusting the second blade, that inconvenience is of course obviated. *Secondly*, that being so easily applied, it is not necessary for the operator to ascertain so intimately the nice obstetrical points connected with the case, or to make himself so minutely acquainted with the position of the head, as where the forceps are used. *Thirdly*, it is said that extraction can be more easily effected with it. *Fourthly*, that it can be used in cases where the short forceps are perfectly inadmissible—namely, before the head has descended sufficiently low for us to feel an ear; because we do not guide this instrument over the ear, but introduce it where we can most easily apply it, and where we can obtain the most useful purchase.

Each of these arguments deserves a distinct consideration. In the *first* place, then, I readily grant that the single instrument can be more easily applied than the two-bladed forceps; but I deny most positively that delivery can be more easily effected with it, at least it is not so in my hands. You have seen the difficulty I just now experienced in endeavouring to extract the fœtus through this stuffed pelvis, which is large in proportion to the head. I may be a bungling operator,—I may, perhaps, neither understand the principles nor the practice of instrumental midwifery;—if so, it is not from neglecting to study, nor for want of opportunities to make myself practically acquainted with the science: and, if the difficulty in the living subject is at all proportionate to what you have this moment observed, I would ask of what use is it to boast the easy adaptation of a power

which, when properly adjusted, is so inadequate to the end proposed. *Secondly*, we are told that, being so much more easily applied than the forceps, it is not necessary that the operator should be so perfectly conversant with obstetrical principles in general, or the particular points of the case under treatment. This, although a very specious, is, in my opinion, the most unfair and dangerous argument which the advocates in favour of this particular instrument adduce :—to prefer the vectis because it may be worked by a person who knows but little of obstetric principles, is, to say the least of it, placing a dangerous instrument in rash hands, framing an excuse for ignorance, and opening a wide door for violence and injury. I cannot but think that man highly culpable who would attempt to introduce the vectis without knowing minutely the bearings of the case under his care, or who was not sufficiently acquainted with the principles of obstetric science to enable him properly to adapt the forceps. Such a man would compromise his patient's safety, to say nothing of his own character. The *third* argument I have already answered by the trials of which you have been witnesses. The *fourth*, and last, is the only argument which with me carries any weight in support of the vectis—viz. that it can be used in cases where the short forceps is inadmissible, owing to the principal bulk of the head remaining above the pelvic brim: it is a longer instrument, and in its application passes higher within the woman's person than the short forceps, being received somewhat, indeed, into the cavity of the uterus itself; but to overcome the difficulty of such a case, we are in possession of a much more efficient, and, in my opinion, even more safe instrument, in the long forceps: so that either with the long or the short forceps we may surmount most of the impediments to which the vectis is applicable.

Positive advantage of the forceps.—Besides these negative advantages, the forceps appears to me positively superior to the vectis in many respects. *First*, when we have applied the blades fully over the ears, we can generally turn the head into that direction most convenient for its exit. I have already told you that if the face be coming forward, towards one or other groin, we may, perhaps, find it necessary to turn it into the hollow of the sacrum before we can accomplish extraction, and that this turn can be effected with no very great difficulty; but we cannot do this with the vectis—we can only extract the head in that situation under which it is attempting the passage. *Secondly*, we can compress the head with the forceps, and diminish its lateral diameter so as to enable it to escape through a somewhat contracted aperture. It may be answered, that this can be effected with the vectis also: but when the head is compressed between the two blades of the forceps, the pressure is taken off from the mother's structures; should the vectis, however, be employed, the counter-pressure is made by the bony pelvis itself, and the soft parts lying between the head and the pelvic bones must suffer more or less from contusion. *Thirdly*, we are not in so much danger of injuring the mother, because, with the forceps, we have a fixed fulcrum, and consequently there is no necessity for us to form one for ourselves. To this observation, again, it may be answered that the instrument should be used as an extractor, and not as a lever; and that therefore our argument is unfair, as being deduced from an abuse of means. In reply, I would observe, that there is such a liability to make the instrument a lever, and, in the anxiety of accomplishing our object, so strong will the inclination be found to use it in the manner most easy to the operator, that it will be almost impossible to resist it. The instrument is so much more easily used as a lever than as an extractor, and the fulcrum is so much more naturally made by the bony pelvis than our own hand, that there is a very great chance, however determined we may be to the contrary, that we should transgress the rule, and endeavour to *scoop* the head out. It will, of course, be understood that these remarks apply to young operators; I am not speaking of experienced practitioners.

These three principal advantages, then, of the forceps—the head being able to be turned in any direction, and into any situation—their producing compression and diminution of bulk, without bruising the soft parts, and the comparative safety with which they may be employed—induce me to use them, and strongly recommend them, in preference to the vectis. There are only two cases in which I think the latter instrument preferable; under presentations of the face or side of the head—the ear, for example. In the kind of case illustrated by this plate of Smellie, I consider the forceps an unfit instrument. Highly as I esteem Smellie as a scientific man—much as I value him as a practical obstetrician—I much object to the employment of the forceps under this situation of the head; because, if extraction only be used, without any attempt being made to bring the vertex into the hollow of the sacrum, the child's chest would be thrown against the symphysis pubis, and delivery could not take place; but if the vectis can be passed along the concavity of the sacrum,



The forceps applied under a face presentation; the head being fully in the pelvis and the chin towards the pubes. The vectis, applied over the head within the sacrum towards the right side, is introduced here to illustrate the lecturer's argument, and is represented by dotted lines.

and hitched over the occiput of the child, we draw the head down, make it turn upon the neck as upon a hinge, the occiput sweeps the hollow of the sacrum, and the face is turned up under the arch of the pubes. So long as the forceps are continued in this situation, such a change in the position of the head cannot be produced through their agency; for if we turn the handles up towards the pubes, for the purpose of bringing down the vertex, the instrument would make no impression on the head, but the blades would slide along the cheeks, describing a semicircular movement, and the head would remain just as before. It is only by the pendulum-like motion from handle to handle that the head can be moved in any degree by the forceps.

It is often urged in favour of the vectis, that its aid can sometimes be usefully taken advantage of in cases where we should not think it *necessary* to apply a more complicated instrument, such as the forceps,—for the purpose, therefore, of saving the patient much painful exertion, and the attendant many hours of weary watching; although, indeed, it might not be employed clandestinely. It is scarcely necessary for me again to insist on the impropriety of using mechanical assistance unless driven to it by necessity. I do not intend to assert that harmless means are never to be had recourse to until the powers of life are low upon the ebb, and exhaustion is approaching with rapid strides; but to inculcate the doctrine, that nature is not to be interfered with merely for our own convenience, or even under the more specious plea of lessening the woman's sufferings. I have good reason, however, to believe that these motives influence some practitioners of the present day; and I have no doubt that the empirical professors of the last century, especially in Holland and the Netherlands, made use of their golden secret on many occasions unnecessarily: else how can we reconcile to fact the boast of Warroequier, that during twenty-one years he had delivered 1200 women by the aid of his instrument alone.

FILLET.

Of the *fillet* or *lacque* I have but a few words to say—an instrument now very properly discarded from practice. The first mention of such a contrivance occurs in the writings of Rhazes, of the Persian Irak, about the end of the ninth century, and became known under the name *laqueus*. It consists of a strip of strong cloth, silk, or leather, formed into a noose, and was sometimes sewn up like an eel-skin, open at both ends, to admit the introduction of a piece of whalebone, cane, or wire, throughout its entire length, by which its application might be facilitated. It was intended to be introduced over the head in whatever way was most easily accomplished; and when the purchase was obtained, the cane was to be withdawn; the two ends tied together, and extraction was to be effected by main force. We cannot suppose that the fillet could retain its hold unless it was actually passed round the neck; and if fixed there, it is very evident that the traction would have a tendency to double the neck upon itself, to turn the head to one side, and to form a most difficult and complicated case out of one which might, perhaps, have been terminated by the efforts of nature alone; because the power obtained could only be employed in one direction, and that in a straight line downwards. I have never personally known this invention applied to facilitate a lingering labour; and I believe the last time it was used in London is recorded by my friend, Dr. Merriman, in a case where his uncle was an unwilling spectator of the highest degree of mischief inflicted by this instrument, in the hands of a French physician. The force employed on that occasion was so great, that the head was severed from the body, and the poor woman, as might be expected, died on the second day after delivery.

Smellie gives a plate of what he considers the best specimen of the fillet, communicated to him by his friend Dr. Mead; and states, that it may be employed, provided the forceps are not at hand. In this recommendation Smellie must have merely followed the fashion of the day; for his consummate knowledge of the mechanism of parturition taught him that such a contrivance could not be introduced if the head was impacted in the pelvis; and he must have been aware, that if adjusted in the most fortunate manner, it could not favour the necessary turns, but was merely calculated to act on the application of brute force.

REMARKS

ON

RELAXATION AND DESCENT OF THE UTERUS AND BLADDER

In the Puerperal State.

By John Roberton, Esq.
Surgeon to the Manchester Lying-in Hospital.

A degree of descent in certain of the pelvic viscera, from their natural situation, constituting disease, takes place more readily in the first few months, or, I may rather say, in the first few weeks, succeeding parturition than under other circumstances. In this most populous and industrious community (Manchester), lying-in women of the poorer classes are soon up, and doing for their families; many on the third day after labour, and the majority within the week. In consequence of this practice, numbers are obliged to apply at the hospital on account of what they denominate "a bearing down," a term used to express in general those sensations which arise from the uterus, bladder, or the vagina, becoming relaxed, and falling lower than is natural. In the upper classes similar effects are apt to follow child-bearing in such as are too wilful to remain sufficiently long in a recumbent posture; and occasionally in those also, even where there has been no imprudence, whose labours are unusually severe and prolonged.

Writers are not well agreed regarding the predisposing causes of this accident in the puerperal state; some attributing it to the unusual weight of the *uterus* at this period, and the relaxed state of those folds and reflections of the peritoneum, which bind the *uterus* and bladder to the pelvis, the rectum, the abdominal parietes, and to each other; while other writers join with these causes relaxation of the vagina, and of the pelvic fascia especially.

Before inquiring what are the *predisposing*, it may be well to notice the *immediate* causes. The following is a summary of the statements of twenty-five women, suffering under different degrees of descent of one or more of the viscera referred to, as to what they considered to be the immediate causes of the complaint, (or the circumstances in which it appeared for the first time), in their own persons. The statements are chiefly taken from a note-book, and include those of both private and hospital patients. Although some of the accounts are sufficiently explicit, others are less so than I could wish.

No. 1. A. D. *First* confinement: aware of the prolapse, after going to her work in a factory, at the end of the second week.

2. G. A. *Seventh* ditto, after rising from her bed on the third day.

3. G. F. *First* ditto, at the end of the third week, after a very severe labour.

4. M. D. *First* ditto, soon after labour.

5. A. C. *Fourth* ditto, after leaving her bed on the fourth day.

6. S. D. *Seventh* ditto, on leaving her bed.

7. M. D. *First* ditto, was doing for her family on the third day.

8. A. R. *First* ditto, after running hurriedly at the end of tho month.

9. M. D. *Second* ditto, after working vigorously at a pump.

10. G. S. *First* ditto, soon after labour.

11. Mrs. C. *First* ditto, appeared when she began to walk about, at the end of a fortnight: labour tedious and severe; child, a male, and very large.

12. B. S. *First* ditto, when weaning, after having suckled her infant sixteen months.

13. M. F. *Fourth* ditto, in the fifth week after labour.

14. E. M. *Second* ditto, at the end of the fifth week; had exerted herself from the end of the fortnight.

15. F. M. *First* ditto, was doing for the household on the fourth day.

16. A. B. *Second* ditto, soon after labour.

17. E. G. *First* ditto, first felt the complaint after attempting to lift a heavy weight during pregnancy.

18. F. H. *First* ditto, appeared five weeks after labour, in consequence of taking a long walk.

19. A. M. *Second* ditto; appeared five months after labour; suckled till the thirteenth month, when prolapse was greatly aggravated.

20. C. L. *First* ditto; soon after labour: contrary to the injunctions of her surgeon, was sitting up on the third day.

21. B. W. *First* ditto; on the 12th day, after tedious labour requiring the forceps.

22. F. R. *Second* ditto; soon after labour: was going about within the week.

23. T. W. *First* ditto; on the fifth day after labour.

24. L. G. *First* ditto; at the end of the first week, after a tedious labour requiring the forceps.

25. A. B. *Second* ditto (a miscarriage.) Three months after this event became aware of a prolapse.

It is well worthy of remark, that of these twenty-five cases, in thirteen the complaint was thought to originate after a first labour: in another, it was attributed by the patient to lifting a heavy weight some time in the first pregnancy; and in a second, to long-continued suckling of her first child. The great distention the vagina endures in giving passage to the head of the fœtus, for the first time, is productive of more temporary injury to that canal and the neighbouring textures than happens in a subsequent delivery; hence a condition of parts favouring the descent of the uterus and bladder, particularly should the patient be so imprudent as to leave her bed, and stand or walk about too early.

The natural contrivances by which the *uterus* and bladder are held *in situ*, in health, are many and complex. Upon these *viscera* and the rectum the peritoneum is spread, and firmly adheres to them. Passing from one viscus to another, it is thence reflected on all sides, and attaches itself to the pelvis, with which it is closely connected as well as to the abdominal parietes. Thus, in the unimpregnated and ordinary conditions of the parts, the peritoneum suspends the uterus and bladder, retaining them in their situations relative to each other and to the pelvis. The inclination of the pelvis, too, in the erect position of the body, assists in some degree in affording support. But probably the most important stay of these viscera during the puerperal period, when greater support is wanted, consists in the contractile texture of the vagina itself; and, more particularly, in the peculiarly firm attachment the vagina forms, at every part of its circumference, where it penetrates the strong, dense curtain, which closes the outlet of the pelvis. A powerful muscle,—the *levator ani*, and strong fasciæ, compose the curtain; and it is through a slit in this texture that the *os externum vaginæ*, itself surrounded by a broad sphincter muscle and dense plexus of vessels, opens under the arch of the pubis. In dissecting the parts within the pelvis we find it easy to separate from its surrounding attachments all that portion of the *vagina* which is strictly intrapelvic; but on arriving where the vagina passes under the arch of the pubis, in other words, where it unites with the urethra above and with the rectum below, the strength of the connexion is found to be very remarkable.

With the view of illustrating this point, I performed, in company with my friend Mr. Boutflower, the following experiment. We had for our subject the recent body of a stout and vigorous woman who died after an illness of a couple of days. First, I cut the broad ligaments, continuing the incision in such a manner as to separate the uterus and bladder from their surrounding attachments to the pelvis. I then attempted to make the *os uteri* protrude at the *os externum vaginæ*, by pressing forcibly on the fundus of the uterus, in a direction corresponding to the axis of the outlet: but I failed in my attempt; I could not push it so low. On examining into the cause of this resistance, we found it to be situated where the vagina passes out of the pelvis, as I have before described.

The vagina, the curtain of the outlet, and the neighbouring textures, undergo considerable changes in the puerperal state: particularly, as has been already remarked, in the first confinement. The peritoneum covering the *uterus* itself, as also those reflections of it to the surrounding parts, after the great extension

produced by pregnancy, is in the progress of shrinking; and being less dense and firm than at other times, is consequently little calculated to sustain the weight of the *viscera*. In dissecting the bodies of those who have died within a few days after delivery, I have noticed that the peritoneum on the *uterus*, where it is about to be reflected to the bladder, is loose and somewhat corrugated. The vagina, on which the bladder, thus ill sustained by the peritoneum, rests its *fundus*, is relaxed and flabby; and the same may be affirmed of the muscles and fascia which surround, and, under other circumstances, brace the vagina. Besides this, which is partly the relaxation naturally incident to parturition, and partly, also, in many cases, the result of long-continued distention of the passage by the fœtal head, there is occasionally inflammation of the mucous membrane of the vagina, and leucorrhœal discharge, giving additional feebleness to that canal. These circumstances, when viewed in connexion, (the somewhat greater weight of the uterus also being taken into account) prove how easily the bladder and uterus may suffer descent, should the patient leave the recumbent posture too early: and they further lead us to infer, that the moderately-distended bladder, from want of due support, may, simply by its weight, drag down the *uterus* with it: and so become the chief cause of descent of that organ; an inference, the truth of which is frequently confirmed by experience.

The three following cases afford examples of slight *procidentia*, succeeding parturition, in first births, where proper remedies could scarcely at all be applied, owing to the poverty of the patients. The notes I possess of the cases, too, are but scanty. That, however, is of the less consequence, as I am not writing a regular treatise on prolapse of the uterus and bladder, but purpose merely to point out the great tendency there is in the puerperal state, and particularly after a first child, to descent of the pelvic viscera; and also to specify the simplest means of cure.

1. Ellen Fogg, aged 20, a month delivered of her first child, which, in consequence of severe and tedious labour, was still-born. Ever since her confinement she has been feeble, although naturally healthy. Five days ago, first became aware that she had a bearing down, which she further described as a sense of weight, conjoined with a feeling as if something was about to protrude from the vagina. On causing her to kneel against a sofa, I find the *os uteri* is little lower than natural, but the vagina is flabby, and in a degree prolapsed. Just within the *os externum*, under the arch of the pubis, the finger encounters a somewhat broad smooth tumor, easily pressed up. This fulness seems to be owing to the fundus of the bladder falling lower than natural, and carrying before it the upper wall of the relaxed vagina. The result of the case is not noted.

2. Martha Sullivan, aged 25, six months ago was confined of her first child. Admits that she was up and dressed on the third day; and that afterwards she daily rose and moved about as if she had been in health. Says that in about a fortnight after labour she was aware of something being amiss, having the feeling as if "her body came down."

On examining *per vaginam*, I discovered slight descent of the uterus, (the examination, I may remark, was made in the morning, and prolapsus in all its degrees is worse in the latter part of the day), also great flabbiness of the vagina, and descent of the bladder; that is to say, a soft tumor occupies the upper part of the vagina, just within the *os externum*, and is easily pressed up. Says that the sense of fulness in the part is greater when the urine is long retained. The above entry is dated the 1st of February. On the 25th of April, after having in the interval daily used the nitrate of silver lotion, (a scruple of the salt to eight ounces of water), and attended to the general health, this patient declares herself better.

3. Ann Mercer, aged 21, healthy-looking, a month and three days delivered of her first child. Applies on account of a bearing down. Suffered a great deal during her pregnancy from itching and inflammation of the vulva, for which she had medicine from the hospital. *Examination per vaginam.* Find the *os uteri* lower, in a slight degree, than it ought. But the cause of the sensation of bearing down is owing to descent of the bladder, for I find a soft smooth tumor or fulness situated at the upper part of, and just within the *os*

externum, under the arch of the pubis. Says she is obliged to work, and cannot take rest. Ordered a Duffin's pessary, with the view that it may be taken out nightly, and the parts washed night and morning; and the nitrate of silver lotion to be injected. This entry is February 11. March 10: Cannot bear the presence of the pessary in the vagina. Tumor larger. Ordered to go into the country.

May 7th.—Has just returned from the country, greatly improved in her general health, and in respect to the feeling of bearing down.

In the foregoing examples the patients were destitute, in a great measure, of the comforts of life, and more particularly of that ease and exemption from toil which admits of the uninterrupted employment of suitable remedies. The following cases, being more favourably circumstanced, were cured.

4. Mrs. L., aged about 28, robust and vigorous. Confined of her second child. The nurse informed me that this lady, ever since her first confinement (about two years ago), has been severely afflicted with a bearing-down; which at times, even when she has been otherwise in good health, has disabled her from walking. It appears, that after her former confinement, being inexperienced and headstrong, she had, contrary to the advice of her surgeon, left her bed on the third day after labour, and in all respects had treated herself after the manner of an Indian squaw.

As I found from her own account that she had suffered much inconvenience, discomfort, and distress, until advancing pregnancy removed the complaint, she the more readily listened to my advice on the present occasion, which consisted solely in recommending *rest in a recumbent posture, as long as the sense of bearing-down should continue;* reminding her at the same time, that now was the opportunity for attempting a perfect cure, and that if she were again to leave her room, the complaint still existing, it was improbable she would ever get rid of it. For the first fortnight, my patient remained in bed, lightly covered. At the end of that time a sofa, morning after morning, was brought parallel with the bed, to which she was removed for the day. At the end of five weeks of strict adherence to the recumbent plan, she began to sit, and to use a little more liberty. On making inquiry many months afterwards, I found there had been no return of the complaint whatever.

5. Mrs. W., aged 20, was delivered by means of the forceps, after the head of the fœtus had remained 18 hours in the pelvis. It was her first labour, and not otherwise unpropitious than that it was severe and tedious. The child was feeble at first, but lived. Considerable irritation of the bladder followed, requiring the employment of the catheter. There was also soreness of the vulva and vaginal passage, and, for the first week, slight feverishness. On the 12th day after labour, when sitting up by the bed, was aware for the first time of a feeling of bearing-down, which rendered it uncomfortable to remain upright. On examining *per vaginam*, I found the passage tender, flabby, and the uterus so low as almost to rest on the perineum. I now inculcated the necessity of perfect rest in the recumbent posture, lightly covered, on a mattress in bed; and occasional ablution of the parts with tepid water. The bowels were attended to. The labour happened on the 25th of July. By the 14th of August she had removed to a sofa. At this period she was suddenly seized with fever and ulcerated sore throat, which obliged her to keep her bed nearly a fortnight. On recovering from this seizure and venturing out of bed, she found the sensation of bearing-down entirely gone. By the 12th of September, although still weakly, she was free from all unpleasant symptoms, and soon, in every respect, perfectly recovered.

6. Mrs. H., aged about 30, was delivered, in her first confinement, by means of the forceps, after the labour had continued 20 hours, and the head of the fœtus, notwithstanding severe uterine efforts, had remained at the outlet for eight hours. It was at the end of a week, when the patient began to leave her bed, that she first experienced a feeling of bearing-down in the situation of the *uterus*. This was both urgent and distressing, unless she remained in the recumbent position. I was now informed by the nurse, that the patient had difficulty in voiding her water, and that she could only expel it by long-continued straining. I was therefore induced to examine *per vaginam*, when I found the *cervix uteri* resting nearly at the *os*

externum vaginæ, and here, by its pressure on the neck of the bladder, impeding the function of that viscus. I directed her to raise the womb with her finger, when she had to void her urine. In this she partially succeeded; but on several occasions I was obliged not only to perform this office for her, but also to employ the catheter. The vagina was very tender and flabby. Soon afterwards (about the end of the fortnight), the patient was seized with an obscure and most severe affection of the head (neuralgia, I believe), which confined her to bed, with little exception, for six weeks. Long before the expiration of that period, all sense of bearing-down, as well as of difficulty in voiding the urine, had left her. Since her recovery, although she has been in the habit of walking considerable distances, she has had no return of the bearing-down.

The following case differs from those already reported, as it was the bladder alone which descended. It constitutes an example of that rare form of displacement known by the name of *hernia vesicæ*.

7. Mrs. K., aged 23, of an active and healthful appearance, two months gone in her second pregnancy, tells me (October 31st, 1831) that four months ago she discovered, in the right labium, a tumor which was soft, and about the size of a small egg. At the period referred to, she was suckling her first child, then ten months old. Says that the tumor, which has sometimes increased in size, appears in a morning after she gets out of bed, and soon disappears when she lies down at night; also that its size is much lessened immediately after she has voided her urine.

On examining, I detected a soft and slightly elastic tumor, occupying the middle of the right *labium*, on its inner or mucous surface. Pressure caused it nearly to disappear, and at the same time excited in her the desire to empty the bladder. With my finger I could readily trace the line of descent of the bladder (for the tumor was evidently produced by this viscus) behind the wall of the vagina. This was not an instance of prolapse of the bladder, as commonly described. Here was *no mucous discharge*, or flabbiness of the vagina, or perceptible *procidentia uteri*. The bladder evidently passed in its descent to the right of the median line of the anterior wall of the vagina, and coursed behind the right wall, till it reached the edge of the labium of that side. It is remarkable that there were no very urgent symptoms, saving a rather frequent call to make water, with some degree of pain and sense of dragging in the back. In other respects the health was fair, and the patient occasionally walked several miles without much inconvenience.

After the reduction of the tumor, which was easily effected, I introduced on different occasions various sizes of the common globular pessary, and always for a time with relief; but sooner or later the bladder slipped past the pessary, and this was sure to be followed by such distress as required the immediate removal of the instrument. I now employed a sponge of moderate size, which was worn only in the day. This kept the bladder from descending into the labium; but when the sponge was removed, I could feel that the bladder was not *in situ*. It had made for itself a nest pretty high up in the right side of the vagina.

Since the date of the foregoing report Mrs. K. has borne two children. By the employment of the sponge pessary, and perhaps also by the advance of pregnancy, the complaint gradually disappeared. On my attendance in the first labour, subsequent to the appearance of the tumor, I could discover nothing unusual in the vagina, although I made a careful examination. And now, after an additional interval of two years, she believes herself to be entirely free from the complaint.

The last case which I shall give, is one of simple *prolapsus vesicæ*.

On the 25th November, 1833, I was at a late hour in the evening requested by a professional friend to consult with him in a case of tedious labour, which had now continued between forty and fifty hours. The vagina, I was informed, was blocked up by a swelling situated on the pubal half of that canal —that is to say, immediately behind the arch of the pubis; and further, that, in the course of the day, the urine had been drawn off six times, followed on each occasion by, in a great measure, subsidence of the tumor. On making a manual examination, the finger came immediately on a large swelling, situated

transversely, under and behind the arch of the pubis; fullest on the left side; caused, doubtless, by the position of the patient, who lay on her left side. Beyond this swelling (which, when grasped by the fingers and thumb, poured a stream of urine from the meatus), there was to be felt, first a deep sulcus, and then, further inward, the flabby lip of the nearly dilated os uteri. The catheter being introduced, a gill of urine was drawn, when the swelling almost disappeared. It was clear that the tumor was caused by descent of the bladder.

It is foreign to my present purpose to detail the nature, progress, and termination of the labour. Suffice it to say, we found the head firmly impacted in a contracted brim, a small fold of the funis (flabby and pulseless) in the vagina, and that the delivery was with some difficulty effected by means of the perforator and craniotomy forceps; but not till rupture of that part of the cervix uteri, which was jammed during labour between the fœtal head and the projecting promontory of the sacrum, had taken place. The rent was from two to three inches in length, and ran clear through the coats of the uterus. Severe constitutional disturbance followed, and the patient had to keep her bed for upwards of a month. By the end of the second month, though still weakly, she was beginning to do for her family. At this period I made a careful examination *per vaginam*, with the view of ascertaining the situation of the bladder. I, however, could detect nothing unusual; the vagina was not flabby, nor was there any fulness behind the arch of the pubis, to indicate that the bladder was lower than it ought to be. At this time (Feb. 20) the patient has no symptoms referable to the bladder. She is, in fact, saving a degree of debility, quite well.

I might easily multiply cases of a similar description, and I might also detail others of a different kind, illustrative of the distressing effects which ultimately ensue when descent of the uterus or bladder, in the puerperal state, is neglected or mismanaged. For the present, however, I shall content myself with giving a series of brief practical deductions, all of them not perhaps fairly drawn from the foregoing cases, but at all events drawn from sufficiently ample experience.

1. Descent of one or more of the pelvic viscera is not, as is commonly believed, a disease chiefly of middle or advanced life. It is true, complete prolapse, the last miserable state of the disease, is generally found in those who have passed their thirty-fifth or fortieth year; but the first stage of descent ordinarily commences early in the child-bearing period of life.

2. In a majority (perhaps I might say in a great majority) of instances, the complaint follows a first labour; and that from causes which have already been stated.

3. When first labours are tedious, more especially when this appears to be owing, in any measure, to the great size of the child, it behoves us to be on our guard against the occurrence of the disease during the puerperal period—that is, during *the first six weeks.*

4. The complaint is curable if detected on its first occurrence, as also when the proper plan of treatment is adopted in the puerperal period immediately following that in which the complaint first appeared. But if the disease is neglected, each succeeding labour renders it less remediable. And further, when it has once manifested itself in the puerperal state, even although it may have seemed to be cured, we are bound, on every recurrence of that state, to subject the patient to certain precautionary restrictions.

5. *Rest* in the recumbent posture, which must be perfect and uninterrupted, is the most powerful, indeed the chief means of cure; of course every other measure that prudence can suggest being meanwhile pursued to invigorate the general health.

6. Astringent lotions and pessaries of every description are, in puerperal "descent," either injurious or wholly inefficient. The only lotion that ought to be employed is tepid water.

7. Those who are habitually subject to even the slightest visitings of this complaint ought never to suckle a longer period than five or six months; since prolonged lactation, next to frequent child-bearing, tends the most powerfully to produce a condition of the vagina favouring "descent*."

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* Edinburgh Medical and Surgical Journal, for April.

EFFICACY OF EMETICS.

To the Editor of the Medical Gazette.

Sir,

It has often been a matter of surprise to me, that emetics should be so little esteemed, and so seldom resorted to, at the present day; and this seems the more extraordinary, when we come to consider the important influence they exercise over the functions of the human system. Their power of evacuating the stomach is of itself no small advantage; but when with this is combined the property of exciting the action of the whole absorbent system, increasing or restoring the diminished or suspended excretions, and thus adjusting the balance of circulation, it must be allowed they are a remedy capable of most extensive application in the cure of disease. It has been supposed that all the advantages of emetics are obtained by nauseating doses of antimony, or some other medicine; but I can by no means subscribe to that opinion. It is true that both produce a general relaxation of the system, but the action of vomiting restores the circulation to the extreme vessels, which is not effected, at least so certainly, by mere nausea. This distinction is forcibly illustrated in cases of dropsy, where powerful diuretics are given, and constant nausea is kept up, but without producing any sensible effect upon the kidneys; but no sooner has an emetic been given, so as to produce full vomiting, than it often occurs that the kidneys begin to act vigorously, and that action is kept up by the same diuretics which previously were of no effect. And somewhat similar takes place in cases of obstinate constipation: the purgatives administered may occasion nausea, and perhaps some vomiting, without producing any evacuation from the bowels, and yet after the full operation of an emetic, the obstruction is not unfrequently overcome, and copious evacuations are procured.

In disorders of the stomach, the effect of emetics is very striking in what is usually termed sick headache: the emptying the stomach generally enables the patient to resume his occupations in an hour or two afterwards. In urticaria the cure is often equally speedy, when it proceeds, as it generally does, from some indigestible food in the stomach. In spasmodic asthma, an emetic generally affords immediate relief. I have known instances where opiates, antispasmodics, smoking stramonium, &c., had all failed to give relief, but where full vomiting never failed to do so.

In my early practice I have often been annoyed by the obstinate continuance of hysteria, in spite of all the fœtid drugs I could pour into my unfortunate patient; but experience has long since taught me that full vomiting is an unfailing remedy: I believe hysterics are invariably produced by food which the stomach will not digest. A female who is subject to hysterical attacks eats something of an indigestible nature, and in consequence has an attack of hysteria, or else some strong emotion of the mind renders her stomach no longer capable of acting upon its contents: in either case the effect is the same, and the remedy is to empty the stomach. In croup and bronchitis they scarcely require an advocate; their effect is too obvious, in emulging the bronchial glands, and promoting an increased secretion from the whole membrane lining the bronchial tubes. In cases of strumous glands in the neck, &c., the good effects of a course of iodine are much assisted, and the cure accelerated, by the exhibition of an emetic once or twice a week; it promotes absorption, and has a most beneficial effect upon the skin, which in these cases is seldom healthy.

From a very early period emetics seem to have been employed at the commencement of fevers, and their utility is so generally acknowledged that it would be superfluous to say any thing regarding their use; but I may here state, that in the remittent fever of children, I have found them of great utility when administered an hour before an exacerbation is expected; the secretion of the kidneys, the salivary glands, and the skin, is promoted, rendering the thirst less intense, and often inducing calm sleep, besides improving the alvine secretions and removing local congestion. In most diseases of the skin, emetics are a useful auxiliary, and in many will effect a cure, in conjunction with the warm-bath.

I might go on much longer enumerating complaints in which emetics are

eminently useful, but I have, perhaps, said too much already upon a subject on which I have little that is new to offer; but if you think these remarks worthy a place in the Gazette, I shall feel obliged by their insertion.

I remain, sir,
Your obedient servant,
EDWARD GREENHOW.

North Shields, May 1834.

CASES OF SPASM.

To the Editor of the Medical Gazette.

SIR,

IF the two cases accompanying this note seem to you of sufficient interest to deserve it, your giving them a place in your valuable journal will favour,

Yours, &c.
J. B. THOMSON,
Assistant at the Preston Dispensary.

Preston Dispensary,
16th May, 1834.

CASE I.—*Convulsions succeeded by permanent spasmodic contraction of the left hand.*

Jane Riding, ætatis 41: admitted the 20th February, 1834. Is a washerwoman by occupation. She has a florid complexion, with a habit slightly tending to plethora; and an attentive observer will discover a degree of imbecility in her look and manner. She has very marked strabismus, which her mother states to have been congenital, and, in her opinion, somehow connected with or arising from frequent convulsions during the patient's infancy. The vision is also imperfect in both eyes, but especially in the left, the axis of which is directed more towards the nose, and the pupil of which is more sluggish. On examining the head, near to the junction of the frontal, parietal, sphenoid, and temporal bones on the right side, and extending upwards and backwards as far as the parietal protuberance, is a very considerable elevation of the skull. This swelling was present at birth. With this exception, the head, as well as other parts of the body, so far as can be observed, present no peculiarity. The mother of the patient says, that she had convulsion fits from the moment of birth, without intermission, till exactly nine weeks old, when they went off entirely, and were never observed to return till she reached the advanced age of 39. Up to this period the patient was healthy, and was only troubled with occasional headaches and dysmenorrhœa. In the beginning of August, 1832, while sitting at some domestic employment, she was observed to fall suddenly on the kitchen floor, and, as her mother expresses it, *her body was all twisted together of a lump:* her hands were clenched, and drawn forcibly upwards against her chest. She did not cry or speak, and appeared not to have any sensation. There was apparent difficulty, but no stertor, in the breathing, and she frothed at the mouth. Patient herself says, that from half-past ten o'clock, A.M. till five P.M. she remained quite insensible.

From this date, the left hand has taken a fixed and distorted character. The little finger is twisted inwards, so as to rest upon the upper phalanx of the index finger, while the middle and ring fingers are immoveably flexed over the little one. Mobility is wanting, but the sensibility remains, and in the middle, ring, and little fingers, it is morbidly increased. For a considerable time after this fit, the right hand remained flexed and rigid, but has gradually improved under repeated venesections. Patient has since this time (and at present) been attacked weekly, or sometimes twice a week, with spasms and contractions of the upper and lower extremities; the arms especially are often violently twitched up against the breast: when her brother (a stout man) uses all his force to bring them into their natural situations, or plunges them in warm water, a copious perspiration breaks out upon her body, and the fit goes off. The only cause which she can assign for these fits, is the present irregular state of the catamenia, which return every second week in profusion. She has also found twitchings follow the immersion of her hands in cold water, which she has now learned sedulously to avoid. She has frequent headache, with vertigo and shooting pain around the orbits. The respiratory and circulatory systems seem healthy; the pulse 85, full and firm; bowels rather costive; tongue clean; appetite almost voracious.

Treatment.—Medicinal agents of various kinds have been used in the treatment of this case, but as the cause of the whole may be fairly believed to lie in some organic cerebral disorganization, these means were only prescribed as palliatives.

Bleedings, local and general; blisters to the affected extremity, and the surface sprinkled with strychnia; blisters to the nucha; liniments of a stimulant kind, —have been employed at different times. The constitutional symptoms were improved by general bleeding; but the contracted state of the left hand has not been affected in the slightest degree.

Remark.—To what class or order of the nosological chart are we to refer this case? Under Class II., Order III., Genus *Raphania*, of Dr. Cullen, we have certainly a near approximation to it. But the length of time during which the spastic contractions of the joints remain in this genus is only a few days, whereas the contracted state of the hand, described above, has existed since August 1832, a period of more than a year and a half. Besides, I am not aware that the cause to which Dr. Cullen imputes *raphania* could have operated in this case.

Some very important physiological deductions may be drawn from this case; but these I shall leave to the sagacity of your readers, for fear of taking up too much room in your valuable repository.

Case II.—*Spasmodic Motion of the Abdominal Muscles.*

On the 25th April, 1834, Nancy Hindle, ætatis 34, No. 1, Peter-Street, a hand-loom weaver, was admitted a patient of the dispensary. She seems a stout healthy woman, but states that she feels very weakly, and has been much reduced by working hard for a family of four children. There is a constant spasmodic action (with short interruptions) of the abdominal muscles. This first began seven months ago, when the patient was in the family way, and within a month of her confinement, and has continued more or less violent, yet never altogether absent, up to the present date. The labour went on well, and was completed favourably, notwithstanding this disease. She thinks the cause was her uncommon weakness at that period. These few weeks past the spasms have been more frequent and strong, and at present the abdomen is heaved up, and subsides regularly at the rate of from fifteen to twenty times in a minute. The belly is not agitated so much when she takes the horizontal posture, but the complaint gives almost no pain, being only of a disagreeable nature. There is no affection of the respiratory muscles, nor sonorous respiration, as in hiccough, the spasm being wholly limited to the abdominal region. Pulse natural in number, but feeble in strength. Bowels rather confined. Dr. Alexander prescribed the following—

℞. Ferri Carbonat. ℈ij.; ft. Pulv. et habeat tales xij.; Capt. j. ter die; Habeat Pil. Calomel. et Colocynth. x. sumat. j. om. nocte.

May 2d.—The patient is much improved: she feels herself stronger, and the spasmodic action of the abdomen recurs only ten or twelve times in a minute, and much less violently. Contin. pulveres.

May 9th.—The spasms have so subsided, and the general health so much improved, that she is considered cured.

Remark.—May we suppose that the distended state of the abdomen during pregnancy had in an enfeebled frame reduced the muscles from a state of tonicity to one of debility? The most curious peculiarity in this case is the length of time it lasted—the space of seven months. Doubtless there is a great tendency, in diseases of this nature, to become habitual; and cases of chronic hiccough are said to have lasted for many months incessantly, and these were at last broken through with great difficulty. In the present case, we certainly had that general weakness and irritability of the nervous system which is so apt to encourage the continuance of irregular and spasmodic action. I am not aware of any case similar to this on record.

DISCOVERY OF LYMPHATICS IN THE HUMAN PLACENTA AND FUNIS,

By Dr. V. Fohmann.

It is well known that the placenta and funis rank foremost among those parts in which the presence and distribution of absorbents have not yet been duly recognized. Some anatomists, certainly, such as Everhard, Pascoli, Needham, and Röslin, who are cited by Schreger, as also Wrisberg, and Uttini, insist that they have seen absorbent vessels in these parts; but little reliance was placed on their observations, because men of great reputation, such as Hunter, Hewson, Cruikshank, Mascagni, and others, had in vain endeavoured to demonstrate them. Moreover, as, after the establishment of Hunter's doctrine of absorption by the lymphatic vessels, new experiments were made which appeared to favour the theory of absorption by the veins, and the attempts to discover absorbents in the placenta and funis were unsuccessful, these parts were, without farther consideration, declared to be unprovided with absorbents, and were even employed as a proof of absorption by the veins. It was main-

tained that venous absorption was not only proved by numerous experiments on live animals, but also by those parts which, though not provided with absorbents, were yet organs—such as the placenta, eye, &c., in which the existence of an active absorption could not be denied. Now in this there was evidently a false conclusion. The fact that absorbents have not been found in certain organs does not prove their non-existence in those organs, but only that we have not been able to discover them. In general, physiologists relied too much on superficial examination of these vessels, and were thereby led into many errors. Thus Magendie contested Monro's and Hewson's doctrine of the presence of absorbents in all the *vertebrata;* and still denies the existence of vasa chylifera in birds. Fishes and amphibia have a perfect absorbent system; the intestinal canal of birds, and many other parts said to be without absorbents, are plentifully provided with them; and the placenta and funis are, as we shall prove, in the same predicament.

The funis consists, its blood-vessels excepted, of a tissue of absorbents, which is so close that the point of a needle cannot be introduced into any part of it without wounding one. Nothing can be easier than to inject them with mercury. Had the anatomists who asserted that they observed absorbents in the funis, been rightly aware of their situation, it would have been easy for them to convince their opponents of the truth of their statement by ocular demonstration. To fill them with mercury, nothing more is necessary than to pierce the sheath with a small lancet, and then inject the metal through a fine tube. As a great many vessels are necessarily wounded in this operation, and the mercury, on entering one branch, gets into others through numerous anastomoses, it is evident that the experiment must easily succeed. It must here be remarked that the filling of the absorbents of the funis is the more readily accomplished, the fuller it is of the *liquor Whartoni;* which no doubt arises from the fact that that fluid distends the vessels (absorbents) in which it is contained, and consequently favours the entrance and advance of the mercury. For the rest, it would be vain to endeavour to discover in the funis and placenta absorbents provided with valves, as Wrisberg pretended he saw, or as we find them after leaving the venous organs of the body. The absorbents of these temporary structures do not attain to such a degree of development, but are circumstanced as they are in the parenchyma of the organs; that is to say, they form tissues and net-works, of which the branches are valveless, so that mercury injected into any one can proceed in all directions.

If we wish to enter minutely into the examination of the absorbents of those parts by means of which the communication between the mother and fœtus is maintained, we have to consider those vessels as they exist in the placenta and funis, as well as their passage into the body of the fœtus, and their distribution therein. The funis, its blood-vessels excepted, consists solely of a tissue of absorbents, as has already been mentioned. The larger ones occupy its axis, while the smaller ramify towards the surface, and are at length lost in the sheath. The cells or vesicles usually observed in the funis are shown by injection to be dilatations of the absorbents, into which branches from the placental side open themselves, and continue their course to the fœtus on the opposite side. The absorbents of the funis become perceptibly finer at each of its extremities, and especially where they enter the cutis at the umbilicus. But before I proceed further with a description of them in that situation, I must relate what I have observed of them in the placenta.

By injecting with mercury the tissue of absorbents at the placental extremity of the funis, and driving the metal towards the placenta with the handle of a scalpel, we may sometimes succeed in injecting a net-work of absorbents that is spread out between the placenta and its investing membrane. I never saw any vessels rising from this net-work, and ramifying into the membrane, (the chorion,) as has been observed with respect to the sheath of the funis; and I but seldom discovered any branches penetrating the parenchyma of the placenta. Where they proceed when they do penetrate, I have not been able to make out; but I am inclined to think they reach its uterine surface.

On the passage of the tissue of absorbents of the funis into the abdominal region of the fœtus, the superficial ramifications at the distance of a few lines from the umbilical ring, become so very minute

as to be scarcely discoverable by a strong lens, even when injected with mercury. On the other hand, the deeper-seated vessels gain in strength while they lose in diameter; so that we can employ the handle of the scalpel to drive the mercury on through them without any fear of their rupture. On reaching the umbilical ring, they become somewhat larger, and some of them run into the dense tissue of the absorbents between the epidermis and cutis, of which the sheath of the funis is only a continuation. The rest unite into branches, which proceed under the cutis, and at the distance of some lines from the umbilical ring generate a lymphatic trunk, which, running in a circular direction, forms another ring. This gives off branches which, following the external abdominal veins, descend between the cutis and the muscles to the inguinal region, and pass under the crural arch to the iliac glands. I have invariably observed this distribution; but I have sometimes also found lymphatic branches beneath the abdominal muscles, proceeding from the funis through the umbilical ring, and following the vein or arteries into the porta of the liver, or else running to the glands which receive the vessels that descend without the abdominal muscles.—*From the Dublin Journal.*

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

The Anatomy of the Human Eye. By JOHN DALRYMPLE, Assistant Surgeon to the London Ophthalmic Infirmary.

IT is not a little singular that hitherto there has been no work in our language exclusively devoted to the Anatomy of the Eye. We have had numerous treatises on the pathology of the organ,—on the surgical treatment proper for its various complaints,—and abundance of detached papers on particular points relating to its structure, and the physiology of vision—but a systematic work, treating of the anatomy alone, has been a desideratum, till now that it has been supplied by Mr. Dalrymple. The author has taken Zinn as his model, than whom, notwithstanding the length of time since the "*Descriptio Anatomica Oculi Humani*" was first brought out, he could not perhaps have chosen a better. It shows, by the way, that on the Continent matters must have been in this respect pretty much as among ourselves; for though there have been many continental writers who have taken up portions of the eye as the peculiar subject of their study, (Rosenmüller, for example, who treats of the *external parts*,—and in whose work, published in 1810, a copious list of ophthalmological authorities may be found), yet we have to go back to Zinn, who flourished about eighty years since, as the best, if not the only complete guide, to whom we may commit ourselves.

Mr. Dalrymple of course avails himself of all the best modern authorities in giving as much of perfection as possible to his work; while the groundwork, he informs us, was originally laid for it in his lectures delivered at the London Ophthalmic Infirmary, illustrated by minute and laborious dissections.

We have no intention of proceeding to analyse for our readers a systematic and ample treatise of this sort; but to all who desire to study the anatomy of the eye, in that full detail which the great importance of the subject demands, we strongly recommend the work before us, as one of the best possible instructors. It abounds with information, very interestingly and perspicuously arranged. As a good example of this, we would refer to the account of the iris, which is the best we have met with in any author.

Yet we would by no means be understood to pledge ourselves for the perfect accuracy of the details of the volume. We noticed, indeed, several points, in our perusal of it, from which we could not help withholding our assent; for instance, in the description of the eyelids, we observe that the author admits the existence of fibro-cartilage in their structure. We confess we thought it was universally allowed that Bichât's opinion of their fibro-cartilaginous nature has been long since disproved, and that the tarsi are purely cartilaginous. Again, in the account of the sclerotic, we are told, that "in its texture it is dense and unyielding, coriaceous in appearance, and nearly resembling *a piece of white*

leather." This is a description which we fear will not readily be assented to by many observers.

In noticing the structure of the cornea, Mr. Dalrymple tells us, that, "*like the sclerotic*, the cornea is a fibrous membrane; and *the only point of affinity* between them is, that they both belong to the same general class of elementary tissues." We should think that, where the "only point of affinity" between two textures is so exceedingly faint, a comparison might very well be dispensed with. What single point of analogy, in fact, can there be said to subsist between the cornea and the sclerotic?

Nor are we quite satisfied with the account which our author gives us of the posterior layer of the cornea; we cannot avoid thinking it very imperfect. But these are trifling objections, which we are willing should have no weight in detracting from the merits of the treatise generally.

There is one opinion of the author's which is too important to be passed over without notice;—we allude to his present view (for it has recently undergone some change) of the fold and foramen of Sömmering. Mr. Dalrymple, from a late opportunity of investigation, has adopted the opinion that there is neither a real foramen, nor a fold in the retina, except as a post-mortem appearance. He informs us, that subsequently to the statement of his views on this subject, contained in the body of his work, he had an opportunity of dissecting a human eye within a very few hours after death; and that the examination was conducted under the most favourable circumstances. "On making a transverse section through the eye, about one-fourth of an inch behind the cornea, and immersing the posterior part in water, I observed the retina without fold or plica throughout. At the usual place of the spot of Sömmering, I found a minute cup-like depression, with an elevated yellow-coloured lip around it. Instead of a duplicature of the retina, it appeared as if this membrane was indeed thicker at the immediate circumference of the central hollow, but still there was *neither fold nor real foramen*. As soon, however, as the vitreous fluid partially escaped, the circular edges of the depression became elongated, elevated, and finally came in contact; presenting precisely the same fold which I had so often before remarked." The author then refers to the actual preparation in his possession, and to a drawing of which he gives a lithograph copy in his book.

What follows on this interesting subject seems to be worth extracting. "It is a curious fact that this fold is always in one and the same place, directed constantly in a horizontal line from the entry of the optic nerve outwards; it is seen in all cases where the foramen of Sömmering exists; in the fœtus, the adult man, in certain monkeys, and in some lizards. In the chameleon there are two folds placed on each side of the central depression. As in this one instance I actually witnessed *the formation of the fold*, I suspect the whole to be the result of some postmortem change; and am therefore compelled to confess that the true nature of this very interesting portion of the anatomy of the retina, is still a desideratum in our science."

The plates, we may add, which are attached to the volume, are extremely well executed; and we have, indeed, no fault to notice in the "getting up" of the work, except that the Latin extracts from Zinn and others are woefully full of errata, and will require to be carefully corrected before they are again committed to the press.

MEDICAL GAZETTE.

Saturday, May 31, 1834.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

OUGHT SURGICAL EDUCATION TO BE EXCLUSIVELY PROVINCIAL?

WE briefly alluded last week to the circumstance of the surgeons* of the Birmingham Hospital having refused to concur in the representation of those of Bris-

* We have received a letter from Dr. Booth, of Birmingham, dissenting from the opinion "that an injury had been done to the profession, by that regulation of the Apothecaries which dispenses with the necessity of any attendance in London." We ought to have used the words "surgeons" of the Birmingham Hospital, instead of "medical officers," in our last number.

tol, with regard to the proposed exemption of their pupils from any attendance in London. The question is one of much importance, setting aside all reference to the individual schools above-mentioned, and taking it merely on the broad ground of its bearing upon medical education. No one acquainted with the provincial hospitals, can deny to them the merit of being exceedingly valuable, not merely as eleemosynary establishments, but as medical schools: at the same time, assuming the remarks of the memorialists to be of general application, we must say they are more happy where they deny the superiority of certain provincial hospitals, which have privileges superior to themselves, than in what they assert regarding their own equality with the metropolitan schools. It is, we agree with them, an anomaly that Edinburgh, Glasgow, and Aberdeen, should all severally be placed on an equality with London, while no town in England enjoys this privilege; and we think it quite monstrous and absurd that attendance on a small hospital like that of Aberdeen, should confer advantages—or rather privileges, which are denied to various noble institutions in England—and among others, to that of Bristol. Unluckily, however, for their argument, the memorialists do not rest with this proposition, but go on to lay down another, viz., that requiring an attendance of even six months in London "is highly objectionable," for various alleged reasons which are specified (see Gazette, p. 191).

Among the circumstances alluded to as constituting those objections, one is, that such attendance is unnecessary, as it is asserted that a student can spend his time just as profitably at a provincial as at a London hospital—nay, it is implied that, with a few exceptions, the provincial hospitals are the better. Now we submit that the question is not whether any given hospital in London is better or worse than any given hospital in a provincial town, but simply this: whether the education of a young surgeon be more likely to be complete, and to make him a good practitioner, if he attend a provincial hospital for a certain time, and a London hospital for six additional months; or if he attend the provincial hospital for a certain time, *and then continue to attend the same hospital for six months longer?* No one can for an instant hesitate about the answer; and we have no objection, for the sake of our Bristol friends, to reverse the argument, by asserting that a student who has attended a London hospital for a certain period, is likely to be a better surgeon if he then attend the Bristol Infirmary for six months, than if he merely continue his attendance for half a year more at his original hospital in the metropolis.

Collision brightens intellect as surely as attrition polishes metal. Errors are removed, new suggestions arise, and the whole machinery of the mind is brought into more vigorous play by the differences in the modes of treatment—the new light in which the same subject is placed—the multiplied comparison, and a thousand other circumstances which are unceasingly in operation amid the vast field of observation, and the vast number of observers constantly engaged in the metropolis. It is not so much that there is individual superiority originally, as that, out of the greater number of persons who embark in the profession, and the greater extent of opportunities they enjoy, a greater mass of knowledge is collected, and ampler means are afforded to the student to appropriate a portion of it to himself. Let it be supposed that the privilege of having their students licensed to practise surgery, were given to any provincial school—say, for instance, to that of Bristol, without their having to come to London for any part of their attendance—can it

for a moment be doubted that the race of surgeons there would deteriorate—that they would fall into *local* habits and peculiarities of practice—that they would be behind most of their neighbours in availing themselves of the improvements which science made from time to time; and that if any one, more thirsty for knowledge than the rest, did visit London or Paris, with a view of completing his education, he would be regarded by his townsmen on his return with a favourable eye, as superior to his neighbours, and as one who had spared no pains to master the difficulties of his profession. One of the causes which has in a very striking degree tended to keep up the standard of knowledge among English surgeons has been the information they have gathered, and the taste and zeal they have acquired for professional pursuits, during their short sojourn in London. An impression is often made at this time which permanently influences the character of the individual, and the period of his resort to the metropolis, where his mind expanded amid the profusion and variety of intellect which surrounded him, is looked back upon with pleasure during the whole of his after-life.

But we are told that coming to London is "injurious to the morals of students." We are unwilling to suspect the memorialists of hypocrisy, but we must say that the manner in which this is put forward savours too much of canting. Do they suppose any one will credit the assertion, that their young men come here quite immaculate, and that having been impressed with all the virtuous habits which prevail in a sea-port and manufacturing town, "after a residence of only a few months in the capital, frequently fall into practices of dissipation and immorality?" The surgeons of Birmingham deny that this is borne out by experience, and we entirely concur with them. The morality of Bristol, Manchester, Birmingham, and Liverpool, must, we apprehend, be very much the same with that of London, despite the protestation of the five surgeons who would have us to believe the contrary.

The next objection made by the memorialists is, that obliging students to come to London for a part of their hospital attendance, tends to render provincial education less efficient than it otherwise would be; and this it is assumed to do by inducing the pupils to neglect their present opportunities, and postpone their studies till they go to London. Here, again, we have the gentlemen of Birmingham opposed to their brethren at Bristol; and we think there can be no doubt but that the students are in general more alive to their own interests than such a view of the case would imply; and that, for one who is injured in the manner assumed, twenty have a stimulus given to them by their visit to the metropolis, which makes them turn to double profit the opportunities at home when they return.

Again: a residence in London, though but for six months, is objectionable in the eyes of the Bristol surgeons, because it occasions "a very considerable increase in the pecuniary expenditure of the student." This is an argument of a very questionable kind. We have always thought, that in order to secure the respectability of the profession, the facilities of entering it ought not to be too much increased; practitioners are already more numerous than the demand for them,—what would it be, if the door were opened still wider? To our poor judgment, this is the most unlucky argument of all those adduced by our friends in Bristol; and here again they are opposed in Birmingham, where we find the opinion to be, that the Society of Apothecaries lessened the respectability of the profession—or at all events have

injured the members of the College of Surgeons—by dispensing with the necessity of any attendance in London. Certainly, so long as a man may become a licentiate of the Society of Apothecaries, without any portion of his studies having been carried on in London, so long will he hold an inferior rank, and be regarded as of inferior education, to a member of the College of Surgeons; and we are surprised that the notoriety of this fact did not strike Mr. Warburton's Bristol correspondents. What the result of the present parliamentary investigation may be, it is of course impossible to tell; but our belief is, that the changes, though important, will be much less *radical* than some hope or than others fear; and we trust that nothing will be found among them calculated to lessen the grade of the English surgeon, —a result which would inevitably happen, if an establishment for the production of home-made surgeons were added to those manufactories which already exist in most of the great provincial towns.

RECENT WORKING OF THE CONCOURS.

The concours for the chair of *Clinique d'Accouchemens* has closed, and M. Paul Dubois is the new Professor. The failure, or the disappointment, of M. Velpeau, the candidate who stood highest in the general estimation, has given very great dissatisfaction to his numerous friends and admirers — the concours system is once more denounced as a vile, base, infamous, and intriguing piece of machinery. MM. Dupuytren and Lebreton, who were among the judges, or the *jury*, as they are called (the crowd, we suppose, being considered as properly the judges), were not present at the nomination; whence it is said that by their absence they protested against the result. M. Paul Dubois, however, had eight votes, while his competitor had but four.

NATIONAL IMPARTIALITY.

The last number of the *Journal Hebdomadaire*, in giving an account of the numerous theses recently presented to the Faculty of Paris, has awarded to a young Englishman (Mr. St. John Avery) the praise of having produced the best thesis on Surgery. The subject, though trite enough in this country, seems to have all the charms of novelty for our French brethren. The thesis of M. Avery—*la plus remarquable,—digne de tenir un premier rang dans les collections*—was on the subject of "union by the first intention, in amputations of every sort." The reviewer (M. Vidal de Cassis), after patiently going through this, and a number of other productions of the same kind, reverts to his former judgment, and says that he has found among them but one essay of superior excellence, and that the work of an Englishman.

MEDICO-CHIRURGICAL SOCIETY.

Tuesday, May 27, 1834.

The proceedings of the evening commenced with the reading of a paper, entitled—

On the Chemical Constitution of Bony Tumors of the Uterus. By Dr. Bostock.

Some years since several bony concretions—two from the ovaries or their appendages, one from the salivary duct, and one from the bronchial cells—were analysed by Dr. B., and found to consist, all of them, "essentially of carbonate of lime;" those from the ovaries containing a little of other earthy or neutral salts, and a quantity of animal matter, but the other two being nearly pure chalk; having, however, mislaid his notes, he spoke of them from memory only. Having obtained six specimens from Dr. R. Lee, he analysed them with the following results.

No. 1, marked "calculi discharged from the uterus during life," consisted of very small, irregular, hard, bone-like bodies, weighing after drying six grains. It yielded carbonate and phosphate of lime, and a peculiar animal matter found also in the serosity of the blood. No. 2 was marked, "a portion of the calculous tumor removed from the body of a female who had passed portions of calculus from the uterus during life." It yielded much chalk, with a little bone earth and gypsum, and a portion of the albumino-serous matter described by Dr. B. in the fifteenth volume of the Society's Transactions, together with coagulated albumen as its basis. No. 3, marked "from the uterus,"

resembled externally No. 1, and was found to contain the same earthy salts and animal matter. No. 4, marked "from the arch of the aorta," weighing eighteen grains, yielded much bone earth, and a little chalk and gypsum, with a substratum of fibrous animal matter, probably coagulated albumen. No. 5, marked "calculi from the lungs," the largest weighing four grains, resembled No. 1 externally, and consisted principally of chalk, with very little phosphate or sulphate. Nos. 6 and 7, marked respectively "from the lungs after death," and "coughed up from the lungs," resembled No. 5 physically and chemically.

Dr. Bostock concluded by observing, that of eleven such calculi that he had examined, four of which were from the lungs, and five from the uterus and its appendages, the former had presented uniform appearances, physical and chemical, whereas the latter had differed amongst themselves in both respects; but he is aware of no certain source of light as to the composition of such substances, but actual experiment.

An elaborate and highly-interesting paper, by Dr. Robert Lee, was next read—*On Fibro-calcareous Tumors and Polypi of the Uterus;* but we are obliged to defer our notice of its contents for the present.

After which the evening meetings of the Society were adjourned till November next.

ROYAL INSTITUTION.

Friday, May 23.

Dr. Williams on Combustion.

The subject announced for this evening's lecture was, "A New Law of Combustion," to be expounded by Dr. Williams. We listened with considerable attention to the learned lecturer, but confess we could not gather exactly what the nature of this new law was. Many of the common experiments of burning phosphorus in oxygen gas, and sulphur in the same medium, &c., with the usual phenomena, were exhibited; and the only difference observable in the present case was that the combustible bodies were introduced into the inflammable gas in a *lower* state of combustion than usual. Wax and cocoa-nut oil were strongly heated, and then introduced into the jar of gas, where they were soon inflamed. The whole secret of the affair, or the novelty dwelt upon by the lecturer, seemed to be simply this,—that bodies are in a state of combustion at a *lower* temperature than that usually described as their burning or inflaming point. If wax, for example, be brought, in a dark room, near a piece of heated metal, it will be seen to melt speedily, with a light blue flame on its surface—which light blue flame would not have been visible under ordinary circumstances; and so with other combustibles. Some remarks on spontaneous combustion were introduced towards the end of the lecture; but we did not think that any additional light was thrown by Dr. Williams on that mysterious subject.

On the preceding Friday evening (May 16) Mr. Cowper gave a very interesting account of *Calico printing*, and described the various improvements which have lately been made in that department of the art. Dr. Lardner is announced for the next evening meeting, to give some further account of the powers of the Calculating Machine.

CLINICAL OBSERVATIONS,

Delivered at the Middlesex Hospital, by

Mr. Arnott.

LACERATED PERINEUM—REUNION BY THE QUILLED SUTURE.

While the circumstances are fresh in your memories, I wish now to revert, gentlemen, to the case of lacerated perineum, which you saw operated upon three weeks ago.

On the 25th of March, Isabella Gunnell, a widow, 36 years of age, who stooped, and seemed to walk with difficulty, sought admission into the hospital on account of, she said, "a falling down of the womb, which could not be returned." On examination, a tumor the size of a child's head was found pendent from the vulva; it felt tense and firm; its surface was red, shining, and dry; on its posterior part was a large ulcer, and at its lower extremity, a little forwards, was an aperture—the os uteri. On attempting to pass the finger upwards between the swelling and the labia pudendi, this was immediately stopped by the reflexion downwards of the vagina, so that the entire length of this canal was everted. The upper part of the tumor felt less solid than the under, and pressure on the former anteriorly, produced the inclination to make water, the bladder no doubt, having descended here; the patient experienced pain in emptying this organ, and great difficulty in evacuating the bowels.

The swelling completely concealed the anus, so that this could not be discovered;

in attempting to do so, a prolapsus of the rectum was observed, the mucous membrane of which, in its healthy state, contrasted strongly with that of the everted vagina, the latter being so changed that it had no longer any appearance of mucous membrane, being enormously thickened and fleshy, having a dry and epidermoid surface, and upon the whole more resembling the hide of some of the pachydermatous animals, than the integuments of any part of the human body.

In answer to our inquiries, the patient stated that she had been the mother of two children; that on getting up after her confinement of the last, ten years ago, the womb came down, and although it was replaced, it had continued to descend ever since. Every thing had been tried to prevent this, and pessaries of various kinds, at the suggestion of different medical men whom she had consulted, but without avail; they were invariably forced out. Of late years she had contented herself with redueing it when she sat down; for when standing or walking it was almost incessantly prolapsed, and for hours together. Six weeks ago she was for the first time unable to return it as heretofore by lying down and using pressure; but thinking that after a night's rest she should succeed in doing so, she allowed it to remain until the following morning, when her attempts proved equally fruitless; and it has continued unredueed ever since, the swelling having in the interim greatly increased in size.

The pathology of the case you have no doubt comprehended. The uterus, wholly prolapsed, was contained in a pouch formed by the prolapsed and everted vagina, the surface of which, from exposure to the air, had lost the appearance of a mucous membrane, and assumed a tegumentary character; and from the continued operation of the same cause, the friction of the clothes, &c., inflammation and ulceration of it had taken place. The poueh of the everted vagina contained, in addition to the uterus, at least a part of the bladder, and probably a portion of intestine.

Procidentia uteri is said to occur in two ways—either by prolapsus of the vagina taking place first, which, gradually descending, at length drags the uterus with it; or, what is more common, the uterus deseends, and drags the vagina *after* it. In the case of Gunnell, this probably occurred from the aecident having taken plaee in the puerperal state, when the relaxed condition of the passages, the shortening and dilatation of the vagina then existing, with the weight of the uterus itself, all tend to facilitate it.

In the *treatment* two points were to be kept in view,—the reduction of the prolapsed uterus, and the retention of it afterwards. To effect the first immediately was out of the question; there was too much swelling, tension, and tenderness of the parts. In order to diminish these the patient was confined to bed and low diet, had aperients, and warm fomentations were applied. In the course of a week (during which time menstruation took place, and you had an opportunity of observing this secretion taking place out of the body, and flowing from the os uteri), these objects had been so far attained that an attempt at reduction was made, but without success; however, on the following day it was accomplished, though with considerable difficulty. You must have noticed that I did not endeavour to force up the whole swelling, or the upper part of it, that which had been last prolapsed, first, and which, from this circumstance, you might possibly have expected. No; grasping the lower part of the swelling (that immediately round the os uteri) with my fingers, and making steady and determined pressure, I carried the uterus upwards, through the pouch of prolapsed vagina into the pelvis; and as this moved, the vagina folded in and followed, the thick and unpliable condition of which was a main obstaele to the operation. You saw that we did not wait until the ulceration of the vagina had healed, to effect the reduction; occasioned, as this ulceration chiefly was, by the unnatural situation of the parts, it would heal sooner when this was remedied. I may mention that you are not to be discouraged from attempting reduction by the length of time the prolapsus uteri has existed. Sabatier, in his Memoir on this subject, in the Memoirs of the French Academy of Surgery, relates a case where a uterus which had remained prolapsed for four years was reduced in eight days.

When the uterus had been replaeed, and we had the opportunity of examining the condition of the parts at the entrance of the vulva, we found our patient to have a much more serious affliction than the prolapsus uteri. There was no longer a partition between the rectum and the vagina; the perineum had been lacerated through its whole extent; the laceration extending through the anus a little way into the rectum; and it was owing to this laceration of part of the bowel that eversion and prolapsus of its mucous membrane had taken place, for this did not exist posteriorly. The reason why all attempts had failed to keep up the uterus by means of pessaries was now explained; and on inquiring of the patient when this aecident had occurred, she stated that it was during her last labour. There seemed to have been

nothing unnatural in this; she was attended by a female, who, though in the room, was not with her exactly at the moment when the child's head was born; she herself was not conscious that any thing unusual had occurred, and it was not until a fortnight or three weeks after that she ascertained (to use her own language) that she "was broke." Although the laceration extended into the bowel, she stated that she possessed the power of retaining her fæces, but she added, that when she had a call to have a motion, this sometimes followed so rapidly that she had not time to prepare for it.

In order to effect the second object of our treatment—namely, to retain the uterus after the prolapsus was reduced—it was evident that we must primarily endeavour to remedy the state of parts which was now displayed. To do this, you are probably aware that an operation, on the same principle as that for hare-lip, has been performed; the edges of the cicatrized rent in the perineum are pared off, and the raw edges brought together by suture. For many years, the suture employed, both in this and other countries, had been the common interrupted one; but with so little success that many surgeons have thought it useless to attempt the operation. To remedy this, a Prussian surgeon (M. Dieffenbach), a few years ago, suggested another method of operating, which he had practised with success. Having pared the sides of the cleft, he makes one stitch of interrupted suture in the middle, and then introduces two hare-lip pins with the twisted suture anterior, and two posterior to this; and lastly, makes a curved incision through the integuments on each side of the united cleft in the perineum, so as in some measure to insulate this, and free it from all tension and stretching. M. Dieffenbach relates two successful cases of this operation, which has been repeated, with an equally favourable result, by Mr. North. I was about to try it in this case, when Mr. Lonsdale, a late intelligent house-surgeon of this hospital, who had just returned from Paris, informed me that he had either seen M. Roux operate, or a case in which M. Roux had operated with success, by means of the quilled suture; and having at the same time met with an account of this surgeon's operations (four of which had been successful) in Dr. Johnson's journal, I determined to adopt it here. This operation by the quilled suture had proved at least equally effectual, and certainly was less severe than M. Dieffenbach's. In considering, too, the causes of failure of the common interrupted suture, I cannot think that this is owing to there being any tension or stretching of the parts; these come readily into contact, and no straining or stretching is required to bring them so. When I find, then, M. Dieffenbach using four hare-lip pins, as well as the lateral incisions, I cannot help believing that the kind of suture, by keeping the raw surfaces more extensively and steadily in contact, had as much influence in the success of the operation as the incisions. I do not mean, however, to deny that these lateral incisions will be of service if the interrupted suture is employed; for by setting the parts free, and rendering them less susceptible of disturbance by motion, union will be facilitated. If, however, by any kind of suture, the raw surfaces can be kept *completely*, *uniformly*, and *permanently* in contact, the success of the operation would probably be as great without the incision as with it. The quilled suture seems to attain these objects.

The success of every operation, you are well aware, gentlemen, depends very much on the condition of the patient; and that of Gunnell was not such as to lead me to put the proposed one in practice immediately. The abdomen or pelvis did not appear to be readily reconciled to its renewed occupancy by the uterus; for the return of this occasioned feelings of uneasiness and pain, which continued for some time, and were relieved by cupping on the loins, castor oil, &c. The state of the vagina, too, was such as to require that some time should elapse, by which it might get into a more healthy state; the patient was therefore continued in bed for a month before any thing further was attempted. Low diet was resumed a few days before the operation; and as it was desirable that the bowels should be kept in a state of perfect quietude for some days afterwards, they were now well cleared out. As regarded the state of the parts more immediately concerned, from the many years which had elapsed since the occurrence of the accident, the lips of the rent of the perineum were in a favourable state; they were generally smooth and pliable, with only at one part some degree of induration; indeed, so completely had their surfaces assumed the character of the adjoining parts, that it required close observation to distinguish their limits from those of the vulva generally. The operation was performed on the 2d instant; the surfaces of the cicatrized cleft in the perineum were shaved off to a considerable extent, the raw surfaces on each side having the shape of an irregular right-angled triangle, one side of which, corresponding to the raphé of the perineum, was more than an inch and a quarter in length, and the perpendicular,

that next the rectum, was three quarters of an inch in height; the anterior angle (if it may be so called), was rounded, or open. Three ligatures were introduced, for the purposes of the quilled suture. In placing these, the points of the needles were entered at the distance fully of an inch from the edge of the rent, on one side carried from without inwards deep into the wound, and brought out from within outwards on the other, at an equal distance. This is the only troublesome part of the operation, from the involuntary retraction of the parts by the patient, the difficulty of turning the needles being increased. A piece of bougie was now introduced into the loops of the ligatures on one side, and the other extremity of these being tied over another piece of bougie on the opposite one of the wound, the surfaces of this were brought nicely and firmly into contact. M. Roux alleges, that in using the quilled suture, the wound on its outer edge gapes a little. This certainly did not occur in the present instance; but in case that it should, I adopted his plan of causing each needle, as it carried the double ligature to take a single thread with it, and these three single threads being in, were now tied across the wound, as so many points of the interrupted suture.

On being sent to bed, the patient was desired to keep as quiet as possible, and to take care and retain her thighs in apposition. For some days subsequently the bladder was daily emptied by means of the catheter, not on account of any inability on her part, but to prevent the urine coming in contact with the surfaces of the wound.

The day after the operation she complained of pain in the lower part of the abdomen; the pulse was frequent, and somewhat resisting: she was therefore let blood from the arm.

On the second day, the parts included between the pieces of bougie were swollen; and as the outer edges of the wound were adherent, I cut the three single threads.

On the third day she complained of pain in the perineum and lower part of the belly. There was some oozing of purulent matter from around the ligatures, and more frequency and firmness of pulse than I liked; so I had her again bled to fourteen ounces, and desired an enema to be administered in four hours, if the pain was not relieved.

On visiting her early in the morning of the fourth day, I found that the enema had been given the preceding evening, and had brought away a motion; and Mr. Elwyn, finding the pain still continued, gave her five grains of calomel and one of opium at bed-time. She still complained of the same pain, and also of pain in the perineum, particularly on coughing, but the abdomen was soft, and not tender to pressure. Six drachms of castor oil were given immediately, which operated freely several times in the course of the day; but in the evening, finding the pain had not materially abated, and the pulse still frequent and resisting, sixteen ounces of blood were taken by venesection, and two grains of calomel, with a quarter of a grain of opium, were ordered, to be continued every three hours, if not followed by purging.

On the morning of the fifth day the pain was relieved, though not removed; the blood drawn yesterday was buffed. Having been purged by the pills, they were omitted after the second dose, and five grains of hyd. c. creta substituted. The state of the parts in the perineum, as seen externally, was promising; but as the pieces of bougie seemed now a source of uneasiness, the loops of the ligatures were cut, and the former were removed, the ligatures not being withdrawn until the following day, the sixth after the operation.

On a close examination, on the eighth day, the parts were found to be perfectly united, and to the extent of an inch and a quarter in length from before backwards. In this instance, also, as in all the cases of MM. Dieffenbach and Roux, a point next the anus, that corresponding to the cleft in the bowel, did not unite in the first instance, and it still exists, though it seems contracting; according to the experience of these gentlemen, it will gradually heal, which process we are endeavouring to promote by touching it with the nitrate of silver.

The success of the operation, then, as regards the rent of the perineum, has been complete,—as complete, in this case, as those of the surgeon whose method of operating was adopted; and we may congratulate ourselves on this, not only as remedying a very distressing infirmity, but also as being a most important step gained in our attempts at preventing the return of that displacement of the uterus for which the patient originally sought our assistance. I mean, that by this reunion of the rent perineum we have obtained (to be used, if necessary, and in due time) a support for a pessary. But I hope we may have derived other advantages from the inflammation which followed the operation,—inflammation which I believe to have been seated in the diseased vagina, and which, though it gave me some anxiety lest it should extend to more important parts, and get beyond our control, yet of which, tractable as it proved, I have not regretted the occurrence. In consequence of this inflammation, I hope the vagina may have now become reunited

in its attachments to the parts in the pelvis, and that an obstacle is thus placed to its re-descent.

Before leaving this subject, I may state that it has sometimes been a question whether re-union of the perineum lacerated during labour should be immediately attempted or not,—a question which seems to have arisen from the frequent failure of the attempts to effect this by the interrupted suture, and these failures being attributed in part to a wrong source. An undue importance has been attached to the influence of the lochial discharges in preventing this re-union: we do not find that, either in the child or the adult, the success of the operation for hare lip or cancer, is prevented by the flow of saliva, or the introduction of liquid food; and probably because, by the suture we employ, the raw surfaces are so kept in contact that these fluids do not reach them. Now, what the twisted suture does for the lip, the quilled suture would appear to do for the perineum; and therefore, although a lacerated wound is not in so favourable a condition for union as an incised one, yet, considering that you have a raw surface ready made, and that without cutting,—that such surface has repeatedly united, and considering the importance of an immediate cure—I think you will agree with me, it would be advisable to try the quilled suture, as a simple, and even in the puerperal state, a safe means, for obtaining a most important end.

OPERATION FOR FEMORAL HERNIA—PERITONITIS.—UTILITY OF LARGE DOSES OF OPIUM.

In alluding to Cox's case, that of femoral hernia, operated upon a fortnight ago, I may advert to two circumstances attending it. The patient was admitted at noon on Thursday, the hernia having been incarcerated from the preceding Tuesday: attempts had been made to reduce it on the Wednesday, to facilitate which, bleeding, &c. had been tried. The case being urgent, the operation was performed immediately on the patient's entrance. There was no serum in the sac, and the contents, a loop of bowel, was adherent almost throughout to the sac, but so recent as to allow the latter to be peeled off. When the stricture was divided, one end of the bowel was readily returned; but although the aperture was now free, the other could not be reduced until it was pulled down a little, when, either in consequence of some adhesion being thus destroyed, or a twist altered, this end was also easily returned. This case, then, was not one in which the operation could have been performed without opening the sac,—a method of operating, however, which is to be kept in recollection, when you are called upon to operate very soon after the occurrence of strangulation.

The bowel was considerably inflamed, and red serous fluid flowed from the cavity of the abdomen on its being reduced: although copious motions were obtained in a few hours after the operation, the indications of peritonitis continued and increased, requiring venesection at night, and again the following morning. After this second bleeding, and although the bowels were being freely moved, the pain and tenderness of the abdomen, increased by pressure, still continued, with an anxious countenance and dry tongue. The patient, who was feeble, and had been previously an invalid, would not have borne further general depletion; and I was unwilling to apply leeches, because I have seldom seen them of positive advantage in these cases, and think I have seen their application (no doubt from the cold and slopping which attends it) do harm. I therefore now ordered three grains of opium to be administered; and when I returned, four hours subsequently, to see the patient, her expression was that she now felt herself in heaven, the relief which she had experienced had been so striking. As pain and tenderness still however existed, two additional grains of opium were given; and on the succeeding morning, our fears as regarded the success of the case were in a great measure removed; she has since continued to do well. I have thought it right to notice to you the advantages of a large dose of opium in this case, as I believe its employment in peritonitis, after the operation for hernia, is not very general. You will recollect, however, gentlemen, the circumstances under which it was employed.

DR. RIGBY'S

MIDWIFERY HOSPITAL REPORTS.

GENERAL LYING-IN HOSPITAL.

[Continued from p. 256.]

UTERINE HÆMORRHAGE.

A CONSIDERABLE number of cases attended with hæmorrhage have occurred during the year; of these it came on *before* labour in only one instance to such an extent as to be worth noticing.

Oct. 2d.—Mary Mahony, æt. 28, delivered of a girl; ninth child; first position.

Came into the hospital last night with slight pains. About 11 she got up to walk about, when she was suddenly seized with

a profuse hæmorrhage, which in less than a minute filled a chamber-pot more than half full, and covered the floor around. She was placed upon the labour-bed, and kept as cool as possible; some vinegar and water was thrown up the vagina, and a cold sponge applied to the vulva by the midwife. When I arrived, the hæmorrhage had ceased; the os uteri was open; the head, and membranes filled with liquor amnii, presenting. Pains were coming on briskly; the membranes burst, and a large quantity of liquor amnii, tinged with blood, escaped; an immense head followed, and as large a child in proportion: it weighed ten pounds. The placenta was also very large, and much lobulated.

This is a case of that species of hæmorrhage occurring before labour which my father has termed *accidental*, to distinguish it from that form of *unavoidable* hæmorrhage which results from the placenta being upon the os uteri; and if it had returned, would have been quickly stopped by rupturing the membranes. A considerable portion of the placenta must have been detached from the uterus; but although I examined it afterwards, I could not ascertain the extent of the separation.

2d July, 1833.—Sarah Buchan, æt. 37, delivered of a girl; eighth child; third position; twins in her last pregnancy. Felt the diverging edges of the lambdoidal suture towards the right sacro-iliac synchondrosis, and could only reach the edge of the anterior fontanelle forwards, and to the left.

A considerable hæmorrhage followed the birth of the child. The uterus was hard, but still the blood flowed profusely. The placenta being within reach, I tried to bring it down, but it required some force to make it stir. A very large placenta followed; the uterus contracted hard, and seemingly small, but a slight hæmorrhage continued; a large coagulum came away, and it stopped. In a few minutes a slight trickling of blood again appeared, although the uterus was quite hard and small: it continued, in spite of cold vinegar cloths to the abdomen and loins, a dose of ergot of rye, and injection of cold water. Finding these measures produce no effect in stopping this discharge, I examined, and having extracted a long slender coagulum, it stopped permanently.

July 4th.—Has felt quite easy; bowels open twice yesterday. Milk appeared copiously this morning. Complained of headache after breakfast, and at 11 was seized with a shivering fit, with further increase of headache. A linseed-meal poultice was applied to the abdomen to induce sweating, there being much febrile heat. A dose of magnes. carb. and sulph. with liq. ant. tart. ℥xxv. was given, and a copious perspiration followed. The bowels not operating, an enema was given at 12, which produced a large costive evacuation, and the bowels were again opened at 1. This produced much relief, and she slept from two till five, when I saw her. On waking, she was much confused, and had a wild expression. Pulse 133, soft; skin dry, and very hot; tongue dry in the middle; much headache. The magnesian mixture and liq. antim. tart. was repeated twice, producing a free perspiration and relief in the bowels, which eased her greatly. Pulse 120; feels easy.

It has been a subject of some controversy, whether we are to hasten the expulsion of the placenta upon the appearance of hæmorrhage or not. The old plan of pulling away the placenta immediately after the birth of the child cannot be too strongly deprecated; but the other extreme is quite as objectionable. The appearance of hæmorrhage shews that a separation of the placenta from the uterus has taken place; and we may generally form a tolerably correct idea of the extent to which this has gone by the degree of the hæmorrhage. Whenever this is at all profuse, and especially if the placenta be within reach, there can be no question as to the propriety, and even necessity, of removing it as soon as possible. Where it is not within reach, and hæmorrhage comes on, our practice must be chiefly guided by the state of contraction in which we find the uterus: if it be soft and large, our first object must be to excite it to contract, by means of circular friction, sudden cold, &c. which will seldom fail to bring the placenta into the vagina. Where, however, the placenta is beyond the reach of our finger, the uterus hard and firm, and our patient still continues to flood, it shews one of two states to be present: either there is anormal contraction of the uterus (hour-glass), which prevents the placenta coming away, or the placenta itself is partially or wholly attached to the uterus by a preternaturally firm adhesion; in either of these cases it will be necessary to introduce the hand for its expulsion: but for a further consideration of this subject, I must refer to the next case. It not unfrequently happens, that after the expulsion of the placenta, a trickling of blood continues, although the uterus be firm, hard contracted, and apparently little, if at all, larger than it ought to be. The usual remedies for stopping hæmorrhage fail here; and where this is the case, we may be almost certain that the uterus is prevented from complete contraction by the presence of coagula in its cavity. The hand is seldom required to be introduced for their

removal; the first and second finger, acting as a forceps, are generally sufficient; for, as soon as we succeed in stirring the clot, the uterus usually contracts and expels it. The febrile attack from which she suffered afterwards, was a form which was very prevalent during the summer months; but I shall delay making any observations upon it until I come to the puerperal affections.

15th March 1833.—Mrs. Neehan, out-patient, æt. 29; second pregnancy; aborted in her first at the third month. The midwife says that she had pretty brisk pains on the 12th. Scarcely any liq. amnii. Head near the inferior aperture of the pelvis; slight swelling of the scalp; pains ineffective; quite inarticulate from hoarseness. Being a stout, plethoric young woman, the pulse quick, the vagina hot, &c., I bled her from a large orifice to about ʒxviii., and then passed Dr. Hopkins's forceps, which at first did not pass easily, from the head being firmly pressed against the bones of the inferior aperture. The head at first did not stir; but a second traction brought it more suddenly than I had expected, and a portion of the perineum was lacerated: the rest of the child followed easily; but although the fundus uteri became hard, still it was very high, and distant from the pubes, and the placenta did not stir. After waiting some time, I was obliged to introduce my hand, when I found a stricture of the upper part of the body girding the lower portion of the placenta. The whole surface adhered very strongly; and at last, after a considerable effort, it gave way. The whole circumference of the placenta was entire; but in the thickest part, corresponding to the insertion of the cord, the placenta presented a deep excavation, showing that a large portion had been torn out, and left behind. No hæmorrhage followed.

March 16th.—Has passed an easy tranquil night; no pain; discharge moderate.

March 18th.—Bowels opened twice by a saline laxative; no pain; quite comfortable; discharge moderate; no fœtor. Says that about three weeks ago she had a fall, by which she bruised her elbow severely, but did not strike the abdomen.

March 24th.—Feels quite easy. Lochia sparing, but without fœtor; no pain whatever: *nothing solid has come away.*

This case is interesting for several reasons. The exact nature of hour-glass contraction of the uterus has been by no means a settled point among accoucheurs: it has been called *hour-glass contraction* in this country, from the uterus being supposed to be spastically contracted in the middle, as if girt by a tight ring. This fact, however, has been called in question by several authors of high rank in the profession, and the stricture has been attributed by them to be produced by the contraction of the os uteri: thus, for instance, Baudelocque considers that the circle of fibres which corresponds to the os uteri internum, being proportionably more stretched during labour than the other parts of the uterus, is likewise more disposed to contract. W. J. Schmitt, of Vienna, even goes further, and asserts that the stricture is formed by the os uteri externum. This I am inclined to doubt; but I am perfectly convinced that in many cases it is produced by the os uteri internum, as described by Baudelocque. A case of this sort, which occurred in the practice of Professor Naegelé, and was communicated, at my request, to the Medical Gazette, January 10, 1829, by my friend, Dr. Merriman, bears a striking resemblance, in many points, to the one I have now recorded. In all the cases which I had hitherto had the opportunity of observing, I had found that the stricture was as described by Baudelocque; and in my own mind felt convinced that this was the real state of things in hour-glass contraction; but, in the present case, the stricture was too distinctly above the os uteri to allow me to doubt for a moment; and when we recollect that the placenta adhered to the fundus, and that its lower portion was included in the stricture, there is greater reason, I think, for supposing that the stricture was in the body of the uterus.

The nature of these preternatural adhesions between the uterus and placenta is not very correctly understood; nor do I know of any instance where they have been properly examined by dissection: they have been generally considered to be the result of an inflammatory action of the uterus at this point; but it is a mere opinion, unsupported as yet by any facts. The portion of placenta which was left adhering to the uterus was very considerable, being at least one-third of the whole mass; and, if I had not been aware of the absorbent action of the uterus upon the placenta, when retained for a certain time, I should have been induced to pass my hand again, and attempt the separation of this portion; but feeling quite convinced of it, from the numerous facts recorded by Dr. Salomon, of Leyden, Naegelé, and others, I considered myself justified in leaving the rest of the case to nature. There was no fœtor of the lochia, which throughout were sparing: it was also ascertained, beyond all doubt, that no solid substance had passed.

Cases of abortion every now and then occur, where the embryo has escaped, but where the secundines have not appeared; and where no trace whatever of them has been observed afterwards. After a time the catamenia have returned; and, in se-

veral instances, the patient has again become pregnant, without any peculiar symptoms.

Cases have been recorded, where the whole or portions of the placenta have been retained for several days, and which have been expelled without any sign of putridity; their surface and edges smooth, and rounded off by the action of absorption.

Dr. Denman has given a case where the placenta, after being retained fifteen days, was expelled with little signs of putrefaction, except upon the membranes; the whole surface which had adhered exhibiting marks of fresh separation.

Dr. Young, professor of midwifery at Edinburgh, in a MS. copy of his lectures, which was taken by the late Dr. Parry, of Bath, gives an interesting case of this sort where he could not bring away the placenta: the patient had been delivered two hours, and the cord was broken. "I laid the woman," says Dr. Young, "upon her side, and introduced my hand; but could not get hold of the placenta: I could get my hand to the placenta, but no further, the uterus having formed a kind of pouch for it; so that at last I was obliged to trust to nature. *What was very remarkable, the placenta never came away, and yet the woman recovered.*"

A similar case, where the whole placenta was absorbed, has been recorded by Professor Salomon, of Leyden, and which was so carefully watched as to prevent any doubt whatever as to its correctness and authenticity *. Professor Salomon, in his essay upon this subject, remarks, that where the uterus has contracted firmly upon the placenta, so as to be everywhere in close contact with it, the access of the external air has been thereby prevented, and the enclosed mass of placenta has not become putrid, which otherwise would have been the case. "The circumstance," says he, "of the placenta not becoming putrid, has served to protect the patient from those highly dangerous symptoms which have been observed in other cases of retained placenta, as early as the eighth day after delivery; and which, under the above-mentioned circumstances, have cost the patient her life."

I have dwelt a little more fully upon this case, because the facts therein stated have met with but little notice in this country; but they are important in a practical point of view, and deserve attention.

The management of hæmorrhage after labour I have already in part discussed, when speaking of Sarah Buchan's case. One great object, after the expulsion of the placenta, is to induce firm contraction of the uterus; and it is of great importance that we should be acquainted with a number of remedies for this purpose, in order that if one fail, we may have the chance of succeeding with another. Friction of the abdomen has lately been held up as being preferable to the application of sudden cold: it is certainly a valuable remedy; but it has been even recommended to grasp the uterus in our hands, and squeeze it,—a proceeding, to my mind, highly objectionable, as not only producing much suffering to the patient, but also liable to injure the uterus itself, when distended with firm coagula. I have never seen any ill effects produced by the application of cold, whether applied externally, or internally in the form of injections. This latter treatment ought not to be had recourse to, unless the external application of cold has failed, which is seldom the case, if properly managed. The mode of its action in hæmorrhage from uncontracted uterus, is precisely the same as that of the well-known popular remedy in epistaxis, viz. where a cold key is put down the patient's back; a sudden shock is produced; a constriction of the cuticular vessels results, with which those of the bleeding parts sympathize. Where we want to produce the full effects of cold, the abdomen should be dried with a warm towel between every application. Where this fails, and the danger is becoming urgent, and we have not the means of throwing up a cold injection into the vagina, nothing has so powerful an effect as water poured upon the abdomen from a height: even where ice itself has failed, this has succeeded instantly, although the water used for the purpose has been of course many degrees warmer. I believe it was in the case of a relation of my own, where the value of this practice was remarkably illustrated by the late Dr. Gooch: he had received an urgent message from a practitioner, intreating him to come instantly, as his patient was flooding to death: he found her pale and speechless, and in the greatest danger: the abdomen was covered with pounded ice, but still the uterus refused to contract, and the flooding continued profusely: he instantly swept off the ice with his hand, and taking the ewer, which was filled with water at the temperature of the room, and standing upon the bed, emptied it upon the abdomen of his patient from a considerable height: the effect of this shock was instantaneous, and the uterus immediately contracted. Injections into the vagina of cold water, or vinegar and water, are also a very powerful means of contracting this form of hæmorrhage, and almost certain of pro-

* Waarneming eener ziekte in het Kraambed veroozaakt door het terugblijven der nageboorte, en dezelver genesing door de natuurkrachten, door middel van de absorbtie der placenta. By Dr. G. Salomon, of Leyden; in the second Part, No. 2, of the Genelskundige Bijdragen door B. Pruys van der Hoeven, J. Looger, &c.

ducing the desired effect: still, however, we occasionally meet with cases, where, in spite of all these means, the contraction was only temporary; and, as soon as the immediate effects of the shock are over, the hæmorrhage returns as before. In this case I know of nothing so certain in producing *permanent* contractions of the uterus as the application of the child to the mother's breast; but as I have very recently communicated to you some observations upon this subject, I must refer your readers to the Gazette (Feb. 22, 1834) for further particulars.

March 17, 1833.—Sarah Woods, æt. 37; fifth pregnancy; prolapsus of the cord, with deformed pelvis. In all her previous pregnancies the children have been born prematurely, and dead. Has gone her full time. Membranes ruptured at 6 A.M.: profuse quantity of liquor amnii. Came into the hospital at eleven. I saw her at twelve, and found a large coil of the funis prolapsed, which pulsates strongly, but unusually slowly for the fœtal pulse: os uteri dilated to about the size of a crown piece; head presenting, high up and moveable; scarcely any pains.

Judging that the fœtal circulation would become more active when the cord was freed from the pressure of the head, and from the absence of pains, I determined to turn. The os uteri yielded; and passing along the head and neck, my hand came to a foot, which I brought down: the other foot came barely within reach to grasp sufficiently firmly; but, being unwilling to increase her sufferings by passing my hand up again, I brought down the breech in this manner: it came with a good deal of difficulty. The toes at first turned forwards; but as the breech came upon the perineum, the right ischium turned forwards; the pulsation of the cord had ceased, but still the whole trunk of the child as it advanced became purple. The pains being slow, I endeavoured to increase them by a dose of ergot of rye, but it produced very little effect. I endeavoured to bring down the arm which was turned forwards, but could not reach it; and after some time, brought down the left. In this position the child remained for some time. I could just reach the right arm, which was behind the pubes, and rather to the left, and at last succeeded in hooking it down with two fingers, in doing which the humerus broke, as I used the greater force, knowing, from the state of the cord, that the child had been dead some little time. Passing two fingers into the mouth, and then upon the malar bones, the head descended almost immediately, and came away very quickly: the placenta followed soon. The child's head was remarkably flattened, so that the parietal bones had lost their convex form, and appeared nearly parallel in all directions; the left malar bone bore a deep depression, where it had corresponded to the promontory of the sacrum, which I could easily reach with my finger.

March 18th.—Had a smart attack of pain in the abdomen, and much fever. Eighteen leeches were applied, followed by a hot linseed meal-poultice; and she took eight grains of calomel, with four grains of pulv. antim.

March 19th.—Has had four or five large fæculent dejections: feels free from pain, and easy; pulse natural.

A variety of causes have been enumerated as tending to produce prolapsus of the cord, but I suspect that their number may be diminished to a very few. An unusual quantity of liquor amnii is undoubtedly one of the most frequent; nor do I, indeed, know of any other distinct cause. The uterus is now so distended, that the presenting part of the child does not fill up the lower part of it sufficiently, and thus allows a portion of the cord to prolapse.

I was not aware how narrow the antero-posterior diameter of the upper aperture was until I had turned the child: the patient was to all appearances perfectly well formed; and from her description had given me the impression that her previous labours had been accompanied with malposition of the child; I had, therefore, entirely directed my attention to insinuating my hand as gently as possible; and, from its being pretty firmly compressed between the uterus and different parts of the child, I had not observed any deviation from the natural form of the pelvis. If this point could have been ascertained, it would have made no change in my practice. The only other means of delivery which was justifiable in this case would have been the forceps; but, in the first place, it would have been very difficult to avoid including a fold of the cord; and, secondly, the passages not having had the advantage of being dilated by the previous passage of the child's body, the head would have passed so slowly, and with so much difficulty, that all chance of saving the child's life must have been lost. In cases where the membranes have not yet ruptured—where the passages are roomy, the pains strong, and her former labours have been rapid and easy—in fact, where there is every reason to expect a quick labour, the head may be allowed to descend into the pelvis, and when within reach, the forceps may be applied, and labour safely terminated: but where, from the history of her former labours, and from present circumstances, we have no reason to expect a quick labour, turning is the only means of saving the child's life.

[To be continued.]

A CASE OF SCORCHING BY ELECTRIC FLUID.

By M. Volpeliere, of Arles.

A very delicate woman, 68 years of age, of a nervous, bilious, temperament, while closing her window on the evening of 26th of May, 1829, during a violent thunder storm, with torrents of rain, was struck with lightning. The electric fluid proceeded from the iron fastening of the window along her left hand and sleeve, till it reached her shoulder, whence it descended along the thorax, abdomen, groin, internal part of the thigh and leg, to the sole of the foot, keeping always on the same side. She fainted, fell backwards, and sustained a contused wound in the occipital region. After remaining an hour in this state of stupor, she with difficulty got up, and threw herself on her bed till day-break, when she called her neighbours. On visiting her soon after, M. Volpeliere found her with a wandering countenance, feeble concentrated pulse, and cold skin. All the parts touched by the electric fluid were scorched. In some places the epidermis was raised, and the thigh presented an erysipelatous redness. Simple cooling dressings were applied, and a stimulant exhibited. On a second visit, the strength was a little restored. On examining the patient more minutely, it appeared that the skin had acted as the conductor of electric fluid. The sleeve of the shirt was not burned, except at the shoulder, where, at the time of the accident, the arm formed with the body an angle of 46°. She had a cloth under-vest, lined with scarlet. Wherever the electric fluid had been delayed for a longer or shorter period, the scars were larger and deeper. The wrist, shoulder, and upper part of the pelvis, were severely burned. The inner surface of the thigh, at first of an erysipelatous red, presented soon after the re-action an enormous wound. The leg, confined by the garter, did not arrest the course of the fluid. There were observed here only two holes, one below the knee, and the other above the *malleolus internus*. Re-action took place several times, but was subdued by abstinence. After the separation of the eschar, the cicatrization of this enormous wound took place; but upwards of two months after the accident, the patient died of gastro-enteritis.—*Dublin Journal.*

POISONING WITH MUSCLES.

A case of this description, classified by French pathologists as one of *toxemia*, was lately treated in the Hôtel Dieu. The malady did not arise from the quantity of the shell-fish eaten, but its quality; and the patient (a female) was affected with colic, diarrhœa, and vomiting. Bleeding and large potations—*pour remedier à l'état general du sang* — were the therapeutic means employed, and the woman got well in six days; the case being rendered complicated and tedious, as it is said, by *gastro-intestinal accidents.*

WEEKLY ACCOUNT of BURIALS,

From Bills of Mortality, *May* 27, 1834.

Abscess	2	Hooping-Cough	23
Age and Debility	54	Inflammation	26
Apoplexy	3	Bowels & Stomach	5
Asthma	10	Brain	6
Childbirth	2	Lungs and Pleura	6
Consumption	89	Insanity	3
Constipation of the Bowels	1	Jaundice	3
Convulsions	29	Liver, diseased	6
Dentition or Teething	3	Measles	13
Dropsy	22	Mortification	3
Dropsy on the Brain	11	Paralysis	4
Dropsy on the Chest	4	Small-Pox	9
Fever	11	Spasms	1
Fever, Scarlet	3	Thrush	5
Gout	2	Unknown Causes	77
Hæmorrhage	2	Stillborn	34
Heart, diseased	6		

Increase of Burials, as compared with the preceding week . . . 157

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

May, 1834.	Thermometer.		Barometer.	
Thursday . 15	from 51	to 70	29·83	to 29·78
Friday . . 16	49	66	29·70	29·61
Saturday . 17	46	64	29·47	29·36
Sunday . . 18	40	62	29·37	29·49
Monday . . 19	37	65	29·65	29·83
Tuesday . . 20	39	70	30·04	30·21
Wednesday 21	36	69	30·35	30·38

Prevailing wind, S. W.

Rain at times on the 15th, 17th, and 13th, otherwise generally clear.

Rain fallen, ·275 of an inch.

Thursday . 22	from 43	to 67	30·33	to 30·27
Friday. . . 23	45	70	30·24	30·23
Saturday . 24	41	72	30·25	30·29
Sunday . . 25	40	71	30·29	30·24
Monday . . 26	37	69	30·24	Stat.
Tuesday . 27	39	69	30·21	30·18
Wednesday 28	42	68	30·13	30·11

Wind, N.E.

Clear, except a few clouds passing at times.

Charles Henry Adams.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JUNE 7, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XXXV.

DIFFICULT LABOUR.—INSTRUMENTS.

LONG FORCEPS.

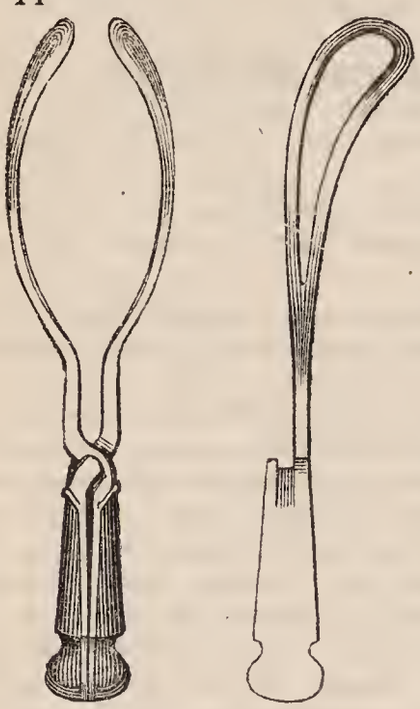
One of the most valuable instruments employed in midwifery is the LONG FORCEPS, if formed according to the size and dimensions which I shall presently demonstrate, and used in the cases to which it is particularly appropriate; for although it must certainly be regarded as more capable of inflicting injury than the shorter kind,—inasmuch as it is introduced higher within the woman's person, and its extremities are actually received somewhat into the cavity of the uterus itself,—still its powers and capabilities are such as frequently to render it a substitute for the horrible operation of craniotomy. This value I have myself often experienced, for I have extracted many children alive by the agency of the long forceps, who had been doomed to death by other parties, and who must have been sacrificed, to preserve the mother, unless we had possessed this instrument.

Modern invention.—Although, however, the long forceps are so very useful and valuable, they were not generally adopted by practical men till about the commencement of the present century. Smellie, indeed, formed a pair much longer than his common instrument, but he considered them so dangerous in use, that he hesitated to recommend them, and did not even display them in his lectures. Smellie, however, we shall soon learn, applied their blades in a very different manner from that which is now usually practised, and which I myself follow.

Description.—The instrument which I have formed for my own use, and recommend for your choice, measures, from the extreme of the handle to the tip, twelve inches and three quarters, of which four inches and a quarter form the handles, and eight and a half the blades, being one inch and a half longer in the blade than the short forceps, and a quarter of an inch longer in the handles. The greatest width between the blades is about their centre, and measures two inches and seven-eighths; the points are an inch asunder. It weighs twelve ounces and a quarter. From the handles, two parallel straight shanks arise, of an inch and a half in length; and it is in the addition of this shank that the instrument differs principally from the curved forceps of Osborn, the curve of the blades springing, not from the handles, but from the extremity of the shank. The object of this addition is to prevent laceration of the perineum, in the use of the instrument; for if the long forceps of Smellie or Haighton be employed, in which the curve takes its origin from the handle itself, the mother's structures at the outlet of the pelvis must necessarily be pressed upon unequally by the commencement of the blades, before the head has descended low enough to distend them uniformly; and thus great danger of injury must ensue. In choosing an instrument, I would recommend this form and these dimensions to your consideration. You must be particular, also, that the internal surface of the blade is slightly convex, and the handles, when locked, should be allowed a considerable play upon each other laterally.

You will observe that this instrument possesses a slight lateral curve; and although I prefer a *straight short forceps*, I think the curve an useful addition to the *long* kind; but I employ both varieties in-

discriminately; and for my own use, I care little which of the two I take up. In the adaptation of these blades, there is not so much risk that we should apply them wrongly, as when we use the curved short pair; for as they are always to be introduced in reference to the *pelvis*, and not to the *particular position of the head*, we cannot well mistake which blade is to be passed uppermost.



The front and side views of the long forceps recommended.

State of the parts requisite to its introduction.—As I have already mentioned that the points of this instrument are introduced within the os uteri, you will be prepared for the observation, that it cannot be used if the mouth of the womb be undilated, especially if it be rigid: for although we might be able to insert each blade separately, still, when they are closed, the two handles cannot come into contact without the os uteri being pressed upon, bruised, and perhaps lacerated. It would be too strong a position, to lay down, as a general rule, that that organ should be *entirely* dilated before the instrument is had recourse to:—so much, indeed, that we shall not be able to feel the least portion of its disc; because there are a great many cases in which it is pinched between the head of the child and the pelvic bones,—is prevented from full dilatation, by being held prisoner, as it were, by this pressure,—and, consequently, in which a great portion of its substance, both anteriorly, towards the pubes, and posteriorly, towards the promontory of the sacrum, can be easily distinguished by the finger; while at the sides of the pelvis it is perfectly soft, flaccid, and distensible, and quite out of the reach of a common examination. The rule which I have been in the habit of following in my own practice is, that if one-third of the os uteri can be felt *continuously*, it is most likely in a state that will not admit the safe action of the instrument.

Cases in which serviceable.—The cases in which this instrument is so particularly serviceable are, where the head has partly engaged in the pelvic brim, having descended too low to be raised for the introduction of the hand into the uterus, and the performance of the operation of turning, while at the same time it has not entered the cavity sufficiently for us to feel an ear, and where delivery has become necessary either in consequence of hæmorrhage, convulsions, syncope, or any other accidental cause;—the os uteri being at the same time perfectly dilated, or most easily dilatable. But it is especially useful in those instances where the pelvis is contracted in its conjugate diameter, measuring between the pubes and the sacrum but a little more than three inches;—where the principal bulk of the head remains above the brim,—the uterine energies being strongly exerted, perhaps, but not powerful enough to squeeze the fœtal skull through the diminished aperture;—in which either exhaustion is approaching, or there exists a well-grounded fear that the uterus may injure itself by the violence of its own expulsive efforts. In such a case, provided the os uteri is entirely, or almost entirely dilated, with the vagina and perineum sufficiently distensible, the long forceps may be had recourse to, sometimes with decided advantage, and may render the horrifying operation entailing the destruction of the child's life unnecessary.

Mode of application.—The mode of applying the instrument differs exceedingly from that adopted when the short forceps are used. We do not apply it in relation to the situation of the child's head, but to the points of the pelvis. I have already mentioned that we adapt the short forceps decidedly in relation to the situation of the head, because we introduce each blade over an ear; but in using the longer instrument, we apply a blade within each ilium. The woman, then, lying on her left side, the one blade will be above, the other below; and whether the child's face be directed towards the right or the left side, one is placed over the forehead and the other over the occiput; or rather the blades are found to be applied somewhat diagonally, one reaching to the upper part of the orbit,—just to the super-ciliary ridge,—and the other exactly opposite to it, on one side of the occiput.

The first recommendation which Smellie gave was, that the blades, like those of the common instrument, should be adapted over the ears—one, then, lying behind the

pubes, and the other anterior to the sacral promontory; and in this recommendation he has been followed by Baudelocque, Burns, Dewees, and other practitioners of repute. But he advises, also, "if the operator finds the upper part of the sacrum jutting in, so much that the point of the forceps cannot pass it, let him try with his hand to turn the forehead a little backwards, so that one ear will be towards the groin, and the other towards the side of that prominence; consequently there will be more room for the blades to pass along the ears; but if the forehead should remain immoveable, or, though moved, return to its former place, let one blade be introduced *behind* one ear, and its fellow *before* the other."

I have read this quotation from Smellie, because it embodies the opinion and practice of his followers, and because it proves how intimately versed that great physician practically was with the difficulties sometimes met with in midwifery. The truth is, that in by far the greater number of cases under which the long forceps become necessary and useful, it is impossible to apply them over the ears, in consequence of this very impediment which Smellie has pointed out; the extremity of that blade which is introduced backwards impinges on the promontory of the sacrum, and neither force nor address will overcome the resistance offered to its progress upwards. When the pelvis is well formed, indeed, and the necessity for having recourse to the long forceps originates in any of the accidental causes just named, it may be very possible, and even easy, to apply them as Smellie, Baudelocque, Burns, and Dewees, advise; but under a contracted state of the conjugate diameter at the brim, it is very seldom that this is practicable.

To M. de Leurie, a French physician of some eminence, we are indebted for having first suggested the propriety of adapting the blades to the forehead and occiput; the mode at present generally followed. In a small work on the Cæsarean section, and the application of the forceps when the head is detained above the pelvic brim, published in 1779, he strongly advises this method of proceeding.

Two objections against this method have been urged by Baudelocque, and the merits of the new suggestion canvassed neither very fairly nor very temperately. He objects, that if the blades "be placed at the sides of the pelvis," they must be applied over the face and occiput, and that there is consequently great danger to the infant's features;—and again, he argues that if pressure be used in that direction, the bulk of the head cannot be diminished in the lateral diameter (where, indeed, such diminution is required), but its width must actually be augmented; because "the head being compressed one way, it must be lengthened in the other." With regard to the first of these observations, it is founded on an erroneous idea regarding the actual position and form of the fœtal head: he seems to have overlooked the very large proportion which the cranium, properly so called, bears to the face in the mature fœtus, and to have forgotten that under labour the chin is generally thrown forcibly upon the chest, the head therefore bent very much forwards, while the vertex becomes the most depending part: in consequence of which position, the forehead, and not the face, would principally bear the stress of pressure. Besides, even allowing that the blades were long enough to cover the face entirely, provided both are of the same dimensions, the point of that which is applied over the occiput would impinge on the neck of the child, and prevent the other passing up so high as to embrace the face; if otherwise, they could not be properly locked. Even in the most recent works, however, on obstetric science, we read of the injury likely to be done to the child's face by the forceps, used as I recommend them; and by some authors (as Campbell, in his late valuable publication) we are instructed that the one applied anteriorly should be softly padded, as a protection to the features. I have employed this instrument on many occasions, and I never, to my recollection, bruised a single feature. In general, the point of the instrument has not ascended further than the eye-brow, or (if the head were transversely placed, instead of diagonally) than the root of the nose.

The second objection is not more difficult to dispose of than the first, for it is also founded on a false assumption. There is no question that the fœtal head cannot be decreased in one diameter without being lengthened in another; but it does not necessarily follow that under the application of the long forceps, the increased capacity should be from one parietal bone to the opposite. It will be found, indeed, in practice, that the increase principally,—if not entirely,—takes place in the direction from the chin to the vertex; and that the cranium is moulded into a still longer or more conical form. Now as this particular change in the figure of the head does not in the least interfere with its passage through the pelvis, the only consideration which presses itself on the mind is whether it is likely to endanger the child's life; and I myself suspect, from observation, that the fœtus will not bear, with impunity, the same degree of pressure when the compressing powers are adapted over the forehead and occiput, as when ap-

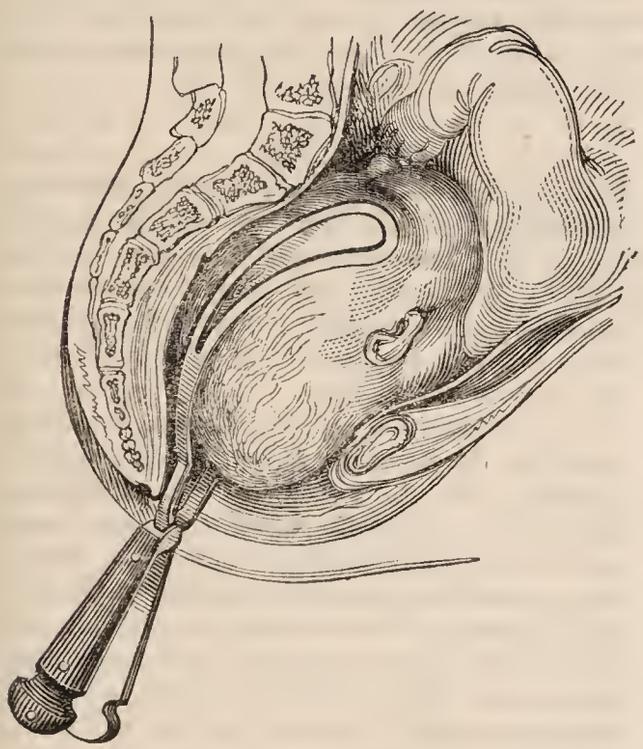
plied laterally:—but this requires further confirmation.

We will take, then, the case most frequently met with in illustration, and suppose our patient possesses a slightly contracted pelvis—the conjugate diameter measuring from three inches and a quarter to three inches and a half; we will presume that the parts are tolerably well relaxed; that the os uteri is dilated, being not discoverable at the sides, but pinched anteriorly between the head of the child and the pelvic bones, tender and slightly swollen; that a large portion of the head has come into the pelvis in an elongated conical shape, but that the base remains above the brim; that eighteen or twenty-four hours have elapsed since the rupture of the membranes; that the pains, which have been exceedingly strong, are beginning to flag, and that other symptoms of exhaustion are appearing:—or perhaps that they still continue powerful, but that there has supervened a violent local pain at one particular part of the uterus, constant and uninterrupted, so that we have reason to fear injury to its structure may occur, unless delivery be speedily accomplished: under such a state, if the pelvis contains three inches and a quarter, or three inches and a half, in the conjugate diameter, and is not diminished in space laterally, we are warranted in having recourse to the long forceps, and the operation must be conducted in the following manner.

The woman being placed on her left side, with the nates close to or over the edge of the bed,—the bladder being evacuated by the catheter, and the rectum being emptied also, if necessary,—we warm and grease both blades of the instrument, and pass two fingers of the left hand under the right ilium, as high as possible upon the child's head; then, taking lightly in our right hand that blade whose convex edge is towards our left side when the back of the instrument is next our person, we incline the handle up towards the pubes, and introduce the point backwards along the sacrum, keeping it in close proximity to the head, and insinuating it with the same semi-rotatory, wriggling movement, before directed. When the blade is so far passed up that the point approaches near to the sacral promontory, the handle must be gently drawn backwards towards the anus, and at the same time somewhat depressed; the point will then slip up under our fingers, and the whole blade will lie flat upon the fœtal skull;— or, to avoid the necessity of the blade making a circuit of any part of the pelvis, the handle may be depressed below the bed-furniture, and the point at once slipped up within the ilium, between our fingers and the head. This, however, requires that the nates of the patient should project considerably over the bed's edge—a posture difficult to obtain and to preserve—else the handle cannot be sufficiently lowered: and since the cavity of the sacrum is not occupied by the head, in the case under consideration, in the same manner as it is when the short forceps are applicable, there is not the same difficulty in making this partially circular sweep, nor the same chance of injury attending the change of position. When the first blade is properly adapted, it must be retained in its situation by our own little-finger and thumb, or by an assistant, and the second must be introduced within the left ilium, guided by the two first fingers of our left hand, exactly in the same manner as the first, until its locking part slips into that of the uppermost blade. If, when they are both fully introduced, it is observed that the blades are not perfect antagonists to each other, and consequently that they do not lock easily, no twisting must be used to make one groove fit into the other; there must be no wrenching or screwing round; for we must recollect that the points are actually received within the uterus, and that the neck and mouth of the womb may be seriously injured by our incautious efforts: we may, indeed, do infinitely more mischief even than if we employed the same force while adapting the short forceps;—but we must withdraw the second blade, and pass it up again in a different and more suitable direction. No effort at extraction must be made until the lock is firmly fixed. The head, then, being included within the two blades, the same cautions must be attended to as in the case of the short forceps. A finger must be carried quite around the lock, to ascertain that none of the mother's structures are trapped; moderate pressure is to be applied by our hands; and extraction must be begun.

We are sometimes told, that we must never think of having recourse to the long forceps until we have absolutely ascertained to which side of the pelvis the child's face inclines: but this information, however desirable it may be, it is not always possible to gain, even in cases perfectly adapted to the instrument: for the head is usually so high that an ear cannot be felt without thrusting the hand into the pelvis, and carrying one or more fingers into the uterine cavity—a measure which would produce both much pain and some danger: while, on the other hand, from the puffy state of the scalp, the fontanelle and limbs of the lambdoidal suture can scarcely ever be distinguished; so that,—although in the earlier part of the

process we might have been able accurately to satisfy ourselves as to the position of the head,—when called to a case requiring the assistance of the long forceps, that knowledge is obtained with great difficulty. And fortunately it is not absolutely necessary, for the success of our operation, that we should positively learn to which side the face is directed; for when the impediment at the brim is overcome, the head will generally of its own accord turn, with the face towards the hollow of the sacrum, without the use of any directing power on our part, so that we have only to *follow* the natural inclination, without attempting to *guide* it.



The long forceps, represented as applied when the principal bulk of the head is above the brim of the pelvis, the face being situated towards the right sacro-iliac synchondrosis; one blade is adapted over the right brow, and the other over the left side of the occiput.

In making extraction, the same pendulum-like motion must be used which avails us with the shorter instrument. It must not consist of a rapid succession of short, hasty jerks, nor a strenuous and forcible swing, but of a full, regular sweep from handle to handle, the lock being kept back towards the perineum as closely as is consistent with its safety, while slight traction is exerted downwards.

As soon as the head has passed the contracted brim, and has become fully lodged in the pelvic cavity, we usually find that the principal difficulty has vanished; and it then becomes a question in what way the labour should be terminated—whether we should finish it with the instruments, as first applied,—whether we should take them off, and leave the case to be completed by the natural powers,—or whether, on their removal, we should apply a short pair over the ears, and act according to the rules previously laid down:—and I think these questions can be satisfactorily answered; for each mode possesses its own advantages according to the peculiar features of every case. If, then, the uterus be acting but feebly, either from exhaustion or any other cause; while at the same time the outlet of the pelvis is of the ordinary capacity, and the vagina and external parts soft, flaccid, and distensible, we may terminate the labour at once by continuing to extract with these instruments, without changing their position, because there is little risk of injury; and if the remainder of the case were left to nature, much time might unprofitably glide by before its completion. If, on the contrary, the pains are still strong, while the external parts continue rigid, the outlet of the pelvis being well formed, it would be better to remove the instruments, and to trust the conclusion of the labour to nature's unaided efforts. But should the inferior aperture partake of the distortion, whether rigidity of the soft parts exist or not, we might then, on the withdrawal of the instrument, apply the short forceps over the ears, and cautiously and tenderly terminate the case through their agency: and this latter recommendation I give you, because of the probability that nature will not herself complete the delivery, and because the short forceps are much less likely to bruise or lacerate, when they are applied over the side of the head, than the long, which are adapted to the occiput and brow. I think that these recommendations will meet the exigencies of almost every case, and if followed, can scarcely lead to error.

Cautions.—There are some cautions necessary in the use of the long forceps, from which the shorter kind are exempt; they principally are—*First*, that we should not apply them in a case unfitted for their use. It has been already laid down as a general principle, more than once, that, unless the pelvis possess, in its conjugate diameter, three inches of clear available space, we cannot expect a full-grown well-ossified head to pass entire; and through such a diminished aperture we are not to hope that we should be able to extract it by the forceps. Burns, indeed, fixes the limit of the deformity which would indicate the use of the long forceps at that space. Davis, from the observations he has given us, would lead us to believe that rather more was generally required than that which Burns specifies; and I am inclined to think that unless the pelvis measures at least three inches and a quarter, we shall generally be foiled in our attempts at delivery; or, at least, be disappointed in our hope of extracting the child living.

Secondly, In introducing each blade, we must be particularly careful that the point slides within the os uteri, and does not run up between the vagina and the neck of the womb, lest we should bruise or lacerate that organ at its junction with the vagina; and especially, lest, in attempting to lock the blades, we should pinch its structure between their extremities and the child's head. This mischance cannot happen with the short forceps, because the os uteri must be entirely dilated before their application; and, when the longer pair is used, may be avoided by taking care that the point is constantly kept in contact with the fœtal cranium, guided by our finger previously inserted.

Thirdly, That we should not use too much exertion, or work with them for too long a period continuously. The longer the instrument, the greater leverage we possess; and it must be evident that each increase of leverage augments our power: we are not only, therefore, liable to use too much exertion, but we run the risk of making pressure upon structures less capable of sustaining it uninjured, than when we employ the short forceps. Unless, then, a decided advance be evident after a few minutes' well-directed efforts, we should desist from renewing our attempts; and we must judge of the progress we are making, by examining after each backward and forward movement of the instrument. We must most cautiously avoid using forcible means. *Force*, indeed, is a word which should be expunged from the vocabulary of obstetrical phrases.

Fourthly, We must be guarded in our promises of terminating the labour by the means we are about to employ; because it is impossible that we can measure the head accurately while its base remains above the brim, and it is equally impossible that we should be able to form an opinion of the degree of ossification it has acquired, and of its compressibility. In all these points we may be deceived, although we may have made ourselves acquainted with the capacity of the pelvis to the greatest nicety; and while such chances of deception exist, we must be most cautious not to add disappointment to suffering. I have myself, in many instances, been foiled in attempting to extract the head entire through a narrowed aperture, and been obliged, eventually, to have recourse to the perforator. I always feel more satisfied, however, in lessening the head after having made these attempts, because I have good reason to think that nature would seldom be able to expel a child through a pelvis of such small dimensions as would not admit its passage by the aid of the long forceps;—provided, in other respects, the case was fitted for the employment of that instrument. I would advise you, then, before proceeding to operate, not to make a promise of delivery, but merely to state that you are about to do something which will materially relieve the patient, and that, *perhaps*, you may at once terminate the labour.

After all I have said on this subject, I must not close my remarks without stating my conviction that the instrument, although very powerful and valuable, is at the same time very dangerous in its use; that it should not be taken in hand except by those who have acquired some proficiency in operative midwifery; and that it is to be had recourse to, more as an experimental measure for superseding the necessity of destroying the child, than as one of the common resources of our art.

CRANIOTOMY.

Of all instrumental operations in obstetric surgery, the perforation of the skull, and extraction of the mutilated fœtus, is the easiest; and much do I fear, that to the facility with which this operation can be accomplished, have been sacrificed the lives of many children.

Various are the terms under which it has been described—embryulcia and embryousia, which have been by different authorities derived from εμβρυον and ελκω, *traho* — or ελαω, *abigo* — or θλαω, *frango*; embryotomy, from εμβρυον, *fœtus*; cephalotomy, from κεφαλη, *caput*; and craniotomy, from κρανιον, *calvaria*, and τεμνω, *seco*. The latter term is, I think, in most common use; and is the one which I shall adopt.

It may, perhaps, be desirable in surgery, whenever necessity compels us to perform an apparently cruel operation, that the horror which the simple and bare mention of that act would inspire might be smothered and absorbed, as it were, by the sonorous and classical title which it bears; but by whatever name it is called,—under whatever high-sounding appellation it is disguised,—we cannot alter or conceal the fact that the operation consists in plunging an iron instrument into the centre of the skull of a human being, probably at that moment living, and extracting it into the world mutilated.

Some, indeed, horrified at this arbitrary destruction of life, have laudably contended that the proceeding is not justified unless the fœtus be dead: they argue with apparent truth, that human life is held at the will of one Supreme Being alone,—and that, unless forfeited to the laws, to no human hand is delegated the power of destroying it. Strong and valid would those objections be, if once the operation were performed

wantonly, or without grave and deep consideration; but it is never had recourse to, except for the purpose of saving life or preventing future misery. Did the mother perish, the fœtus within her must perish likewise. But in British midwifery we consider the mother's life as paramount,—nay, more, we think ourselves warranted in sacrificing the infant, if that be the only way by which we can preserve her person from those dreadful lesions of sloughing and laceration, which, if they took place, must render her future existence a scene of uninterrupted wretchedness.

Nor are there wanting arguments sufficiently strong and numerous to justify our practice. These are, perhaps, more the province of the philanthropist or the medico-logician, than the practical surgeon: but I may remark, that the woman is bound to the world by many social, moral, and religious ties; she has shared the enjoyments, as well as the cares of life; she has her feelings and affections, her fears and her hopes; she is dependent on others, and others are dependent on her; when she dies, there is left a blank, which, to some surviving, never can be filled. Not so, however, with the unborn infant. Although, in dying, some personal pain may be experienced, yet the agony of mind it cannot suffer: it has no affections, no dependents; its existence centres almost entirely in itself;—except to its nearest relatives, then, its death cannot be greatly felt. And, although we cannot estimate life relatively, as we can any other possession, (since life to the possessor, however burdensome it may be, is still life), yet we are surely justified, in a political point of view, if not in a moral one, in preferring the safety of the strong to the weak, the healthy to the diseased, or the mother of a family to the unborn infant, provided one or other in all probability must be sacrificed. From these considerations we prefer, whenever we have a choice, the mother's safety to the infant's life: yet I would entreat of you, in the strongest terms, to think long and much—carefully, deliberately, and dispassionately—before you have recourse to so dreadful an expedient as is offered by craniotomy.

Many of the continental, and some of the American practitioners even now contend that craniotomy should never be performed unless there are positive indications present of the child's death. Thus M. Duges, one of the latest of the French authors, when speaking of the crotchet, says, "Nous avons dit, qu'on ne pouvoit l'appliquer qu'après la mort certaine du fœtus; — ne peut être appliqué que sur un enfant indubitablement mort." This opinion perfectly coincides with the ancient barbarous feeling expressed in the answer of the doctors of the Sorbonne, in 1688, to the question, whether it was lawful to destroy the child for the purpose of saving the mother? "Nous sommes d'avis que si l'on ne peut tirer l'enfant sans le tuer, l'on ne peut sans péché mortel le tirer:" and they quote as their authority the maxim of St. Ambrose—"Si alteri subveniri non potest nisi alter lœdatur, commodius est neutrum juvare." In 1755, however, the same tribunal ruled that it was lawful to baptise children in utero by means of injection; thus removing one of the objections to craniotomy in force in the Catholic countries; viz. that the child would necessarily perish eternally, unless that rite had been administered; but leaving the main question undecided.

Instruments employed.—Few and simple are the means required for this dire, this terrible, this little less than murderous operation; the perforator or the scissors, the blunt hook of different sizes, the crotchet or the sharp hook, the craniotomy forceps, and perhaps a pair of bone forceps, are all the implements necessary. Our instruments, therefore, may be divided into two kinds,—the one to perforate the skull, and the other to extract, after the necessary diminution in bulk is effected.

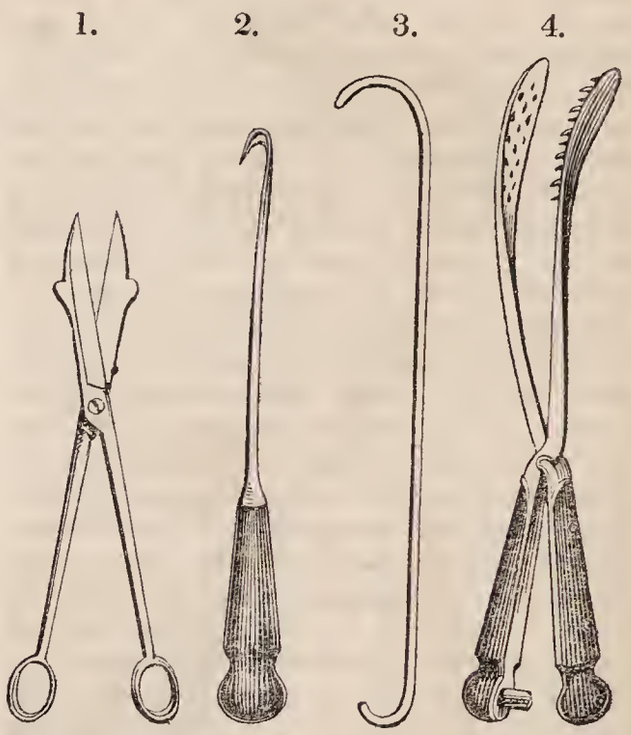
Perforator. — Denman's perforator is about twelve inches in length; it has a sharp point, and two sharp external edges, but the internal surface is blunt; it opens with a joint like a pair of scissors, and has a rest about an inch from the extremity, to prevent its penetrating beyond a certain distance into the skull. I generally, however, carry in my case a pair of Smellie's scissors, nearly of the same dimensions, having a cutting edge both externally and internally, because they are useful also to perforate the chest in cases of impacted transverse presentations, as well as for the operation of craniotomy. In choosing a perforator, we should be particular about its length; that it is made sufficiently strong not to bend under use; that the blades should not fit close at their junction, lest they should inclose a fold of the vagina when shut; that there should be no sharp edges about the joint, but that the limbs should be in every part smoothed round, and that the point should be slightly curved. Dr. Wallace Johnson added this curve to the extremity of the perforator: it does not increase the value of the instrument, as far as making the incision is concerned,—its only object is the preservation of our own fingers from injury; for—as the instrument is guided up to its destination along the groove between two of our fingers—if the point were quite straight we should run the risk of lacerating them; and as this form by no means

detracts from its power, it is as well to adopt it.

Crotchet.—The crotchet, or sharp hook, is intended for the purpose of extracting the child's head after it has been opened, and is formed in the shape of a beak. The point should not be very sharp, lest it tear too easily through the bones, and injure the woman's structures, or run into our own fingers; nor too blunt, lest it should not retain its hold. Simple as this instrument appears, it is one of the most difficult in midwifery to procure good; for the least variation in the sharpness of its extremity makes considerable difference in its value.

Blunt hook. — It is desirable that we should be furnished with two or three blunt hooks of different sizes: they are also used as extractive powers, and may sometimes supersede the necessity of the crotchet, or be employed instead of that instrument, if the bones of the skull are too much loosened by putrefaction from their membranous attachments to afford a purchase to the crotchet: in such a case, the extremity of the hook may perhaps be passed into the foramen magnum, or behind an orbit.

Craniotomy forceps.—Another extracting instrument, much in use in the present day, is the craniotomy forceps. This is formed of two separate blades, which are joined, after their application, by a hinge similar to that of the forceps, one of which passes within the skull, and, being furnished with teeth, perforates the bones; while the other is introduced externally to the cranium, and possesses indentations or cavities, into which the teeth of the first blade are received. When properly fixed, the extremities of the handles are to be bound firmly together by a ligature, and steady traction applied: if the bones are very strong, and the diminution of space but small, this instrument is most powerful and highly useful; but if the bones are weak, and easily give way, I cannot help considering it a more dangerous instrument even than the common crotchet; for, while the crotchet is fixed within the skull, the finger is applied exactly opposite to it externally; and if the point perforates the bone, or slips away from its attachment, it cannot injure the os uteri or vagina, as it must necessarily come against our finger. But when the craniotomy forceps are employed, we have no power of guarding the patient from injury in the same manner: their teeth are certainly well sheathed by the antagonist blade; but, in consequence of the perforations which they make in the bone, its structure is so weakened that it easily gives way; and the instrument loses its hold, and passes suddenly externally, bringing with it a portion of the skull, and perhaps some of the scalp also. This accident would indeed be of little consequence,—no other inconvenience being suffered except the loss of the purchase,—if the angles of the broken bone were always either covered by the scalp, or sheathed by the instrument itself: but this I have found by no means the case; for the irregular, jagged edges of the torn bone project beyond the margin of the blades, and—in its rapid passage through the vagina, as the instrument breaks from its hold—they are very likely to tear that organ, as well as the external parts; nor is it possible to protect these structures from injury, as it is when the crotchet is used. For these reasons I prefer employing the latter instrument, although I always carry with me the craniotomy forceps as well.



1. Perforating scissors.
2. Crotchet.
3. Blunt hook, of two sizes. In the specimen represented there is a hook at each end; this is only to prevent the necessity of multiplying the number of instruments. It is better, however, that each should be fixed into a separate wooden handle.
4. Craniotomy forceps.

SINGULAR CASE

OF A

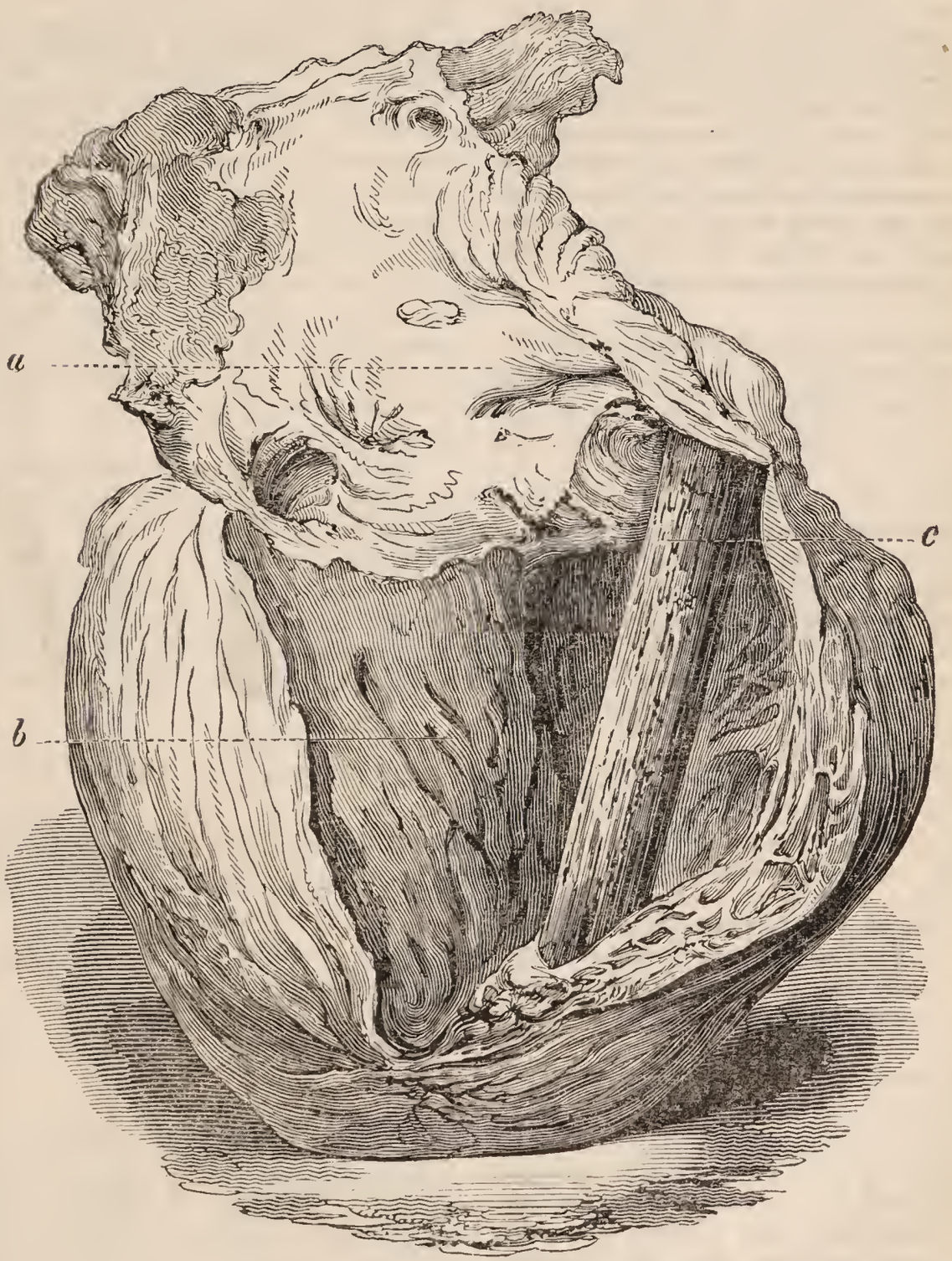
FOREIGN BODY FOUND IN THE HEART OF A BOY.

By Thomas Davis,
Surgeon, Upton-upon-Severn.

On Saturday evening, January the 19th, 1833, I was summoned to attend

William Mills, aged 10, living at Boughton, two miles from Upton. When I arrived, his parents informed me that their son had shot himself with a gun made out of the handle of a telescope toasting-fork. To form the breech of the gun, he had driven a plug of wood, about three inches in length, into the handle of the fork. The touch-hole of the gun was made after the charge of powder had been deposited in the hollow part of the handle; the consequence was, that when the gunpowder exploded, it forced the artificial breech, or piece of stick, from the barrel part of the gun, with such violence that it entered the thorax of the boy on the right side, between the third and fourth ribs, and disappeared. Immediately after the accident the boy walked home, a distance of about forty yards.

By the time I saw him, he had lost a considerable quantity of blood, and appeared very faint. When I turned him on his right side, a stream of venous blood issued from the orifice, through which the stick entered the thorax. Several hours elapsed before any degree of re-action took place. He complained of no pain.



a, the right auricle.—*b*, the right ventricle.—*c*, the stick, the one end of it embedded in the columnæ carneæ, near the apex of the heart, the other end resting upon the auriculo-ventricular valve, and rupturing it.

For the first ten days or a fortnight after the accident, he appeared to be recovering, and once, during that time, walked into his garden and back, a distance of about eighty yards; and whilst there he amused himself with his flowers, and even stirred the mould. He always said he was well, and was often cheerful, and even merry. There was no peculiar expression of countenance, excepting that his eyes were rather too bright.

After the first fortnight he visibly emaciated, and had frequent rigors, which were always followed by faintness. The pulse was very quick. There was no cough nor spitting of blood. The secretions were healthy. He had no pain throughout his illness.

He died on the evening of the 25th of February, exactly five weeks and two days after the accident occurred. At the examination of the body there were present, the Hon. W. H. Coventry, Mr. Walker, the father of the boy, Mr. Shcward, surgeon, and myself.

On opening the thorax, a small cicatrix was visible between the cartilages of the third and fourth ribs, on the right side, about half an inch from the sternum.

There was no effusion of blood, or of serum, into the sacs of the pleura.

The lungs appeared quite healthy, excepting that there was a small tubercle in the right lung, and at its root, near to the pulmonary artery, a small blue mark in the cellular tissue, corresponding in size with the cicatrix on the parietes of the chest.

Heart and pericardium.—When the pericardium was divided, about half an ounce of serum was found in it.

The heart, externally, appeared healthy.

When an incision was made into the heart, so as to expose the right auricle and ventricle, we were astonished to find, lodged in that ventricle, the stick which the boy had used as the breech of the gun, the one end of it pressing against the extreme part of the ventricle, near the apex of the heart, and forcing itself between the *columnæ carneæ* and the internal surface of the heart; the other end resting upon the auriculo-ventricular valve, and tearing part of its delicate structure, and being itself encrusted with a thick coagulum, as large as a walnut.

We searched, in vain, for any wound, either in the heart itself, or in the pericardium, by which the stick could have found its way into the ventricle.

Observations.—This case strikes me as one of the most interesting on record. In the first place, that this child should have survived such an accident, as the lodgement of a stick, three inches in length, in the right ventricle, and have been afterwards equal to so much muscular exertion as he was, appears wonderful, especially if we consider the mechanical difficulty which the heart had thereby to encounter in carrying on the circulation of the blood.

In the next place, it appears somewhat difficult to point out how the stick found its way into the right ventricle of the heart. There was no wound, nor remnant of a wound, either in the pericardium, or in the muscular structure of the heart. The stick seems to have entered the mediastinum, without wounding the anterior part of the right lung, but to have wounded the posterior portion of the lung, near its root. What became of the stick afterwards is uncertain, excepting that we know it found its way into the heart.

I am inclined, myself, to think, that the stick, after wounding the lung, passed into the vena cava, and was carried by the stream of blood, first into the right auricle, and then into the right ventricle, where it became fixed, in the manner before specified, and as is shewn in the accompanying plate *.

COMPARATIVE PROBABILITY OF LIFE AMONG THE MALE POPULATION OF ENGLAND AND WALES, AND THE MEDICAL PRACTITIONERS OF GERMANY AND NORTH AMERICA.

To the Editor of the Medical Gazette.

Sir,

Though I was aware, in common with all who have the least acquaintance with the probabilities of life among the several orders of society, that the members of the medical profession are rather short-lived, in comparison with those of the

* Transactions of Provincial Medical and Surgical Association.

other learned professions, and people in easy and healthy circumstances, still the exposition of Professor Casper, as given in the Medical Gazette of the 10th instant, a little surprised me, and naturally drew my attention to what might in other countries be ascertained.

With this view, I took the trouble of calculating, from the *Tables of Mortality in England and Wales*, published in the *Official Returns of Population*, &c. the probability of life in this kingdom from 1,996,195 male persons buried, at every age from birth to 100 years, during a space of 18 years, from 1813 to 1830, and which result I have stated in the second column of the following table.

TABLE OF THE COMPARATIVE PROBABILITY OF LIFE AMONG THE MALE POPULATION OF ENGLAND AND WALES, AND OF MEDICAL PRACTITIONERS IN GERMANY AND NORTH AMERICA.

1. Age.	2. Probable Male Life in England and Wales.	3. Probable Life of Medical Men in Germany.	4. Probable Life of Medical Men in North America.	1. Age.	2. Probable Male Life in England and Wales.	3. Probable Life of Medical Men in Germany.	4. Probable Life of Medical Men in North America.
21	38·7	..	..	61	13·	9·7	14·9
22	38·13	...	..	62	12·5	9·	14·5
23	37·6	35·5	..	63	11·9	8·8	14·
24	36·8	34·4	..	64	11·3	8·5	13·4
25	36·3	35·4	..	65	10·6	8.	12·6
26	35·85	33·	..	66	10·1	7·5	11·7
27	35·25	32·	38·	67	9·6	7·2	11·2
28	34·75	31·4	..	68	8·9	6·5	10·3
29	34·	30·1	..	69	8·5	6·	9·5
30	33·4	29·8	36·	70	8·	5·5	8·9
31	32·8	28·6	35·	71	7·6	5·4	9·5
32	32·1	28·	34·1	72	6·9	5·	8·7
33	31·5	27·6	33·4	73	6·6	5·	8·5
34	30·75	26·8	32·5	74	6·3	4·7	7·75
35	30·1	26·	31·6	75	5·9	4·6	7·25
36	29·5	25·	30·7	76	5·6	4·4	6·6
37	28·8	24·7	29·9	77	5·12	3·9	5·8
38	27·9	24·	29·5	78	5·	3·3	5·75
39	27·4	23·5	29·	79	4·75	3·	4·8
40	26·7	22·7	28·75	80	4·3	3·	4·7
41	26·	22·	28·12	81	4·	4·	4·16
42	25·5	21·3	27·	82	3·6	4·	3·5
43	24·75	20·3	26·62	83	3·1	4·	4·25
44	24·2	19·7	25·75	84	2·95	4·	3·75
45	23·5	19·	25·12	85	2·8	4·	3·5
46	22·75	18·2	24·5	86	2·75	3·5	3·16
47	22·1	17·3	23·75	87	2·63	2·6	2·33
48	21·5	16·7	23·5	88	2·4	2·	1·7
49	20·9	16·	23·	89	2·32	1·5	1·
50	20·1	15·4	22·08	90	2·3	0·5	1·5
51	19·5	15·	21·37	91	2·57	0·0	1·3
52	18·75	14·2	20·5	92	2·4	..	1·
53	18·	13·5	19·75	93	2·35	..	·5
54	17·6	12·8	19·	94	2·23	..	·0
55	16·8	12·6	18·	95	2·04	..	..
56	16·2	12·4	17·5	96	2·	..	..
57	15·5	11·9	17·37	97	1·86	..	..
58	15·	11·	16·9	98	1·7	..	..
59	14·3	10·6	16·8	99	1·6	..	..
60	13·6	10·3	15·25	100	1·23	..	..

Not having at hand any extended and continued necrology of medical men in this kingdom, I availed myself of Dr. Thacher's "American Medical Biography," published in 1828, which contains the lives and ages of 150 medical practitioners, who had died in the United States during the last and the elapsed part of the present century; and the calculation deduced from this source I have stated in the fourth column.

The third column contains an exact transcript of Prof. Casper's probability of life, as taken from the Medical Gazette of the 10th instant.

The probable duration of life, in columns 2d and 4th, is calculated from Halley's method, the same which Prof. Casper has used, and which leads to as near an approximation of the average duration as can easily be deduced.

From the above Table, it is seen how much the probable duration of life, among the whole male population of the kingdom, exceeds that of the medical practioners in Germany, which is a result that would not at first be thought to be the case,—seeing that the duration of life is short among the great proportion of the working and manufacturing people of this kingdom, and which must counterbalance, to a great degree, the more lengthened lives of the agricultural classes, and those in easy circumstances. It is allowed that the members of the medical profession are peculiarly exposed to many causes deteriorating the vital energies of the body, and abridging life; still, it is presumed, there must be something more particularly affecting the physical existence of the practitioners in Germany, to create such a low probability of life; especially since Professor Casper has taken his calculations from the age of 23—a period when the severer and more trying studies of the generality of pupils are either become familiar or completed. Perhaps the low remuneration for their professional services, and the consequent want of many physical comforts which practitioners in this country enjoy, even when busily employed, may serve to solve the problem.

The probability of life among the medical practitioners in America, however, shows a rate above that of the male population of the kingdom, though not to the extent to which that of the profession in Germany sinks below it, being, in general, only about one-half the difference,—a result that might, considering all relative circumstances, be *à priori* assumed. It is true that the biography of Dr. Thacher includes all those who were eminent in his list, which notice they perhaps would not have obtained, provided length of years had not confirmed the nascent talents of youth, and made useful and honourable the moderate and otherwise unnoticeable acquirements of the individual. As it is, the different ages to which 150 practitioners have lived, must give a fair average of the duration of life among the members of the profession in that country, after they have arrived at the age of 30 years.

I had intended to have made this communication more full on some other statistical subjects, but shall conclude, in alluding to the editorial remarks of the 10th instant, on the salubrity of London, and this kingdom in general, by stating, that the rate of mortality in this town, one of the healthiest in Lancashire, and with a population of 41,195 in 1831, was, in that year, 1 in 36·5, when a good deal of epidemic dysentery and fever prevailed; but it falls down to 1 in 40, on the general average of late years.—I remain, &c.

J. Black, M.D.

Bolton, May 24, 1834.

PATHOLOGY OF THE NERVOUS SYSTEM.

PARTIAL PARALYSIS OF THE FACE.

To the Editor of the Medical Gazette.

Sir,

Some doubts having recently been expressed as to the accuracy of Sir Charles Bell's opinion, relative to the portio-dura of the seventh pair of nerves, I have thought the following case, confirmatory of the doctrine advanced by that able anatomist, might not be unacceptable to your readers.

Mrs. C., a person of hale appearance, and about 40 years of age, applied to me for advice. I found the integuments of the left side of her face puffed and swollen; and behind the root of the left ear, a hard, deep-seated, glandular tumor, painful on pressure. The left angle of the mouth was drawn to the opposite side, which deformity was much increased by the act of smiling or

speaking. The left eye had a staring appearance, remained wide open during sleep, and could not be closed by any effort. The left nostril also was motionless, as well as the skin covering the left portion of the forehead, and the left temple. In short, all the muscles of the left side of the face had lost their subserviency to the will; but it is highly worthy of remark, that, in this complete state of paralysis, the sensibility of the part should remain wholly unimpaired. The slightest touch was distinctly perceptible, and all the other senses perfect. My patient has always enjoyed good health, and neither previously to her present malady, nor during its continuance, has she ever experienced the slightest headache, giddiness, or other symptom of cerebral derangement.

On inquiring into the history of this case, I learned, that about three weeks ago my patient had been exposed, when at her occupation of washing, to a draught of cold air, which, making its way through a broken pane of the window, came in contact with the left side of her neck and throat. In the evening she found a stiffness at that part, which, on the following morning, had increased, and was then accompanied by some degree of swelling. Resuming her occupation, however, she was again exposed, during the greater part of this day also, to the consequences of the broken window. At night, both the pain and swelling had increased, and had extended upwards to the root of the left ear (which part was extremely painful to the touch), and over the corresponding side of the face. Then it was that Mrs. C. first perceived she had lost all power of motion in the left side of her face; and, becoming alarmed at the circumstance, she sought medical assistance. Phlebotomy and purgatives were unsparingly administered, under the notion of an apoplectic seizure. By these means the general inflammatory swelling of the face was much subdued, without, however, making any favourable impression on the paralytic state, or materially lessening the tumor behind the ear. Distressed and inconvenienced, she determined on asking farther advice, and I was, about three weeks afterwards, consulted. I leeched the part freely, afterwards blistered it, then rubbed in mercurial ointment, and gave, internally, mercurials with diaphoretics. At the expiration of three months I had the gratification to perceive the tumor nearly gone, and the paralysed muscles almost restored to their wonted functions.

Now the observations I would make on the above case are—

1. That the paralysis was not the result of any cerebral affection, there never having been the slightest evidence or symptom of such a state.

2. That the hard, deep-seated tumor, below the root of the ear, was the real cause; for with that tumor the paralysis commenced, and gradually disappeared as it subsided.

3. That the immediate cause was compression of the portio dura of the seventh pair of nerves; for, from the size and inward direction of the tumor in its immediate neighbourhood, that nerve could scarcely escape pressure after its escape from the encephalon, and previously to its distribution on the face.

4. That the portio dura being thus proved to be the motor nerve of the face, the sensibility of that part must be derived from the other nerves sent to it—viz. the branches of the fifth pair.

5. That the sensibility and muscular power of a part are quite independent of each other; and, consequently, that the nervous influences, by virtue of which these faculties subsist, are derived from different sources, and are conveyed through different channels.

These inferences, it is obvious, are in strict conformity with the opinions so ably advocated by Sir Charles Bell, who has shewn, on convincing evidence, that not the face only, but that every other part of the body, is indebted, for its faculties of sensation and motion, to nerves perfectly distinct from each other, both in their nature and their origin. Should, therefore, the above case be regarded as an unnecessary accumulation of proof, I must rest my apology for making it public on the anxiety I feel to place so important a truth beyond the reach of doubt or difficulty, and which, though apparently simple, required nothing less than the genius of a consummate anatomist to disentangle from the perplexities in which it lay involved. Valuable as that truth assuredly is, in its practical application, and solving as it does phenomena previously considered inexplicable, we need look for no further incentives to pursue the path of in-

quiry thus happily struck out. But never let it be forgotten, that, in the whole compass of physiological research, there is no subject of greater difficulty or complexity than the functions of the brain and nerves. Momentarily in danger of being led astray by the imagination, we ought ever to be on our guard against its seductive influence, and resolve to encounter the laborious, though not ignoble, task of collecting facts that will bear the test of the most unsparing scrutiny. From data such as these, and from such alone, may we hope to rise, by a series of inductions, to some further knowledge of the specific uses of the various parts of that system, or bodily organization, with which the thinking and percipient principle is mysteriously united, and through whose medium it develops its operations, sustains animal life, and holds intercourse with the external world.—I am, sir,

Your obliged servant,
F. Bailey, M.D.

Reading, May 26, 1834.

SKETCHES OF THE PRESENT STATE AND FEELINGS OF THE MEDICAL PROFESSION IN THE PROVINCES;

And more especially in Liverpool and its Vicinity.

By James Collins, M.D.

The more I contemplate the abuses and anomalies that disgrace and impoverish the practice of medicine, the more I am convinced how hopeless it is to suppose that acts of parliament can alone rectify them: they may check, as I trust they will, the wholesale license that exists at present for quackery and empiricism; they may prevent chemists and druggists from usurping, as they do, the rights and privileges of medical men; they may raise the standard of medical education, and establish some uniform system in our colleges and universities for degrees in medicine and surgery, and thereby diminish the facility of inundating a profession already overstocked with men of comparatively low attainments; they may do all this, and much more, but they cannot put an end to our low and mean charges detailed in my former communications—to our system of dunning, bills, clubs, club-doctors—to our habits of farming at so much a year, for what would scarcely remunerate a porter, the medical attendance of the families of our merchants and tradesmen—of the clerks, labourers, shop-boys, male and female attendants, from the porter at the door to the lady in the drawing-room—of our wholesale and retail establishments; nor to our attending, for a ticket to the play, the families of the directors and managers of our theatres. No; the remedy here is in our own hands, and consists in discountenancing these abuses, from whatever quarter they proceed, or with whatever pomp or pretension they may be accompanied. In doing so, we shall be sustaining the respectability of the profession, and multiplying the chances and sources of practice.

In this paper I wish to convey some notions of the state and management of our public institutions for the relief of the sick, and of their effects on the character and emoluments of the profession. This I intended to have done long since, but circumstances prevented me; nor should I now attempt it, if others, more conversant with the subject, seemed willing or disposed to take it up. The country is beginning to awake to the bad effects of indiscriminate charity; the poor-law commissioners have sounded the alarm, and astounded the public by their picture of its demoralizing results; they have shewn in their report, what every one of common observation and experience must have long since seen, that too great facilities of relief induce persons to avail themselves of it for whom it was never intended—that they destroy their sense of independence, and their feelings of delicacy, self-aid, and respect; in short, that they do more harm than good. If these views and principles be correct, they are equally applicable to our medical charities; for they afford the greatest facilities for gratuitous advice and medicine, and thereby rob the profession of their legitimate practice, by relieving those that ought to pay for it, without any remuneration to those whose skill and time they dispose of for this purpose; for there is scarcely a person employed in them, except the medical men, who is not paid for what he does: the porters, nurses, parsons, attorneys, collectors, matrons, and all the rest of the working agents, do

nothing for nothing; — why, then, should the physicians and surgeons, whose time and services are the most valuable, without which the machinery could not go on, nor the charity be of any service? Is this right, or ought it to continue, unless medical men do not intend to live by their profession, and wish to become the instruments of their own ruin?

But the evil does not stop here: these charities destroy the profession, by taking from it the materials by which it ought to live—in admitting as pauper patients those who could and ought to pay for attendance. Here, it is as notorious as the noon-day sun, that hundreds come who ought not—that persons in easy, and what, comparatively, may be called affluent circumstances, apply to these, without any indication that in doing so they deem themselves lowered in their own or the public estimation. I have seen men and women go there, with the dress and appearance of the better and paying classes, drive up to the doors, and keep the cars or carriages they hire to bring them waiting until they have done, and all this without any evidence of shame or remorse, or consciousness that they were doing wrong in applying for relief to a medical charity: others, again, with a little more tact and decency, dress, on these occasions, a little more shabbily, and often give, when they apply, false or fictitious names and addresses. It may be asked, why do medical men encourage or receive such patients? It is easier to ask the question than to answer it: many of them are deceived, others are not, and do it from calculations of one sort or another, that ultimately something may be got by them or their connexions, and from a feeling that the mere poor can serve no one. A physician that had been many years attached to one of our dispensaries, told me, in illustration of these practices, that one day, whilst passing his dispensary patients, one of them asked him if he would advise her to ride occasionally on horseback: astonished at the question, he inquired if she could afford it. "Oh, yes," was the reply; "my father is Mr. ——, the riding master"—a gentleman, then in the receipt, it was supposed, of five or six hundred a year from his business; yet his daughter, a nice accomplished young lady, attended as a dispensary patient. Another physician attached to the same institution, who has long set his face against such patients, took one day one of our principal merchants, a great supporter of these establishments, to the house of a patient then attending them, in order to show him an instance of these malpractices, and to convince him by ocular demonstration that he ought not to be admitted. They found this patient's house rented at fifty pounds a year, besides the local and government taxes, well furnished and carpeted, with all the other exterior marks of ease and comfort; yet he was a dispensary patient! But why multiply examples? They are of every-day occurrence, and neither excite surprise or attention, though means are nominally in force to detect and prevent them.

Thus we take the ground from under our own feet, and become in a great degree the instruments of our destruction. We give our services for nothing to charities for the benefit of those that ought to pay for them, and we all complain of being overstocked. How few can now live, much less attain wealth and independence, by their practice!—yet we refuse to open our eyes to the truth, that we ourselves are in a great degree the cause of all this, by tending to swell the tide of pauperism that has been long setting in upon us from these and similar causes. The whole system is rotten, and must be changed. Medical men ought and must be paid, and the doors of these institutions closed against all but paupers. Persons who can pay, and will not pay, ought to be made to pay, or at least have no business there. What would become of law, divinity, or any other trade or calling, if people could take its services for nothing?—if institutions, as in medicine, were opened to the public for their gratuitous aid? Do you think, Mr. Editor, that they would continue long lucrative or respectable, or that the public would not take advantage of them by taking for nothing what they are now obliged to pay for, and soon reduce them to poverty and obscurity? But there is no fear of this. The merchant, the lawyer, the parson, will never give their time or trouble for nothing, in the way of business or professional services: would that medical men would do so likewise, when they could and ought to be paid. See how others act, with all their religion and holy zeal: the chaplain of our infirmary

is paid; the chaplain to our jail is paid, and so is the chaplain to our workhouse; though they tell us that their calling is not of this world; and though these offices are mere sinecures, and have been created to swell patronage and the loaves and fishes, without any regard to the interests of the charity or the feelings of the inmates, as the greater part of the latter do not belong to their church, and never trouble them for their ministry.

Now let us see how the attorneys act: our vestry clerk, an attorney, gets a thousand a year, independent of his regular business, for a few hours' attendance in the week on the select vestry. His salary is three hundred a year; and the rest, the profits of his law bills, that amount to near two thousand a year: every body knows that such bills are, like apothecaries', cent. per cent. Our town-clerk, an attorney, swore before the commissioners that his place was worth three or four thousand a year, independent of all its indirect and direct patronage. The clerk to our magistrates, an attorney, makes, by the perquisites and salary of his office, one thousand a year; at least this was the sworn statement before the commissioners. The clerk to the commissioners of the Watch and Scavengers, an attorney; the clerk to the Paving Board, an attorney; the clerk to the Dock Trustees, an attorney, &c., make each by their respective offices sums varying from eight hundred to two or three thousand a year. Many of them are very religious men,—good and God-fearing Christians; but do not allow religion or charity to stand in the way of business, and get paid for what they do: not so the medical men; they do not frequently get credit either for charity or religion; and yet no men in the community render such disinterested services to both. For them there are no such sinecures or places, with good salaries and no trouble; there are no loaves and fishes served in this way for them at the public expense; for, with the exception of the apothecaries or house-surgeons of our public institutions, no medical man is paid for his services here, or receives one shilling for his time or trouble, but is very often treated with ingratitude and disrespect, or is cast off in his old age without ceremony by the very charities he served all his life; and he has his reward for making himself so cheap, for being willing to work for nothing, and to risk swamping his profession, for the mere contingency that in doing so he may ultimately attain notoriety and practice by being attached to a public institution.

But let us come a little closer to this gratuitous system, and see how it works for the patients and the subscribers, and we will find that it is as injurious to both as to the profession, and therefore ought, in the name of justice and humanity, to be done away with. What is not paid for, is seldom well or regularly done: without a motive, there can be no great exertion; and what greater incentive can there be, taking human nature as it is, than remuneration for our trouble. Medical men are appointed to our public charities, without fee or reward, to take charge of the patients. Is it likely, in these circumstances, that they could or would? and is it not notorious that they do not? They get what they covet, the name of attending them; but the greater part of the work is done by others.

There are two classes of medical men attached to our dispensaries, the honorary and stipendiary officers: the first are supported by the public and the subscribers, and were elected to be the physicians and surgeons of the patients; but in fact they are not: for until very lately the custom here was to leave, in a great degree, the house-patients to the apothecaries and their apprentices—lads without any previous or professional education. Whenever they supposed the case dangerous, or wished to shift the responsibility from their own shoulders, they sent for the physician or surgeon, or, as they termed him, the honorary officer, and, like the huntsman, he was often only in at the death; the lads or apothecary keeping until then the patient, particularly if it were an interesting case, to try their own hands, or to satisfy their own skill and curiosity. This was the common practice for nearly half a century—almost from the very foundation of our dispensaries; until at last public attention was directed to this cruel and unnatural perversion of the nature and object of these charities. The newspapers teemed with letters, addresses, and appeals, of one sort or another, to the subscribers: things became too hot from agitation to be handled and con-

tinued as heretofore. Two parties were formed amidst the subscribers. The managing committee split among themselves: some were for bolstering up and continuing the old system; others were for either modifying or doing away with it altogether. Years rolled on before any thing was done, and the worst of passions and petty vexations were in the interim indulged between the contending parties: religion, politics, little and local differences for victory and supremacy, were all brought into action without any credit to the individual or advantage to the charity. At last the cause of justice and truth in some degree triumphed. A sort of compromise was made some time since, namely, to get assistant house-surgeons to visit the patients, instead of the boys, or, as they were then termed, the visiting pupils. Thus did this gratuitous system work, and thus were the interests of the charities made subservient to personal and factious feelings. And though great improvement has taken place by this partial recognition of the stipendiary principle, still the working of the dispensary apparatus is bad and defective, and will never be good until the name and place of honorary medical officers be swept away from the list of the appointments; indeed, the very meaning of the word, even if we had not the light and benefit of experience, would suggest the propriety of doing so. Our dictionaries tell us, that it means "possessing a title or place without performing services or receiving a reward:" yet we are anxious for the appellation, and think there is some honour attached to it.

The practice is now for the honorary physicians and surgeons to see, or, as they term it, to *pass* their patients at the dispensaries, and to visit only the home ones, or those that are too sick to attend, when the house-surgeon or his assistant report them dangerously ill. Until then they have charge of them, like the boys in the former system, and may keep them on their hands almost as long as they please. It is true the latter are generally well-educated young gentlemen, members, for the most part, of the College of Surgeons in London; but yet, from their age and standing in the profession, too young, and very often too inexperienced, to have such a charge. It is here they first begin practice; and at the expense of the poor and the charities, they advance in experience. Now common sense ought to point out, that if any such arrangement like this were to exist, it ought to be reversed;—that the house-patients, from the very fact of being generally the most dangerously ill, should have the best advice—that is, the attendance of the honorary physicians and surgeons, as they are presumed to be by the subscribers and the public the best informed and most competent to give it; and that the house-assistant, house-surgeons, or, as they are termed, the stipendiary officers, should pass the patients that come to the dispensary, having usually the more trifling and insignificant complaints, and less in want of superior medical aid. Is there not something absurd and distorted in the arrangement that exists—at variance with sense and experience? Here we have the most important complaints intrusted to the inferior medical officers, and the most trifling to the superior ones. Is not this putting the cart before the horse, and reversing the very order and nature of things, and strongly indicative of the inconvenience and absurdity of our present dispensary systems? We have the public every year returning thanks to the honorary officers, the physicians and surgeons of the charities, as the persons presumed to have exclusively the care of the sick, and not one word of notice or thanks of the stipendiary officers, who really have it. The whole arrangement is complex and unnatural. I do not blame the honorary physicians and surgeons; they undertake what they know they neither do, nor can do, even were they to devote all their time and health to it. I know that some of them have been the slaves of their efforts in this respect, and have worked as hard as men under the circumstances could, or ought to be expected; but in vain,—the abuses have gone on. The fault is in the system, and not in the men, and only proves the necessity of a total change. This is an easy and obvious one, equally useful to the public and the profession—namely, paying the medical men for their work, and doing away with all the farce and farrago of the present cumbrous and unwieldy machinery of honorary officers, and their usual appendages of porters, nurses, servants, matrons, apothecaries, &c. Every one knows that they do not, nor cannot, attend to the patients, and that these places tend but to keep up unmeaning

and unmerited distinctions; that they are elected, almost without an exception, by local influence, personal or family connexions, without much, if any, regard to their talents, capabilities, or professional claims: in fact, that these places are made a cat's paw to feed vanity, and prop up self-conceit and emptiness, and that the result has often been, that men have from time to time been appointed to them, physically and mentally incapable of discharging their duties; and hence the public are every day beginning to look on them with more and more suspicion and distrust.

See how the paying system would work—how different would be its result and advantages to the profession. We have seen that the gratuitous one is bad and inoperative, and that the practical improvements that have occurred in it are owing to the stipendiary officers, and only prove the necessity of farther changes. We have here two public dispensaries, the North and the South, relieving, as their reports state, from forty to fifty thousand patients a year—that is, nearly one-fourth of the population of Liverpool, and having attached to each three honorary physicians and surgeons, and two or three stipendiary officers. These institutions are supported by donations, legacies, subscriptions, forming a common fund for the supply of their respective wants, and amounting in one way or other to about from two to three thousand a year. This sum may seem large to strangers, but not to those who know the customs and resources of the town. The greater part almost of our merchants, brokers, ship-owners, manufacturers, tradesmen, &c. subscribe to them as a matter of policy and interest, and are thereby entitled to send there the men in their employment, or others, when sick. Thus for a trifle they get what may otherwise cost hundreds, and the character of humane and charitable men into the bargain. To such an extent does subscribing to the public institutions, however ill conducted, appear to indicate here generous and ungenerous feelings, that candidates for public situations frequently reproach each other with their omissions and deficiencies in this respect, and that the most notorious and shameless bankrupts are often in the habit of increasing the donations or subscriptions to them, even at the very time they have beggared hundreds, and cannot, or will not, pay a shilling in the pound of their debts; so much do they calculate thereby on recovering caste and character. Now, if instead of expending nearly three thousand a year in supporting two dispensaries, they had divided the town into districts, and appointed medical men to each, physicians and surgeons, according to its extent and exigencies, under the direction of a managing committee selected by the subscribers, the work would be well done, the poor would be properly attended, and a large sum, three thousand a year, would be divided among the profession, that is now eaten up in supporting an expensive, defective, and jarring apparatus of matrons, servants, porters, clerks, collectors, apothecaries, assistant-apothecaries, house-surgeons, assistant-house-surgeons, and a host of other minor and subordinate agents. By this simple plan, humbug, and cant, and hypocrisy, would be restricted, and none but proper objects of charity would be received or attended. Thus the profession would be served and protected, and all the wishes and intentions of the charity fulfilled. In this case medical men would have an incentive to do their duty,—the fear of losing the salary; and we would all be protected from the indiscriminate abuse, and distribution of gratuitous advice and medicine, alluded to in the beginning of this paper. The stipendiary medical men would take care, for their own sakes, not to admit them, as by doing so they would only be increasing their own trouble, and subjecting themselves to the censure and reprimand of the committee and the profession. Ten medical men would be more than sufficient to attend to all the poor of the town, under proper management; and at the rate of three thousand a year—the expenditure of but two dispensaries—might have, if this sum were divided as it ought, a salary of three hundred a year each. What a new source of revenue or emolument the introduction of this system would bring, now utterly lost and cast away, or expended on a retinue of useless retainers, in sapping the foundation and comforts of medical men!

Liverpool, May 1834.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

The Transactions of the Provincial Medical and Surgical Association (Instituted 1832.*)* Vol. II.

WE take the earliest opportunity of noticing this volume—the fruit of the more mature experience of the body from which it proceeds. In our opinion, as a whole, it greatly surpasses its predecessor. The papers, though perhaps not so numerous, are more practically valuable and elaborate, and the arrangement is more complete. The excellent suggestion offered by Dr. Conolly last year, as to the propriety of cultivating local natural history and medical topography, has been acted upon; and we accordingly have, on the present occasion, three valuable topographical papers on Penwith, Bristol, and Stourport.

The *Address* delivered by Dr. Barlow at the last July meeting forms the opening paper, and is well adapted for precedence, as it takes so comprehensive a view of the various objects which engage the attention of the society, and presents so masterly a sketch of recent events in the history of medicine. It has been circulated in a separate form, however, and has already been so much read and appreciated, that we think it needless to dwell upon its contents. The author was allowed but a limited space for touching on a great variety of subjects, and he has been brief without being obscure or complicated. One fact noticed by Dr. Barlow may serve as a specimen, and, indeed, is too curious not to deserve special record.

"It is not unworthy of remark, that, on the subsidence of cholera, there appears to have occurred, in many places, a considerable abatement of ordinary disease. This was so evident at Bath as to excite the attention of the physicians of the United Hospital, to which the whole of the sick poor of Bath and its vicinity daily resort. In order to ascertain the extent of this decline, I took the average of dispensary, or out-patients, on my days of attendance, and found that for the first nine months of the year, within which period the cholera prevailed, the average daily number was 120, while for the last three months it declined to 84. As the applications of out-patients to the United Hospital are unrestricted, disease and poverty being sufficient passport, the attendance of the sick poor at the hospital may be regarded as a fair measure of the healthfulness of the city. I pretend not to account for the fact, but it is sufficiently extraordinary to justify its being recorded. It could not have proceeded from the mortality occasioned by cholera having swept away the ordinary invalids, for in the period of three months, during which cholera continued in Bath, the deaths from this disease did not amount to fifty, in a population exceeding fifty thousand."

Of the articles on medical topography we shall treat but briefly: they are all rich in details, but their chief value will arise from the general conclusions which shall ultimately be deducible from them. Medical topography is in its infancy in this country; and much caution is requisite lest we should be tempted too hastily to proceed to immature results. Extensive and very varied attainments are required for the proper conduct of researches of this nature; and however the particulars be gathered, a perfectly distinct qualification from that of mere diligence is demanded in him who would classify and generalize the conclusions. We trust, however, that the members of the Association, as they have now made a beginning in this species of inquiry, will not relax in their efforts, but every year contribute largely to the general stock of local information. In the end, ample benefit cannot but accrue to society at large from the pursuit of such investigations.

In Dr. Forbes's *Sketch of the Medical Topography of Penwith, comprising the District of Landsend, in Cornwall*, we have seen many valuable and curious facts which we should be glad to transfer to our pages; but we must keep in view the limits to which we are confined, and the duty we owe of not neglecting the learned labours of other contributors to the volume before us. Our readers, however, will be glad to see some of the particulars which Dr. Forbes is enabled to communicate, relative to the proceedings and general habits of the miners of Cornwall. After giving some account of the nature of a mine, and the economy of working it, the author proceeds:—

"A person unacquainted with the de-

tails of mining, on hearing of many hundreds of men employed in a single mine, might naturally imagine that a visit to its recesses would afford a picturesque and imposing spectacle of gregarious labour and bustle, tremendous noise, and much artificial brilliancy to cheer the gloom. Nothing, however, is farther from the truth, as far as regards the mines of Cornwall; for, like their fellow-labourers the moles, the miners are solitary in their operations. Seldom do we find more than three or four men in one gallery at a time, where they are seen pursuing the common operations of digging or boring the rock, in the inner extremity of the gallery, by the feeble glimmering of a small candle, with very little noise or bodily movement. Very seldom are they within the sound of each other's operations, except occasionally, when they hear the dull report of the explosions.

"In the vicinity of the main shaft of the mine, indeed, the incessant action of the huge chain of pumps, produces a constant but not very loud noise, while the occasional rattling of the metallic buckets for raising the ore, against the walls of the shaft, as they ascend and descend, relieves, in some degree, the monotony of the scene. Still every thing is dreary, dull, and cheerless; and one can be with difficulty persuaded, even when in the richest and most populous mines, that he is in the centre of such extensive and important operations. What adds greatly to the impression of tameness, is the extreme darkness and dirtiness of the galleries. There is no light whatever but that afforded by the candles of the workmen, and by those carried by yourself and guide; while the universal presence of water soaking through the crevices of the galleries, and intermixing with the dust and rubbish, keeps up a constant succession of dirty puddles; through these the visitor must pass, besides being frequently obliged to crawl on all fours through passages too low to admit him in any other manner. The galleries are extended by breaking down the looser parts by the pickaxe, and by rending the more solid by gunpowder. Each miner has a candle, which is stuck close by him, against the wall of his gallery, by means of a piece of clay; and besides the men employed in extending the gallery, there are generally one or two boys employed in wheeling the broken ore, &c. to the shaft. Each of these boys has also a candle fixed to his wheelbarrow, by the universal subterranean candlestick, a piece of clay. A certain band of men (called, however numerous, *a pair*) generally undertake a certain portion of work in the galleries. These subdivide themselves into smaller bodies, and, by relieving each other at the end of every six or eight hours, keep up the work uninterruptedly, except on Sunday. By means of this subdivision of the pairs, there is, in general, not more than one-third of the underground labourers below at any one time. Notwithstanding this incessant labour, the progress of the miner in excavating his gallery is, in general, very small: one, two, or three feet in a week, or a few inches daily, is often the whole amount of the united operations of twenty or thirty men. In loose lodes, and in killas districts, they often cut much more than this, but often they do not cut so much. It is to be recollected that the lode is very rarely so wide as the gallery, so that it becomes necessary, in order to continue the latter of the proper size, to hew through the solid rock on each side, which is often very hard, even when the lode is soft. The Cornish miner rarely sleeps or eats under ground, but returns to *grass* (the technical name of the surface) and to his home, often many miles distant, at whatever depth he may be working, when relieved by his successor. The mode of ascent and descent is by means of vertical ladders fixed in the shafts."

What is said in a subsequent page, relative to the mode of "returning to grass," is worth extracting:—

"The descent and ascent into and from mines, are through the shafts, by means of perpendicular ladders, and constitute a very important feature of the labours of this class of men. The labour of ascent is, to a stranger, very severe, producing, in a very short time, great breathlessness, palpitation, and exhaustion. It is accomplished by grasping the steps above you, with an alternate change of the hands, and, as it were, drawing up the lower parts of the body: it is going on all-fours up a perpendicular plane. Habit makes the labour comparatively easy to the miner, and the younger ones often make trial of their respective activity in coming to grass. Several miners have accused this practice as the primary cause of

their asthmatic sufferings in after life. When the mine is of any considerable depth, it hardly ever happens that the train of ladders is in one continuous chain from top to bottom; on the contrary, after ascending two or three, it is commonly necessary to traverse a greater or less extent of a horizontal gallery, in order to reach the next series of ladders. This gives time for breathing, although it adds to the length of the journey; for as almost all galleries are horizontal, every foot of positive ascent must be by ladders, which are almost always perpendicular. In very deep mines, this makes the labour of ascent a very serious matter, when taken daily for years. Unfortunately for the miner, most of the richest and populous mines are the deepest, and most worked in their lowest regions. When I visited Dolcoath copper-mine, in 1819, the lowest gallery was 1386 feet deep; and Huel Abraham, in 1822, was 1440; the former employed 800, and the latter 560 men, in the under-ground labours. From the account formerly given of the *ventilation* in mines, it is evident that the miner, in returning from his work, must be frequently exposed to strong or partial currents of air."

The valuable paper of Dr. Forbes is not finished in the present volume: the medical part of it is yet to come, and cannot fail to be of much interest.

The article on the *Medical Topography of Bristol*, is communicated by Drs. Carrick and Symonds, of that city, and contains much (especially in the geological department) which will entertain and instruct many readers. We observed in it some appalling facts illustrative of the effects of want and wretchedness, as sources of disease; from which Bristol is by no means exempt, any more than other large cities. "Of the moral habits of the people," says one of the authors, "it is difficult to speak with any degree of precision; and I am doubtful whether any thing more specific can be stated, than that, where so much indigence exists, its offspring, vice and ignorance, must also abound. Dr. Chisholm remarks, that he had much satisfaction in observing the regularity with which the poor of Clifton and the Hotwell-road attended to their religious duties on Sundays: we fear that there has been some degeneration in this particular since the time of Dr. Chisholm."

The ratio of mortality in Bristol the authors venture to put down at 1 in 45; yet they do not state this rather unlikely ratio with any degree of confidence. They lament, in common with every inquirer into the medical statistics of this country, that in consequence of the present (we hope we shall soon have to say the *late)* mode of keeping public registers, the results which they attempt to deduce from certain tables are less valuable than they ought to be.

"The deaths being registered at the places of *burial*, no estimate can be formed of the proportionate mortality of a parish, from an inspection of the register in that parish, because a large number of the inhabitants may have been interred in other parishes, or in private burial-grounds, or in those attached to dissenting places of worship. As a proof of the great inaccuracy that would accrue from a calculation founded upon parochial registers, the number of interments stated to have taken place in the parish church-yard of St. Philip and Jacob, would give a ratio of mortality amounting to no more than 1 in 82, to the most ill-fed and ill-clothed portion of the population of Bristol. If deaths, instead of burials, were registered, and in the places where they occur, information of the most important kind might be deduced from them, relative to the comparative salubrity of particular districts. We trust that this change, together with many others greatly needed in the system of registering, will ere long be effected by the legislature."

Mr. Kenrick Watson supplies the account of the *Medical Topography of Stourport and its neighbourhood*, in Worcestershire. He mentions, among many other interesting facts, that the mortality of Stourport, in 1832, was very great, the deaths being 82,—thirteen of them from cholera; while, during the first six months of 1833, there have been only nine funerals. The population, according to the last returns, was 2952.

Among the Essays and Cases, which form the next portion of the volume, we find *Additional Facts and Observations on the Efficacy of Strychnia in some Forms of Paralysis*, by Dr. J. L. Bardsley; and *Observations on the Treatment of Syphilis without Mercury*, by Dr. T. Green, of Bristol; both of them papers of much practical importance; as are also the *Observations on Chronic Peritoneal Inflammation*,

and its Treatment, by Mr. E. Thompsen, of Whitehaven; and the rectum lithotomy case, by Mr. Dawson, of Liverpool. But we cannot at present enter into particulars regarding their merits.

The case of *Hydrophobia*, by Mr. R. B. Grindrod, of Manchester, is uncommonly interesting, from the minuteness with which it is related, and the very creditable attention paid to it in its progress. The subject was a boy, six years and a half old, who was bitten over the eye by a rabid dog. The wound was rather neglected for a day or two, the part not being excised till then. In the course of five weeks the first symptom of the fatal malady was observed, and death ensued on the morning of the fourth day after. It may be mentioned, that the convulsive dread of fluids, in this case, only became intolerable during the paroxysms. During the first night the little patient slept for a time, disturbed, however, occasionally by the occurrence of pharyngeal spasms, when he expressed his uneasiness by a slight cry.

"After he had remained in this situation for two or three hours," says Mr. Grindrod, "I again offered him some water, which he refused, but consented to take a little warm tea. The tea, however, produced the same effects as the water; but on trying to take some by means of a spoon, he swallowed a few drops; and, on being encouraged to persevere, the deglutition became less difficult, until he had swallowed more than half a teacup full."

Blistering, and the application of morphia to the abraded surfaces; the introduction of vaccine virus into the venous system, anodyne clysters, and other active remedies, were tried, but all with little or no effect. Downright insanity was observed among the symptoms, and it was of no inconsiderable duration. The boy sunk at last without any immediate struggle. We shall give a short extract from the postmortem examination.

"On opening the pharynx, it was evident that this part had been much affected during life. The greater portion of its inner surface was intensely inflamed, and terminated in a defined line opposite the cricoid cartilages. The velum pendulum palati, uvula, and most parts of the pharynx, showed considerable development of the mucous glands. The tonsils were slightly enlarged. The œsophagus was greatly inflamed near its termination, and in one part the mucous membrane was slightly abraded. The whole mucous membrane was so softened, that the slightest touch of the scalpel separated it.

"*Abdomen.*—The stomach was remarkably contracted, and completely empty. Intense inflammatory patches were seen on the rugæ, which were very much developed. The mucus was very thick, and had, in some parts, an unusually deep colour. On one of the rugæ was observed a dark spot, about the size of a small shot. The stomach appeared healthy between the rugæ, but had, in some places, a darker colour than natural. The mucous membrane of the duodenum displayed intense inflammation also; and the glandulæ solitariæ were exceedingly enlarged, and very numerous. The bowels were not particularly examined. The liver was healthy. The gall bladder contained bile, and the urinary bladder about an ounce and a half of urine. Kidneys apparently healthy, as also the spleen. The whole of the nervous system was remarkably developed.

"The par vagum, glosso-pharyngeal, gustatory, pharyngeal, and phrenic nerves, were in a healthy condition. The ganglia of the sympathetic were larger, and appeared of a deeper colour, than usual."

A well-executed plate, representing the morbid appearances, is subjoined to the paper; and the author concludes with an interesting conjecture on the probable predisposing cause of *rabies canina*.

"The bitch [that did the mischief in this case] was about two years and a half old; and, about a year and a half ago, had intercourse with a dog of a very large size, and eventually gave birth to two unusually large pups, which occasioned her much pain during parturition. The next morning it was discovered that she had eaten the greater part of the inside of both her offspring. The owner positively affirms it to be his belief that she had no connexion with any other dog after that time. Both he and others have frequently seen her refuse the familiar approaches of dogs in the street. For three or four days previous to her exhibiting any symptoms of madness, the uterine orgasm was present."

Dr. Brown, of Sunderland, has written an excellent paper *On the Variations in the Prevalence of certain Diseases not usually supposed to be epidemic.* It is founded on a remark made by Laennec relative to the subject—namely, that when he was a pupil of Corvisart, he saw more cases of extensive ossification of the valves of the heart than he met with during the subsequent twenty years, which he attributes to certain influences of the *constitution médicale*, more frequent in certain seasons than in others.

The observations of Dr. Brown extend to various disorders—such as those of the heart, hydrocephalus acutus, diffuse inflammation and gangrene resulting from accidental injuries, cerebral apoplexy, and syphilis. What the author says on the two latter subjects will, we think, be interesting to the reader.

"In the early part of 1832, more cases of cerebral apoplexy fell under my observation than had been furnished by the whole course of some preceding years. Besides several cases occurring less closely on each other, in little more than one week six fatal examples of the disease fell under my observation. It is true that these attacks occurred to individuals, who, from constitution and period of life, would have been thought likely to become subjects of such an affection; but the circumstance that, in so short a time, the predisposition should have been brought into action in so many persons, affords the aspect, at least, of some general cause co-operating with the diseased tendency. This is not the sole period in which a diffusion of apoplexy, that might be deemed epidemic, has been observed; indeed, it rarely occurs in my practice, in a manner that appears purely sporadic; and the period of its prevalence has more frequently been the winter, or early part of spring, than a warmer season, when some effect might probably have been ascribed to insolation. It has, in fact, coincided, in the period of its prevalence, with inflammatory diseases in general.

"With regard to these last-named diseases, as they are generally considered to be the product of atmospheric influence, either hidden or manifest, they ought not, perhaps, in strict propriety, to be noticed in an essay professing solely to furnish examples of the variable prevalence of diseases, not usually thought capable of assuming an epidemic character. They are adverted to here for the sake of calling the attention of the Association to the simultaneous occurrence of many cases of the same disease, or of diseases strikingly congenerous of this class. Thus, at one time, we shall see many examples of enteritis; at another period, the inflammations we witness shall be seated in the thoracic cavity. This last has been the case during the present year, (1833,) the internal inflammations which have occurred here having been situated almost universally in the chest, the affinity being thus marked to the great epidemic of the year, influenza. Within a very few days in July of this year, four cases of pleuritic effusion, more or less of a chronic character, fell under my observation."

"Some years ago it fell to my lot to witness, frequently, the varying prevalence, and the variable intensity, in the symptoms of a disease, one cause of which being distinctly understood, the profession has not been disposed, and, perhaps, will not yet be disposed, to admit that another may modify its diffusion and its phenomena. I allude to syphilis. Not only is there a wide difference in its prevalence in different localities, and at different times in the same locality, but, under such circumstances, a very wide range indeed is observed in the intensity of its symptoms. At certain periods, or in certain places, not merely suppurating bubos are more frequently met with, but the much worse symptoms of rapid gangrene, or slowly consuming phagedæna, threaten the patient with the most melancholy of mutilations, and add to the difficulties of the surgeon. So frequently, some years ago, did I witness this occasional aggravation of the symptoms of this disease, without any assignable cause, except, possibly, an epidemic condition of the air, affecting both its diffusion and its intensity, that nearly every lapse of chastity was followed by condign punishment; and which condition appeared to me to furnish the real explanation of its ravages at the close of the fifteenth century—ravages which gave rise to the absurd fiction of importation from the newly-discovered America. Arguments to illustrate this view of the calamity, which, at the epoch mentioned, fell so heavily not only on the commonalty, but on popes, kings, cardinals, and the whole convents of *devout* and *chaste* nuns, would

be misplaced in the present paper, and would, besides, be rather difficult to treat by one (to use the expression of Celsus,) '*et pudorem et præcepta artis servante*;' but all who have taken pains to examine the not unamusing records of that period, will agree that it is a more rational explanation of what occurred, than the doctrine of importation hatched by Oviedo, and subsequently fostered by Astruc."

We are obliged to postpone our further remarks on the contents of this volume till another opportunity: at present we will only stay to add, that Dr. Walker, of Huddersfield, has contributed to the work some further observations *On the Diseases of Infants*; and that Dr. Scott, of Liverpool, has continued his remarks *On the Influence of Sleep*, so ably begun in the former volume of the Transactions.

MEDICAL GAZETTE.

Saturday, June 7, 1834.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

MEDICAL SCIENCE IN SPAIN.

IT is not a little interesting, as well as satisfactory, to learn that the recent changes in the political aspect of Spain are likely to operate beneficially towards the advancement, perhaps we should rather say the revival, of medical science in the peninsula. Few things were more wanted. The almost perfect stillness which has reigned so long over the Spanish medical world—the torpidity of medical knowledge in that country, while almost every where else signs of animation were visible—has been for some time matter both of surprise and regret. Spain is generally known as a land rich in resources for the contemplation of various and peculiar forms of disease*; as a land gifted with intellectual capability of a high order: and every scholar recollects it as the country of Averroes, and where Moorish and Jewish medicine flourished at a time when darkness was spread over other parts of Europe. Yet the blank which it has for a considerable period presented in medical history, has been but too conspicuous. May it be hoped at last that the new impulse now given will be attended with a degree of energy and reaction proportionate to the late inactivity? At all events, we are glad to see that even the semblance of a better state of things begins to be discernible in that kingdom, and that the province of medicine, which has so long been there a barren waste, comparatively with its condition in other places, will probably soon be brought into something like a state of culture: measures, it appears, are taken, or are about to be brought into effect, for such a thorough change in the aspect of medical science in that country, as cannot but be hailed by the profession throughout Europe with pleasurable anticipations. For our parts, we cannot be insensible to the rise of a new namesake and contemporary above the southern horizon; we need scarcely say that we wish every success and all prosperity to the *Medical Gazette of Madrid!*

"The impulse now given to the public mind in Spain (says the *Times* correspondent of this week), begins to be felt in every department of useful knowledge throughout the kingdom. There was none, perhaps, that needed it more than the science of medicine; the study of which has been kept up by a few of the more eminent practitioners, to the common level of Europe, through the expensive, and often unattainable medium of foreign publications. You will judge of its condition generally when you are told, that, even in Madrid, the sister science of surgery is still in the hands of the barber; the helmet of Mambrino being universally combined with the intimation that under it dwells *El Pro-*

* There are twelve or fourteen public hospitals in Madrid alone; some of them on a very large scale. The largest, by the way, is said to be chiefly supported by the money gathered at the bull-fights.

fesor de Cirurgia. To provide a remedy for this state of matters, a few of the most distinguished physicians of the capital have resolved on issuing a weekly periodical, under the editorial care of the well-known Dr. Alfaro, to be called the GACETA MEDICA DE MADRID. In this publication, the doctrines of the French school will no longer have that exclusive ascendancy they have hitherto enjoyed in the medical practice of the peninsula. A decided preference will henceforth be given to whatever is national in project or discovery, in theory or practice, without omitting the means of comparison afforded by the systems pursued in other countries. The physicians of Spain will then have the means of supplying their quota to the general European stock of medical knowledge."

Some readers may, perhaps, hastily infer, from this paragraph, that the field of periodical literature has been hitherto totally untouched in Spain: but this has been by no means the case. For some time back — and, indeed, we are not aware that the work has yet been discontinued—the *Decadas de Medicina e de Cirurgia practicas* have been published in Madrid thrice a week, under the direction of Dr. Hurtado, a strenuous supporter of the Broussaian doctrines. A medical journal was also, not long since, established at Barcelona, but, for want of sufficient patronage, it soon ceased to exist; and a periodical devoted to the natural sciences was lately begun at Madrid, but much permanency was not augured for it, owing to the low state of the cultivation of natural science in Spain, and, of course, the deficiency of contributions that must be the result. The *Memorias Academicas de la R. Sociedad de Medicina de Sevilla*, and the *Periodico de la Sociedad de Cadiz*, have, we believe, for some years been numbered with the defunct. But it must be confessed there is much difficulty in procuring exact information in this country relative to Spanish literature, owing to the little encouragement afforded for the importation of Spanish books, or, perhaps we should rather say, the embargo laid on their exportation by the authorities in Spain.

The mention of surgery and its practitioners, in the above paragraph from the *Times*, reminds us of the once-celebrated *Colegio de Cirurgia*, of Madrid —concerning which so little has been heard of late, though it was commenced, and for some time carried on, under the happiest auspices. It was founded in the year 1787, by Carlos III.; and the laws and regulations (a copy of which we happen to have now lying before us) are many of them, we are bound to say, excellent. Except that the authors prescribed as text-books (such as Boerhaave, Gorter, and Astruc) were rather out of date in other parts of Europe even at the time they were enjoined to the Spanish students, every thing relating to the exercises of the pupils—their partition of time—their frequent examinations—and the rewards held out to them—could scarcely be improved at the present day.

A passage from those laws, in which the qualifications for admission to the College are laid down, is not a little curious, as it shows the desire of the government to keep the profession respectable.

"In order to be matriculated, the students must understand Latin, and have studied logic, algebra, geometry, and experimental philosophy, for three years. They must produce certificates from the Priest and *Regidores* of their parish, of their purity of blood, baptism, parents' marriage, and of their life and manners. They must also have such supplies from their friends as will support them decently, *without the necessity of their serving either as barbers* or servants. If any one is discovered in thus supporting himself, after matriculation, his matriculation shall be void."

And the following item is rather amusing,—as it seems to indicate a propensity in the Spanish surgeons to

wield the lathering brush as well as the lancet.

"The students (having obtained their certificate) may settle and exercise their profession in any part of the kingdom. But if any student *turns barber*, he forfeits all his privileges."

The medical practitioners of Spain, we may add, are at present chiefly divided into three classes:—1. Physicians, who have received a regular preliminary education in arts at an university; 2. Medico-surgeons, or surgeons who are privileged by their education to practise also as physicians; and, 3. The lowest grade, called *Cirugianos romanceros*, who are only permitted to practise minor surgery, and are forbidden, under a serious penalty, to undertake the treatment of *inner* complaints. The education of this last class, or at least the preliminary part of it, is very superficial,—candidate *romanceros* not being required to understand Latin, or indeed any other language than their mother tongue. But we have reason to believe that the discipline observed in forming the other two classes is generally good, though perhaps it will require considerable reform to render it suited to produce practitioners like those of England, Germany, or France.

SPECIMEN OF THE CONCOURS.

Nothing can exceed the contempt expressed by the various French journalists for the recent decisions of the Concours; and that which some blockheads among us hold up as the *beau ideal* of what an election ought to be, is abused by those who have it, as the most corrupt and inefficient method of appointment that can be devised. Great part of their recent proceedings for the choice of a Professor of Midwifery consisted of theatrical displays between the candidates, who fenced with great dexterity, for the amusement of the by-standers. The following, which is the concluding portion of a scene between two of the aspirants, may serve as an example:—

M. Velpeau.—In hæmorrhage before delivery, the precept is to hasten the delivery—why do you advise delay?

M. Colomb.—I have seen the attempt to deliver prove fatal.

M. Velpeau.—Certainly it does not always prevent death.

M. Colomb.—But if it hastens death!—besides, I speak of the means of producing contraction of the uterus where there is want of action.

M. Velpeau.—The best way is to deliver.

M. Colomb.—But if there be danger of death?

M. Velpeau.—You must deliver before.

M. Colomb.—If the patient is weak, you kill her.

M. Velpeau.—What would you do then?

M. Colomb.—Wait.

M. Velpeau.—She dies!

The exclamation of Velpeau—*Elle meurt!*—is scarcely less theatrical than the famous *Qu'il mourut!* of Corneille. In such a contest, a ready wit and a sharp tongue are sure to carry the day against the soundest head and the most extensive acquirements.

FRESH BURTHEN ON MEDICAL MEN.

In the County Coroners' Bill, now before the House of Commons, there is another added to the many specimens presented to us of a disposition on the part of the legislature to overlook, or trample on, the rights of medical men. A clause is contained in the above, which requires that they should attend postmortem examinations without any fee! What! do not medical men give enough of gratuitous service to the public, that this new tax should be laid upon them? We observe that Colonel Wood, on Wednesday night, presented a petition from some medical men, deprecating the hardship of the clause just mentioned. We would suggest that *all* medical men are called upon to protest against the enormity of such partial legislation—such absolute infringement of the rights, if not the liberty, of the subject.

PARLIAMENTARY MEDICAL COMMITTEE.

THERE was no sitting on Tuesday, in consequence of the late hour at which the House broke up on Monday night: but on Wednesday, Mr. Warburton was engaged, for the greater part of the day, in hearing the evidence of Dr. Macartney, of Dublin. We understand, on the best authority, that the business of the Committee will be completed before the close of the present session; so that we may expect parliament to proceed to legislate on the subject early next year.

ROYAL INSTITUTION.

Friday, May 30, 1834.

Mechanical Notation of Machinery.

DR. LARDNER gave an excellent lecture this evening, on a new contrivance of Mr. Babbage's, which bids fair to eclipse, in point of extensive utility, even the far-famed calculating machine. In the progress of his invention of the latter surprising structure, Mr. Babbage frequently found himself at a loss for a comprehensive view of its simultaneous movements, without which he almost despaired of bringing the engine to any perfection. The thought struck him, that it might be possible to transfer to a table some form of expression for the several motions performed simultaneously and successively by the various parts; so that by thus translating all the conditions of the work into conventional symbols (as is done in the application of algebraical formulæ to questions in physics), it would be easy to reason abstractedly or mechanically upon the operation of the whole. He succeeded to his wishes, and soon saw that the tabular notation which he adopted for his calculating engine, might be applied to machinery in general; that, in fact, there was no limit to its applicability in all cases where work was performed by a number of agents, either physical or moral. It would require more space than we can afford, to follow the lecturer in his speculations on this subject; nor can we pretend to give more than a mere hint of the method adopted in this kind of tabular notation. Dr. Lardner illustrated the subject by a reference to the steam-engine; the various parts of which, with their circumstances, were disposed in a hundred compartments, so that by simply running the eye along the horizontal divisions of the table, the business of each part at a given moment was exactly perceived. One immediate advantage of this happy contrivance has been, that the time of working with the calculating engine has been reduced one-half. Before the method of notation was adopted, it required eight turns of the handle to arrive at and to register the result; but by taking a comprehensive view of the proceedings of each separate portion of the engine, Mr. Babbage was enabled to see where he might economize the time, by packing, as it were, the motions into smaller space; so that he is now able to procure a complete movement of all the several parts of the engine by four turns of the handle. The whole subject was satisfactorily explained by reference to tabular diagrams and a sectional model of a steam-engine.

SURGICAL REPORTS FROM THE LONDON DISPENSARY.

By R. R. ROBINSON, Esq.
Surgeon to that Institution.

Meningitis—Tumor of the Brain—Pressure on the Fifth Nerve on the left side—Sloughing of left Cheek—Caries of the Bones of this side of the Face.

JOHN DUPEN, æt. 11, admitted January 21st. About seven months ago he had a pimple upon the left cheek, which broke, discharged a yellow matter, and healed up. In five weeks afterwards it again appeared, but was dispersed by an herbalist. About five weeks ago he was seized with fever, and was attended by the same man. He is exceedingly reduced; his face is much disfigured by a considerable swelling of the left side of it; skin of left cheek pale and shining; upper left eye-lid completely closed from œdema; two-thirds of left cheek occupied by a deep irregular slough, which extends into the mouth, and which his father says only began two days ago, and spread rapidly; the surrounding edges very red and thickened; the whole of the left side of the palate is destroyed by ulceration; the bones carious. He has always had a difficulty in breathing through the left nostril; some bleeding from both lately; headache; quite sensible; no inclination to take food; will drink wine or porter; skin dry; tongue dry; night sweats; pulse quick and feeble; no motion for some days; debility so great that he cannot sit up in bed.

Ol. Ricini, ℥j. statim; Cat. Panis. Inf. Gent. Comp. c. Acid. Sulph. Dilut. ♏x. ter die. Porter.

Jan. 24th.—Some sleep; gangrene extends towards the inner canthus of the left eye, from which there is sero-purulent dis

charge; slough firm and very offensive; left eye-lid still closed from œdema; right now open; left nostril swelled, and apparently blocked up with a whitish matter.

Sol. Calcis Chlorid. Rcp. omnia.

28th.—Sloughing has extended, and has destroyed the integuments of the left inferior palpebra, the left side of the nose, and extends to the inferior edge of the left ramus of the inferior maxilla: the sloughing has extended internally also. Bowels open.

Repetantur omnia.

31st.—Did not speak after I left him; appeared to become insensible; and died this morning.

Dissection.—Some serum under scalp; dura mater, in course of longitudinal sinus, thickened, and covered externally by vascular specks; the whole of the left side of the dura mater, interiorly, was covered by yellow coagulable lymph; a considerable quantity of serous fluid upon the surface of both hemispheres of the brain; tunica arachnoidea opaque and gelatinous; surface of the brain and corpora striata preternaturally firm, but the thalami and posterior part of the fornix in a complete state of ramollissement, and ragged; water in the ventricles and base of the skull, to the amount of full ʒvi. Upon turning up the brain, upon the anterior of the middle lobe was seen a small oval tumor, of green colour, with crimson vessels ramifying upon it; in the centre was a depression, the size of a pin's head, apparently from softening; the tumor altogether was soft. Upon soaking it some time in rectified spirit, it lost its green-coloured exterior; and upon cutting it, it was found externally soft, internally hard, and possessing all the appearance of coagulated blood. There was much coagulable lymph about the arachnoid at the base of the skull, also on the interior of the dura mater; exterior also to the dura mater was a considerable quantity of hard coagulable lymph, in which the fifth nerve, anterior to its origin, but before it forms the ganglion, on the left side, was completely imbedded; the substance of the brain surrounding the tumor healthy; the bones at the anterior part of the left side of the base of the skull were of black colour; superior maxillary bone (left) deprived of its periosteum: its palatine process, as well as the pallati bone, destroyed by caries.

Remarks.—There occur to me two ways of viewing this case; which is the most probable I will leave to those of more experience to determine.

We may suppose that the pimple either went on enlarging, or that inflammation was set up around it, which spread from the integuments inwards till it affected the membranes of the malar, superior maxillary, and pallati bones, on this side, and that then inflammation of the membranes of the brain occurred, with the effusion of coagulable lymph,—thus making the head affection secondary. Or we may suppose, that, in consequence of the fever with which he was said to have been affected, cerebral meningitis first occurred, which was followed by effusion of coagulable lymph around the fifth nerve on the left side, which destroyed its function; and that inflammation and destruction of the parts to which it is supplied was the result.

This supposition is, perhaps, of the two the most probable, especially if the father's statement can be relied upon; viz. that the sloughing only began two days before I saw the patient, and then spread rapidly.

This view is further confirmed by what happened to the eye in Mr. Stanley's case of tumor of the brain, related at p. 531 of the first volume of the Medical Gazette; by its agreeing with the effects of experiments upon animals, by Magendie and others; and by the inflammation here being analogous to that occurring in the mucous membrane of the bladder in retention of urine the result of fractures of the spine.

Ascites after Parturition—Diminished by Mercury—Subsequently increased—Removed by Tapping—Rapidly re-appeared—Ultimately disappeared spontaneously—Perfect Recovery.

Mary McKey, æt. 26: transferred from the physician to me, August 27th, 1833. Appears of good constitution; the mother of three children. Labours short and natural. Was put to bed of her last child, 29th June. After delivery her abdomen was as large as before; her feet and ankles were rather swelled before delivery, and afterwards remained so. About the third day after the birth of the child she got up, and the abdomen increased in size. She was churched at the three weeks end, after which the abdomen still further increased in size, and she had some dyspnœa and cough. Elaterium and supertartrate of potash and gamboge were given; and on the 22d instant she rubbed in the strong mercurial ointment. Her mouth is slightly sore; the abdomen, though considerably swelled, (forty-eight inches in circumference), is not very tense; there is a decided fluctuation on percussion. She has still a slight cough, but her breathing is free; respiratory murmur audible throughout the chest; slight rhoncus crepitans posteriorly and inferiorly, particularly on the right side. No fever; sleeps well. Pulse small, quick, and feeble; tongue clean and moist; bowels regular; urine copious; skin dry; faints at times

on the least motion. Repetatur ung. nocte maneque; Ol. Ricini, ℥ss.; p. v. 2; Pulv. digitalis, gr. $\frac{1}{6}$; Quin. Sulph. g. i. 3 die. The abdomen to be bandaged.

Sept. 3d.—General health exceedingly good. Finds great support from the bandage; feels stronger; mouth sore; abdomen has gradually diminished, and is now reduced to 9½ inches; legs rather less swollen; milk deficient from the first; child much purged and greatly emaciated. Rep. ung. o. n. et Pulv. and bandage. A wet nurse.

7th.—Child wonderfully plumped out since the change, and no longer purged. Mother steadily improving: legs have not swelled since last report, though she has sat up for an hour or two at a time: abdomen still decreasing, measuring four inches and a half less than on the 3d instant: a troublesome cough the last few days, relieved by linctus; mouth more sore. Some ptyalism. Repetantur omnia.

16th. — Up, and goes out; ointment omitted; mouth still sore; abdomen two inches less; faint at times. Omitt. medicamenta.

Oct. 1st.—Swelling of the abdomen rather increased, it is feared, but not proved by measurement; fluctuation more distinct; is going into the country. Repetatur unguentum nocte maneque.

Nov. 12th.—Returned from country a few days; abdomen much more swelled and tense, 44 inches in circumference; umbilicus has started; the fluid passes into it, forming a fluctuating tumor, easily emptied; no œdema of legs; much dyspnœa; slight rhoncus crepitans throughout posterior part of both lungs; general health good; bowels opened yesterday by oil; paracentesis abdominis; ten quarts of green, clear fluid removed; no flakes of coagulable lymph; operation well borne; no visceral disease detected, though there seemed slight enlargement and hardening in the right lobe of the liver.

14th.—Sitting up; easy; slept well; breathes much freer; cough trifling; mouth sore; several motions, with some griping.

Pil. Hydr. gr. iij. Pil. sap. c. Opio, gr. ij. o. n.
Inf. Gent. Co. ℥iss. Potass. Acet. ʒ ter die.

Jan. 7th, 1833.—Abdomen within an inch as large as when she was tapped; has not menstruated since her confinement; breathing unoppressed; legs do not swell; general health remarkably good.

Jan. 13th, 1834.—She called at the Dispensary to-day to inform me that she went into the country soon after last report; that the abdomen gradually decreased, and that she believed the fluid had entirely disappeared for the last two months. On a careful examination, there is not the least trace of fluid in the abdomen, nor any visceral enlargement; the abdominal parietes are flaccid; she finds great support from the bandage; goes about her work well; looks and feels in perfect health.

Remarks.—There are a few points of interest in this case, to which I will briefly allude. 1st. I would ask to what was the dropsy attributable? — to pressure upon the large veins during utero-gestation; to disease of the liver; or to increased action in the vessels of the peritoneum?

That it is not entirely owing to pressure during pregnancy, is, I think, to be inferred from its return after paracentesis abdominis subsequent to delivery.

That it was not caused by hepatic disease, I am also inclined to infer, from the patient's perfect restoration to health; from the few symptoms of hepatic derangement under which she laboured; and from enlargement or induration of the liver being suspected rather than proved.

I am therefore of opinion, that the dropsy was in this case owing to increased action in the vessels of the peritoneum, to which I am more especially led by the rapid return of the fluid after its removal—by its spontaneous disappearance subsequently—and by the circumstances which, in my mind, militate against the dropsy being produced by the before-mentioned causes.

I do not think the increased action amounted to actual inflammation, because there were no symptoms of peritonitis, and the fluid when withdrawn was quite transparent, and free from flakes of coagulable lymph—a circumstance by which I have frequently distinguished ascites with inflammation, from ascites the result of chronic visceral enlargement, or induration, a distinction sometimes difficult, but always important.

The good effects of mercury in diminishing dropsical effusions were conspicuous in this case, which is further remarkable from the palliative means proving radical, a circumstance rare in the treatment of hydrocele, and still more unfrequent in that of ascites.

Extensive Strumous Abscess of the Abdominal Parietes.

Ruth Sycamore, æt. 19; admitted 7th June. Perceived occasional pain, slightly increased on pressure (without either constipation, diarrhœa, or sickness) in the right side of abdomen, thirteen weeks ago, soon after which there was a swelling, which (in spite of three dozen leeches at different times applied, after which the pain was always worse) has gradually increased; chills at times, but no rigors. There is a hard,

indistinctly-circumscribed tumor, extending over the greater part of the abdomen below the umbilicus, very tense, more especially in the situation of the rectus muscle, where the integuments are very red, and at one point soft, as if disposed to point; excruciating darting pain throughout abdomen, but more especially to the left side; she cannot bear the least pressure; face flushed; nocturnal sweats; nights restless; catamenia regular, and have been throughout; pulse soft, small, and slow; rest has been procured by opiates; sulphate of quinine, with dilute sulphuric acid, have been taken. Cat. Panis, rep. med.

June 9th.—A distinct rigor last evening. Tumor exquisitely painful; disposition to point more decided; an indistinct fluctuation. An incision an inch long was made into the part at which the pointing appeared, and a pint and a half of curdy, greenish, offensive pus, escaped. The tumor, of course, greatly disappeared, but some thickness appeared to the left of the linea alba, and over the anterior superior spinous process of the ilium. A roller, with slight pad, were applied; the lips of the wound were slightly kept apart.

Medicines to be continued.

14th.—The wound has been kept open, and matter evacuated daily, though less and less in quantity; and to-day only ℥ij. of serous fluid escaped. Much easier; improved in all respects; is able to sit up.

Rep. Med.

18th.—More restless the last two days; pain and tenderness in the right inguinal region, where there is a small abscess forming; original orifice all but closed; does not discharge.

21st.—Pain has continued severe; abscess in groin opened; appetite gone; sweats have returned.

Rep. Cat. et Medicamenta.

August 13th.—Pain subsided; punctures still discharge, and are surrounded by slight strumous fungus; hardness in right iliac region much diminished; bowels confined.

P. P. ℈j. Tr. Iodinæ, ηιv. ex Dec. Cinchon. ter die.

The orifices were slow in healing, and she gradually regained her health; but she has since occasionally been subject to small abscesses in the above situation. She lately had one of larger size, which broke of itself, discharged much pus, and then healed. She has, however, been able to follow her occupation (that of a housemaid), though occasionally interrupted by these abscesses.

Remarks.—Abscesses may occur in any part of the abdominal parietes, either above or below the fascious productions met with here: above, they are of no moment, as they soon get well; below the fasciæ, however, they are much more important. If they take place between the external oblique muscle and the superficial fascia, or under the anterior sheath of the rectus, they generally point externally; and if a free incision is made, further mischief is usually prevented. But when they occur about the posterior part of that sheath, or between the internal oblique and transverse muscles, they are generally serious, as they are seldom detected sufficiently early to be opened by the surgeon: inflammation spreads rapidly to the peritoneum; the bowel is at some part perforated, and the abscess in that way evacuated. And although it has occasionally happened that persons have eventually recovered, the generality have been carried off by hectic. It was this disease which so prematurely destroyed Mr. Burns, the distinguished anatomist of Glasgow*.

DR. RIGBY'S

MIDWIFERY HOSPITAL REPORTS.

GENERAL LYING-IN HOSPITAL.

[Continued from p. 335.]

PUERPERAL MANIA.

During the whole year there has not been a single case of puerperal convulsions, either among the in or out-patients. Two patients died from apoplectic attacks at different intervals after labour: in the one, a severe blow on the head was ascertained to have been received during her pregnancy, from which she had never perfectly recovered; the other was a short-necked woman, but where the immediate cause of the attack was very obscure; she had been seen, when sitting up in bed, to lean forward, apparently lose her balance, and fall out of bed, when she struck the head with considerable violence upon the floor; she was quickly raised, and found to speak incoherently: fatal coma, in spite of active bleeding, came on. Having had no opportunity of examining the state of the brain, it must remain undetermined how far the blow conduced to cause the apoplectic seizure.

No case of the common form of puerperal mania has occurred during the year. In using the word *common*, I mean that form which is the result of debility—a state of excitement without power, as the late Dr. Gooch has admirably expressed

* Vide Burns's Surgical Anatomy of the Head and Neck, by Granville Sharp Pattison.

himself. The case which I present to your notice shows a very different state of the circulation.

December 2, 1833. — Anne Fearman, æt. 18. First pregnancy. Short, stout; black eyes and hair, flushed face; manner wild and strange. The pains do not appear to be genuine labour pains, being chiefly situated in the abdomen: pulse quick and labouring: she was bled to ℥xij. with slight relief. In about four hours after, finding her manner more wild, and her head and face very hot, I bled her again to ℥xx. which cooled her considerably, but did not produce faintness: she vomited much, having eaten a large quantity of pork and onions before coming to the hospital. The relaxation produced by the bleeding was sufficient to bring on labour very rapidly, and she was quickly delivered of a girl, which presented with the head in the first position.

4th.—Has felt very well till this evening. She is now feverish, with pain of head, and wildness of manner: no appearance of lochia to-day; no pain of abdomen: a dose of sulph. and carb. magnesiæ, with ♏xxv. of liq. antim. tart. was given. Perspired during the night: bowels open.

5th. — Complains of tenderness of abdomen; a poultice was applied; and at noon eight grains of calomel and four of pulv. antim. were given.

6th. — The calomel has operated five times, producing curdy, pale stools, one of which showed flakes like chopped membrane: the lochia has been recalled by warm injections into the vagina: feels much better: a slight laceration of the perineum had been produced, not having been sufficiently supported by a midwife pupil, and the vagina had appeared somewhat ecchymosed, but is now much better. Upon inquiry, I find that her father had destroyed himself, and that her whole family had shown a disposition to mania; and some time ago she received a wound of the head, by which a large portion of the scalp was separated from the skull.

8th.— Feels much better; but having an unpleasant odour about her, some warm water was thrown up into the vagina: slept well; bowels open: sumat hyd. submur. and pulv. antim. aa. gr. iv.

This form of puerperal mania is much more rare than the other: it is the mania accompanied with more or less phrenitis, and if not timely checked is much more liable to lead to a fatal result than the asthenic melancholic form, so admirably described by Dr. Gooch. The plan of treatment must be as different as the causes are opposite; for the opiates, which in the one produce such relief, would run a risk of converting the other into fatal apoplexy.

PLACENTA PRÆVIA.

Of *placenta pravia* two cases have occurred among the out-patients.

Name and date omitted. Out-patient: was suddenly surprised by an attack of uterine hæmorrhage three weeks ago, which ceased, but again appeared, much more profusely, this morning. By the midwife's account, she had lost a very considerable quantity of blood. The patient is weak, but not faint, and the pulse tolerably good. Pains very slight; vagina full of coagula; and the os uteri, which is remarkably forward, and dilated to about the size of a crown-piece, is covered, to the extent of a third, by the placenta; beyond which the head is felt presenting. I was obliged to introduce my hand to ascertain this, the os uteri and fœtal head being so very distant. As the head was pressing the placenta, and the pains very weak, I gave a dose of ergot of rye to increase them: this soon produced its effect; the os uteri began to dilate, and the head continuing to press firmly, no further hæmorrhage occurred; and in a short space of time she was delivered of a dead child.

It is of great importance to ascertain how much of the os uteri is covered by the placenta; and to do this properly, it will be occasionally necessary to follow the advice which my father has given, and introduce the hand. Generally speaking, the vagina is more or less relaxed by the hæmorrhage, and the hand is insinuated without causing much suffering. When the os uteri is but partially covered by placenta, and the pains slow, the secale cornutum is a very valuable remedy; the head presses upon the os uteri, and acting thus as a plug, controls all further hæmorrhage.

Jan. 8th, 1833.—Ellen Lennard, æt. 27, out-patient; fourth pregnancy; of middling size; well-made; healthy; has gone her full time. Was suddenly surprised, when crossing the threshold of her door, by a profuse discharge of blood from the vagina. She sent for her midwife, who, finding the placenta covering the os uteri, sent for me.

I found her extremely faint; no pulse at the wrist, and not easily roused; she was lying on her back, with a large pool of blood under the bed. On examination, the vagina was found soft and dilated, full of coagula; no hæmorrhage at this moment. The os uteri was sufficiently open to admit my four fingers, and soon dilated so as to allow the hand to pass; scarcely any pain, and the parts peculiarly flaccid. I passed my hand along the left sacro-iliac synchondrosis side of the os uteri, and insinuated it as well as I could between it and the placenta, until I felt the membranes. The

head presented in the first position. On rupturing the membranes I felt the arms and hands, but could not gain a foot for some little time. At last I succeeded in bringing one down. No pulsation in the cord. When the trunk was born, I gave her a dose of secale cornutum; but from the unruly behaviour of the patient the head did not follow for some time. The child was full-grown, and the placenta was quickly expelled; it had been centrically fixed over the os uteri, and besides the rupture which I had made in the membranes, it was torn from its edge to the insertion of the cord. No hæmorrhage took place either during or after the operation.

9th.—I was not able to visit her; but as it was reported that she was suffering much pain of the abdomen, I ordered her eight grains of calomel, with ten of Dover's powder; a linseed meal poultice; and a draught of liq. opii sed. at night. Let her kneel when passing water.

10th.—Much relieved; bowels opened two or three times; slept somewhat in the night. Pulse natural, but very weak; tongue clean; complains much of violent headache, increased by the slightest noise; still some pain of abdomen; uterus large and hard. I repeated the calomel and Dover's powder immediately, and the poultice.

12th.—Little or no pain; bowels open moderately; has not had much sleep; complains of headache.

℞ Ext. Hyosc. et Camphoræ, aa. gr. x. M. ft. pil. iv.; capiat ij. statim, et ij. h. s.

One great peculiarity in this form of uterine hæmorrhage is, that it comes on suddenly, and mostly without any assignable cause, and usually at what, in the unimpregnated state, would have been a menstrual period; thus, if it has appeared some little time before the end of pregnancy, it generally stops a while, and does not appear again until four weeks have elapsed, when it becomes more profuse. In both of the above-mentioned cases the hæmorrhage appeared suddenly, although in the latter not until the full term of pregnancy. In looking over the numerous cases of placenta prævia which my father has recorded, in his valuable work on uterine hæmorrhage, I find it mentioned in only one case (37th), that the placenta was partially situated on the os uteri; and from his description I should have been induced to suppose that in every other instance the os uteri had been entirely covered by it; but in the cases which I have had the opportunity of observing, the partial attachment of the placenta seems to occur as frequently, if not more so, than where it is centrically fixed. This is a point, however, which requires greater experience than I can as yet boast of, to decide. It would seem, nevertheless, to lead to a question which is not only interesting, but may be of practical importance, viz. are not those hæmorrhages from placenta prævia, which make their first appearance in the seventh and eighth months of pregnancy, generally connected with a *partial* attachment of the placenta to the os uteri? On the other hand, where it is centrically attached, does not the patient usually go to the full term of gestation before any hæmorrhage takes place?

The faint and exhausted state in which I found the patient, so far from contra-indicating my attempting to turn the child, required the immediate performance of this operation. There is a passage in Wigaud's "Geburt des Menschen," which I generally make a point of quoting in my lectures on turning; it is so applicable to the present subject, that I trust I shall be excused giving it here:—"The vagina is never so soft, so dilatable, and capable of admitting the hand, as during the presence of an active hæmorrhage; and it is a mistaken kindness in the medical attendant, who, in order to spare his patient's sufferings under these circumstances, delays to introduce his hand until the hæmorrhage has ceased. The moment that this is the case, the vagina regains more vitality, sensibility, and power of contraction; the hand now experiences much more opposition, and excites far greater pain, than during the state of syncope." But for a fuller consideration of this subject, I cannot do better than refer the junior part of your readers to the admirable rules which my father has given.

[To be continued.]

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *June* 3, 1834.

Abscess	4	Hæmorrhage	1
Age and Debility	48	Heart, diseased	4
Apoplexy	7	Hooping-Cough	21
Asthma	31	Indigestion	1
Cancer	3	Inflammation	30
Childbirth	11	Bowels & Stomach	6
Consumption	80	Brain	4
Constipation of the Bowels	1	Lungs and Pleura	5
Convulsions	25	Insanity	1
Croup	2	Liver, diseased	17
Dentition or Teething	6	Measles	6
Diarrhœa	2	Miscarriage	3
Dropsy	14	Mortification	7
Dropsy on the Brain	13	Paralysis	4
Dropsy on the Chest	1	Small-Pox	5
Erysipelas	1	Spasms	1
Fever	7	Thrush	1
Fever, Scarlet	9	Unknown Causes	9
Gout	2	Stillborn	27

Decrease of Burials, as compared with the preceding week . . . 63

W. WILSON, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JUNE 14, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XXXVI.

DIFFICULT LABOUR.—INSTRUMENTS.

CRANIOTOMY.

Antiquity of the operation.—The first of all obstetrical operations practised, consisted in extracting the mutilated fœtus from the womb by cutting and sharp-pointed instruments, in whatever way it could best be accomplished. Thus, even Hippocrates has left us some observations on this subject; and Celsus is particular in his directions, having dedicated the twenty-ninth chapter of his seventh book to the mode in which a dead child may be extracted from the uterus by means of instruments. We may trace in the very expressions used by Celsus, the antiquity of the prejudice, that the voluntary efforts of the fœtus were the principal efficient cause of its escape; for he says, "Ubi concepit aliqua, et jam propè maturus partus intùs emortuus est, neque *excidere per se* potest, adhibenda curatio est."

The instrument he describes for this purpose is a cutting-hook, "uncus undique lævis, acuminis brevis;" but he mentions no means by which the cranial bones might be perforated, if necessary. From this we may infer, that distortions of the pelvis were not known to the Greeks and Latins; else, since he speaks of breech and shoulder presentations, the fœtus being dropsical, and the mode of decapitating it when transversely placed, he would not have omitted to notice such a serious and dangerous cause of difficulty. Nor can we be surprised at this; for I have already mentioned that rickets was first described by Glisson, in a treatise on the disease published in 1659. The habits of the ancients, indeed, were not favourable to the production of this formidable affection; their poorer population was mostly engaged in agricultural, or mechanical,—not in what may properly in this age be termed manufacturing,—pursuits; which latter, of all other occupations, most fosters the ricketty predisposition.

The Arabians, indeed, instituted the practice of perforating the skull occasionally; and Albucasis has given us a drawing of an instrument, designed both to open and extract the head. In later years, variously-fashioned perforators have been employed. Mauriceau's *tire-tête* consisted of a sharp-pointed, double-edged knife; Deventer used a long scalpel; others a crooked bistoury; Sir Fielding Ould invented an instrument which he called the *tenebra occulta;* Smellie, as I have already said, adopted the scissors, which, with Johnson's curve added, I employ myself.

Cases in which required.—I need scarcely again repeat, that although on some occasions we may feel justified in having recourse to harmless means for the purpose of delivery, before exhaustion has proceeded to any great extent, we are never warranted in taking the perforator in hand, unless driven to it by a dreadful necessity;—provided, indeed, there is any doubt as to the child's being still alive; or a well-founded hope exists of its being expelled by the natural powers.

I have before endeavoured to lay down a practical rule on this interesting subject, founded on actual measurement of the pelvis; and to draw a distinctive line between those cases in which it is possible for the head of a full-grown, mature, and well-ossified fœtus, to be extracted whole, and those others where a diminution in bulk by mutilation must be practised be-

fore the birth can take place; and as a principle, we regard a pelvis possessing less space than three inches in the conjugate diameter, unequal to the transmission of the skull entire. Nevertheless, it behoves us, even under such a diminished capacity, to wait as long as is at all consistent with the woman's safety, before we employ such deadly means.

Many accidental causes may occasionally oblige us to have recourse to the terrible alternative of sacrificing the child's life—such as hæmorrhage, convulsions, rupture of the uterus, syncope, and other anomalous states immediately and seriously threatening the mother's existence; but we never adopt it if a safe delivery be practicable by any other method.

It is, however, by far most usually found necessary where disproportion obtains between the head and the pelvic bones; and this diminution in capacity we generally observe at the brim, in the conjugate diameter,—as has been more than once exemplified in the progress of these lessons.

It is by no means necessary for the success of the operation, that the os uteri should be entirely dilated;—the wider, indeed, the orifice is opened, the less chance will there be of injuring that organ; but should it not have acquired a diameter greater than that of half a crown, we do not on that account shrink from its performance. In many cases I have been compelled to deliver by these instruments, when the mouth of the womb was not only undilated, but still possessed of considerable rigidity.

Mode of performing the operation.—After the perfect evacuation of the bladder,—the patient lying in the usual obstetric position, and two or three folds of napkins being placed under her, to receive the portions of cerebral matter as they escape,—two fingers of the left hand must be carried into the pelvis, and their tips brought steadily to bear against the most depending part of the fœtal skull. The perforator, having been previously warmed and greased, must then be directed along the groove between the fingers, until its extremity comes in contact with the head; a rapid semi-rotatory or boring motion must be given to the instrument, and it will soon be felt to perforate the bone, and enter the skull itself; it must then be pressed onwards until the studs prevent it passing any further. The fingers must be separated, and their inner edges placed against the rests of the instrument. The eye at the extremity of the lower limb must be held firmly by the finger and thumb of our right hand, while an assistant is required to open the blades, by raising the upper limb to the extent of about three inches. By this separation of the handles a laceration of more than an inch in length will be made in the fœtal skull. Provided the rests be well protected, there is little likelihood of any injury happening to the maternal structures; because all the cutting portion of the perforator is sheathed within the head itself. The instrument must afterwards be half turned round, without being withdrawn, and the edges directed respectively towards the sacrum and pubes;—again the handles must be separated in exactly the same manner, so that a crucial aperture may be formed in the bones. The projecting stops require now to be covered by the fingers with even greater diligence than before; for, independently of the space between the pubes and sacrum being so much less than the lateral diameter, there is greater danger of wounding the os uteri, the rectum, and particularly the bladder, while this second incision is being made. An aperture sufficiently large being obtained to admit the instrument more completely within the cranium, it must be introduced beyond the rests, and turned rapidly round in every direction, that the cerebral mass may be broken down as completely and speedily as possible. In this stage of the operation we shall find the scissors more efficient than Denman's perforator; because, by opening and shutting them, we can more perfectly destroy the organization of the brain, tear the vessels, perforate the tentorium, and even break up the cerebellum.

It is of much moment that the cerebellum should be destroyed, because it is from the base of the brain that the nerves take their origin; and it is possible that these organs may preserve their vitality, and perform their functions, although the principal part of the cerebrum be reduced to the consistence of pulp, or even evacuated. Instances have been known of the child breathing and crying loudly on its birth, after the head has been opened, and the brain partially extracted,—than which no accidental mischance, in the performance of any operation whatever, I should imagine, could produce a more lively thrill of horror.

A sufficiently large aperture being formed in the bone, the second part of the operation, extraction, must be commenced. This may be effected either with the craniotomy forceps or the crotchet; but, for reasons previously given, I generally use the latter instrument,—for I have found that, if employed with due caution, it is less dangerous than the craniotomy forceps, and,—if a firm hold be obtained,—equally or even more powerful. The crotchet, then, being introduced within the skull, must be fixed on the internal surface of the bone, wherever there is sufficient resistance to afford the necessary purchase: a finger of the

left hand must be kept close upon the head externally, exactly opposite the spot on which the extremity of the instrument is fixed within; by which means its sharp point is perfectly covered, and all chance of injury to the mother avoided. Extraction must be attempted by a steady power downwards, applied in the direction of the axis of the pelvic brim, which is in a line from the umbilicus to the coccyx.

If any jerking movement be had recourse to, the bone will certainly be broken, and our purchase lost. It is most probable, but not desirable, that after the continuance of exertion for some time in the same position, the point of the crotchet will perforate the bone, and be felt naked by our finger; it will then be of no avail to continue our extractive efforts, without changing the situation of our instrument, because it will break through its hold, and tear itself away,—but a fresh purchase must at once be sought at some other part of the cranial bones, and the same steady efforts used to overcome the difficulty. Should the instrument suddenly break through its hold, or slip from its attachment, the finger being constantly kept against its point will receive it, and injury to the os uteri or vagina thus be prevented.

If the contraction in the pelvis be considerable, we may expect that much exertion will be necessary, and much time will be spent, before the head passes through the brim; and we must not be disappointed in finding the bones break constantly, and piece after piece come away. The loosened portions must be carefully removed, either by the fingers or a pair of small forceps contrived for the purpose, and the naked edges still remaining within the vagina studiously covered by the scalp. You will occasionally in this manner bring away the parietal, the greatest portion of the frontal, and parts of the occipital bones; and the orbits, or the foramen magnum, will then afford a strong and most valuable hold either to the crotchet or blunt hook. If you find these instruments slip, or the crotchet break through the different bones too easily, you may have recourse to the craniotomy forceps, being most careful not to inclose any portion of the os uteri between the blades, and remembering the other dangers to which I have before adverted. A tape should be tied round the handles of these instruments, to prevent their slipping, and to increase their power.

Another great objection to the craniotomy forceps consists in the difficulty of their re-ajustment when they have broken from their previous hold; for it is not always easy to find a fresh purchase on which they can be applied without injury. The guarded crotchet of Davis will sometimes, perhaps, be useful; but I cannot help thinking the best security the patient can experience is in our own caution, and the best guard we can employ, our own finger.

It happened to me, on two occasions, to destroy the skull so entirely, that both the orbits and the foramen magnum had given way, and nothing was left which would afford a hold to any of my instruments. In these cases I delivered eventually by turning, not without subjecting the patient to considerable hazard.

It is recommended by some practitioners, indeed, that we should endeavour to break up the cranial bones on all occasions where this operation has become necessary, and take them away separately, as soon as we can accomplish it. With these instructions I cannot coincide; because I have found that when they have been easily separable from each other, in consequence of a high degree of putrefaction having taken place, the operation is both more difficult and more dangerous than when they possess a firmer texture, and offer more resistance. As the head collapses in its passage through the brim, the brain oozes through the opening we have made, and the appearance of cerebral matter externally is almost a sure sign of the head's descent: it will be received on the napkins previously applied, which should be removed occasionally, and others substituted; care being taken that none of the pulpified mass be strewed over the bed furniture, or fall upon the floor.

There can be no necessity, in the generality of cases, for the introduction of a *spoon* within the cranium, or any other kind of scoop, for the purpose of extracting the brain; for if that organ be broken down, and the membranous septa within the skull be divided, it will readily escape when our extractive effort is applied.

Osborn advised that, after the perforation was effected, some hours should be allowed to elapse before extraction was attempted, that time might be given for the bones to collapse; and that the diminished head might accommodate itself to the irregularities of the pelvic apertures.

From this recommendation, also, I dissent, because, if we have delayed operating until symptoms of exhaustion have appeared, we cannot expect that the uterus will retain sufficient power to accomplish the delivery, or even to propel the head into the pelvic cavity:—besides that in so acting we are lessening the chance of recovery which the patient enjoys, by adding to her present sufferings, and allowing her system to become hourly more depressed:—we are rendering the operation more difficult, by losing the advantage of whatever energy may still remain to the uterus; and perhaps, also, by permitting putrefaction

to take place; when the bones will be more or less loosened from their attachment to each other, and the purchase they ought to afford necessarily weakened. If, then, we subject our patient to such an increase of danger, and render the operation so much more difficult,—especially as we have inflicted the *summa injuria* upon the infant,—what advantage can we gain by delaying the completion of the delivery?

Such a proceeding may certainly be advisable in those more rare instances of extreme deformity, where not the slightest hope exists of the head being expelled whole, and in which early perforation is had recourse to, under the conviction of its absolute necessity. We may then, while the powers are strong and unimpaired, wait for a few hours with impunity, or probably even with benefit; but, as a general principle, the practice will be injurious.

By some, again, we are recommended to seek for a suture or a fontanelle, and to perforate the head at one of those spaces, because the instrument more readily pierces the membrane than the bone. My custom is, to make the opening at the most depending part of the head,—that which is most readily touched,—because there is less danger of injuring the os uteri—because the point of the perforator is sufficiently sharp to drill a hole through the bone itself—and because, if we carry it to one side, it is very likely not to enter the head at all, but to run up between the skull and the scalp, merely separating the one from the other.

On the head being born, it must be enveloped in a napkin; and it is most probable that a continuance of our efforts will be required for the extraction of the shoulders. If much difficulty exist, we shall be assisted by placing another napkin round the neck; and traction must be used in the direction of the axis of the brim, viz. in a line tending towards the coccyx. It may often even be necessary to introduce a small blunt hook under each axilla in turn, to facilitate the exit of the shoulders; and sometimes, also, to perforate the chest or abdomen, before delivery can be completed: and in cases of extreme deformity,—which, however, may still admit of the extraction of the fœtus *per vias naturales*,—it may be found necessary to turn the base of the skull sideways, before it will pass the narrowed apertures.

Baudelocque suggests that after extraction we should inject the uterus, to wash away any particles of brain which may be lodging in that cavity, or in the vagina: this, also, I deem useless; because none of the cerebral substance escapes into the uterus, and what lies in the vagina must be perfectly wiped out by the passage of the fœtal body: as forming an unnecessary complication of the operation, therefore, the advice should be rejected.

Signs of the death of the fœtus.—From the creeping shudder which must insensibly steal over the mind when we are about to perform the operation of craniotomy, it is highly desirable,—both that our feelings should not be wounded without cause, but especially as a guide to our practice,—that we should be able to determine whether the infant be dead or still living. This knowledge it is by no means easy, on all occasions, to obtain; but many signs have been noted as indicative of the loss of fœtal vitality—many of them most equivocal, but some few tolerably certain; and these I shall proceed to notice.

Those signs on which we can place the least reliance are—

First—The loss of fœtal motion.
Secondly—A sense of dull weight experienced by the mother in the uterine region.
Thirdly—A sense of coldness in the womb.
Fourthly—The meconium coming away under a head presentation.
Fifthly—A putrescent fœtor in the discharges.
Sixthly—Discharge of flatus from the uterus.
Seventhly—Want of cerebral pulsation.

Those which are much more satisfactory, and which, indeed, with some limitations presently to be mentioned, may be considered conclusive, are—

First—Loss of pulsation in the funis.
Secondly—Desquamation of the cuticle.
Thirdly—Looseness of the bones and breaking up of the texture of the cranium.
Fourthly—Emphysema of the scalp.

We often hear it advanced, that the child must be dead, because its movements have not been felt for a length of time; and the mother herself will be persuaded that such is the case; but it by no means follows that, because the motion should not have been experienced for a number of hours even, the child should therefore have lost its life. I have already mentioned my belief, that, under the compression which the brain suffers in labour, the fœtus is thrown into a state of partial stupor; during the continuance of which it is incapable of moving its limbs, and consequently cannot make any impression on the mother's sensibility. The brain, indeed, will bear with impunity much greater pressure before birth than after breathing life has commenced, because the action of the heart is less dependent on the nervous energy being sound and unimpaired, than is the function of the respiratory organs: compression, then, may have occurred to such an extent as to take away all power of motion, with-

out so far interfering with the function of the brain as to suspend the heart's action. M. Merat, too, in the Dict. des Sciences Médicales, has shewn, by many highly-interesting experiments, that the vitality of the heart in the fœtus is much less dependent on the perfection of the nervous system, than after breathing life has been continued for some time. Besides,—putting out of our consideration the probable effect of pressure on the brain,—if we recollect how the child's limbs are cramped after the membranes are ruptured, we shall no longer wonder that they are inactive. Its body is firmly embraced by the contracted fibres, and all power of motion paralysed.

A dull sensation of heaviness in the uterus has been enumerated as a second sign. It is alleged, that as long as the child is alive there is a certain degree of buoyancy about it which is lost when it is dead; and that a feeling of weight is consequently experienced. But this at the best is very questionable.

A sense of coldness in the uterus is given as another sign. It is supposed that so long as the child is alive, it forms heat for itself, through the medium of its own circulation; but when it is dead, it abstracts heat from the mother's body, and therefore that she must feel a sensation of cold. It is very possible that this position may be correct, but it does not follow that the inference is a true deduction, and—inasmuch as these signs are all dependent upon the mother's sensations, and consequently upon her sensibility, and as we cannot depend upon the accuracy with which she describes her feelings — we could not rely upon their infallibility, even were they much more positive than they really are.—How is it possible for a woman suffering under the violent agonies of protracted labour, to talk calmly about a sense of a weight at the lower part of her person; or to speak of coldness, when the heat of her body is raised two or three degrees, perhaps, by the exertion she is sustaining?

The coming away of the meconium, when the head presents, is assigned as another evidence of the child's death. It is assumed that the bowels do not naturally evacuate themselves into the uterus; that their contents cannot be squeezed out by the action of the uterine fibres; but only pass in the last death-struggle of the child, or after the sphincter has lost its opposing power. This does not always hold good, of which I have myself had proof: besides, a mistake may easily be made on this subject. I have already mentioned, that, generally, a brownish, olive-coloured discharge, escapes from the uterus in large quantity, under lingering labour, which has been looked upon as meconium mixed with the liquor amnii; and I have known such an appearance adduced more than once as a proof that the child was dead, when it has afterwards been born strong and healthy.

The discharge possessing a putrid odour, is said to be another evidence of the infant's death. If the child be putrid, unquestionably the discharges will have a fœtid smell; but it is not necessary that the child should be putrid because the fluids escaping from the uterus possess an unpleasant fœtor.

The discharges may have been pent up within the womb, owing to the child's head being impacted in the pelvis; and a few hours will be sufficient to induce putrescency. Nay, the liquor amnii has been frequently observed, at the commencement of labour, to possess a putrescent smell, when the child has been born vigorous. I grant that the odour arising from mere putrid discharges differs considerably from that emanating from the body of a fœtus dead in utero; and it is very possible that a person, much engaged in operative midwifery, might be able to discriminate between them;—of the two, that arising from the death of the child is by far the most sickening: it is, indeed, the most nauseous fume that can possibly assail the nostrils. On some occasions I have with difficulty restrained myself from vomiting, while extracting a putrid child; although I am far from fastidious in such matters.

A discharge of flatus from the uterus may be regarded exactly in the same light as the appearance of putrid fluid. This gas is generated by putrescency, and will often escape, on the opportunity being given to it, when the finger is carried up to the brim of the pelvis by the side of the child's head. This, therefore, must be ranked as another most equivocal symptom.

Nor is the inability to discover pulsation through a fontanelle more conclusive. Even at the commencement of labour, when the brow presents, it is very seldom indeed that we are able to distinguish the pulse of the cerebral vessels: how much more difficult, then, must this means of diagnosis become, when the smaller, posterior fontanelle, is the depending part; and especially when the scalp is tumid and puffy, owing to the collapse which the bones are suffering.

The symptoms more to be depended upon, are, *first*, the funis having prolapsed before the head of the child—having remained without pulsation for a considerable time, and having become cold and flaccid. Here we have a positive proof that death has taken place. But does it necessarily follow that the funis belongs to that child whose head is at the pelvic brim? Is it

not possible that there may be twins in utero? Is it not possible that both the bags of membranes may have given way, and that the funis of the second or uppermost child may have prolapsed by the head of the first? If so, might not the first child be alive, though the second was dead? It is very *possible* that such should be the case; but, to produce such an accident, three circumstances must concur—there must exist a plural gestation; the liquor amnii of both children must be evacuated; and the funis of the second must be preternaturally long, to have so dropped down. It is a most unusual occurrence for the membranes of a second child to burst before the first is born; and the prolapsus of the funis belonging to that child, at the same time, would be such a rare complication of chances as to remove the case entirely out of all calculation: so that we may safely regard the fœtus as dead, if the pulsation in the prolapsed funis have entirely ceased for the period of thirty or forty minutes.

Another almost unequivocal sign is desquamation of the cuticle. If, under a head presentation, we can bring away, between our fingers, three or four hairs having some of the cuticle attached to their roots, we may be pretty well assured that the child is dead. But even this is not an infallible sign, for there are cases on record to prove the contrary. A slough may have occurred in the scalp, from long-continued pressure while the child was still alive; and from such a spot the hair and cuticle might be removed without difficulty. A case of this kind is furnished, I believe, by Dr. Davis; and Dr. Orme met with another, where, in consequence of cutaneous disease (probably syphilitic), the cuticle easily desquamated. In his recent work, also, Kennedy has recorded another similar instance, in which there existed "a livid discoloration of the whole body, and a complete denudation of the cuticle, to the extent of several square inches, from different parts of the surface; while the remainder of it was so easily separable as to be removed by the friction of the clothing;" and yet this child was born alive, and survived its birth several hours. Such accidental occurrences, however, are most unusual; and if, at the same time, the discharges were very fœtid, little doubt could remain as to the child being lifeless.

A third sign is the breaking up of the structure of the head, so that when we touch it the scalp feels loose, as if it were enclosing a number of shells; owing to the brain being pulpified, and the membranes connecting the bones having become softened, and having partly lost their uniting power. Together with this breaking up of the structure of the head, we usually also observe the last symptom — emphysema of the scalp; which produces a crackling sensation under the fingers: and if these indications be accompanied by the peculiar fœtid odour I have just mentioned, we may be sure that life is extinct. These three occurrences, indeed, can only take place when the child has been dead some time, and putrefaction has advanced to a considerable height.

I have not thought it necessary to dwell upon some other symptoms which have been noticed as evidencing the child's death: such as vomiting, shivering, lividity or pallor of the face, discoloured and sunken eye, offensive breath, or extreme languor on the part of the mother; because it must be evident that all these occurrences may take place from many causes entirely referable to the maternal system, and perfectly independent of any impressions derived from the state of the fœtus. It would be a waste of words, therefore, to canvass their separate merits.

But I must not pass over so abruptly another means of diagnosis, only lately introduced to the notice of the profession—viz. auscultation; a method, the value of which I have already confessed myself but partially acquainted with; but which I nevertheless consider must prove a great practical acquisition, provided, indeed, its merits are at all deserving the high encomiums bestowed upon it by its advocates. If, by the simple application of the ear or the stethoscope to the abdomen of the parturient woman, we can decide, in a doubtful case, on the present state of fœtal vitality, we shall be gaining the greatest possible advantage, without subjecting the patient to the least pain, danger, or inconvenience; and even without shocking, in the slightest degree, the most delicate or sensitive mind.

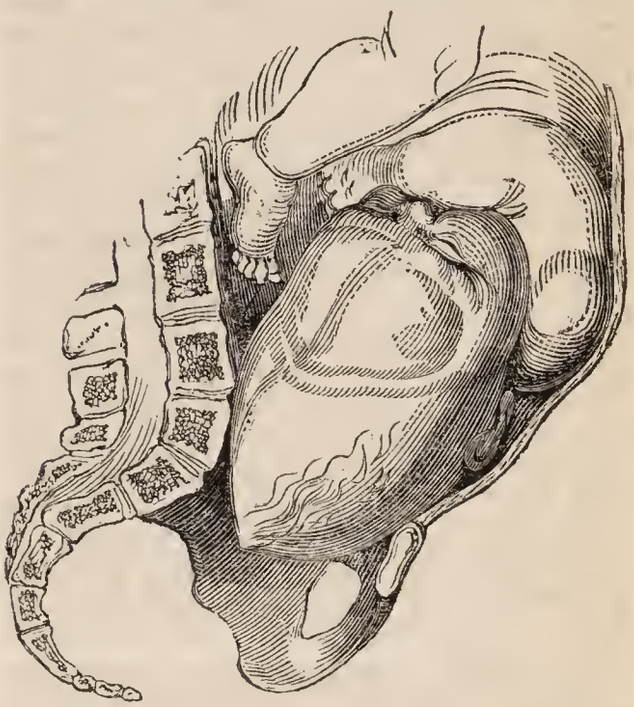
From the observation I made on this subject in an early part of the course, you are aware that this instrument has been applied to the purposes of ascertaining the existence of pregnancy; and you are not strangers to the name of Dr. Kennedy, who has stood forward as one of its staunchest supporters. This accomplished physician has not overlooked the benefits to be derived in protracted labours from determining whether the infant be still living; and he has given us some cases to prove that the method he recommends far surpasses all other means of diagnosis, and of itself will be sufficient to set the question at rest. Although, however, such a warm advocate, he is by no means a bigotted one; he treats the subject in a fair and candid manner, and is desirous of convincing us by argument and proof,

rather than compel us to agree with him by empty assertions. Those of you who have studied the stethoscopic sounds, under various states of thoracic disease, will have, I should imagine, little difficulty in detecting the pulsation of the fœtal heart towards the close of pregnancy, or even under active labour; and I recommend this study to you all, as a useful acquisition, in addition to your other medical acquirements.

Some practitioners, indeed, think it useless to form a diagnosis on the state of fœtal vitality, since we never have recourse to the operation of craniotomy except where delivery has become requisite, and where the perforator affords the only choice. This is certainly true as a principle, but exceptions to the general rule will constantly occur. On many occasions we should be inclined to endeavour to deliver by the forceps or vectis, if we had any suspicion of the infant being still alive, although, perhaps, in so doing, we might subject the woman's structures to some hazard; while on others we should be perfectly warranted in lessening the head, although the case might possibly be terminated by one or other of those instruments—provided, indeed, we are quite certain of the child's death.

Although, then, it is often difficult to distinguish whether the fœtus be still living before the operation is performed, that knowledge is easily gained when perforation is accomplished; for if the heart be acting, as soon as the cerebral vessels are ruptured, a quantity of fluid blood, partly arterial and partly venous, will escape externally, before any portions of brain appear; on the contrary, if the circulation have quite ceased, no *flow* of blood will take place, but a number of small clots will come away with the cerebral matter, as it oozes out on the application of our extractive efforts. We shall observe also, on the birth being perfected, that the whole person, but especially the face and lips, present an exsanguined look, in consequence of the cuticular vessels, as well as those supplying the internal parts, being drained of their contents: and this bloodless appearance has led me to suppose, that death under these circumstances occurs as frequently from hæmorrhage as from the injury inflicted on the brain itself.

This remark, however, is of no importance, practically, in regard to the case under treatment; because, whether the child be dead or not, the act cannot be recalled: the observation is only valuable for our own satisfaction—to bring peace to our mind, and soothe our excited feelings, if we should fortunately ascertain that we have not ourselves been the instruments of death, but that it had occurred from the hand of nature;—and to determine the correctness or fallacy of our previously-formed opinion.



In the above plate the head is represented as offering itself at the brim of a contracted pelvis, the bones overlapping each other to a considerable extent.

CLINICAL LECTURES ON MEDICINE,

Delivered at the Hôtel Dieu, Paris,

By M. Chomel.

[From the "Leçons" published under his own immediate inspection.]

ON TYPHOID FEVER.

The diseases described by different authors, and those which I have myself sketched, in my Treatise on Fever, under the name of severe continued fever—whatever be the particular form displayed—(whether inflammatory, bilious, mucous, adynamic, ataxic, or nervous) are but varieties of one affection, which has received many different names: in fact, however dissimilar in appearance, they possess certain characters common to all, and are especially linked together by a series of anatomical lesions which are not met with in any other disease.

SYMPTOMS AND PROGRESS OF TYPHOID FEVER.

Mode of invasion.—The onset of typhoid fever is not always made in the same manner. In a certain number of cases the phenomena peculiar to the immediate

attack are preceded by various preliminary symptoms; but more generally it is sudden, coming on unexpectedly, during a state of health apparently the most satisfactory, and without any kind of warning.

Some disturbance of the relative and nutritive functions sometimes precedes the attack; but these are not peculiar to this disease, as they likewise precede others—particularly the eruptive fevers. Sometimes, before the attack of typhoid fever, a change is perceptible in the expression of the countenance, which becomes more sombre and oppressed, with a diminution in the capacity for mental labour: there is also loss of strength for some days, or even for weeks, with wasting; and the individual feels that he is threatened with some formidable disease. [Various other common premonitory symptoms are here detailed.] Of 112 patients received into the clinical ward of this hospital,—in 73 the invasion has been sudden; in 39, it has been preceded by certain warnings, as above described. The phenomena of the actual invasion appear with most intensity where there have been premonitory symptoms; as where the attack is sudden. The most common circumstance is for the patient to suffer from intense headache, especially on first rising in the morning: the physiognomy is then rapidly changed, and it is by no means rare to observe, even from the first day, that kind of stupor which some have described as belonging to a more advanced period: the muscular strength is so much impaired, that they cannot walk without difficulty; and when we ask patients here how they have come, the answer almost always is, "In a coach;" or, "Supported by two friends, because I could not walk alone."

Diarrhœa is one of the most important phenomena of the invasion: in the greater number of cases it comes on by the first or second day, but sometimes later. There is also generally some pain about the abdomen; and this, when present, completes the diagnostic marks. In order to give a clear account of the disease, I shall divide it into three parts, of seven days each; for though its duration is not invariable, it is yet sufficiently common for general purposes.

First period.—The symptoms which usually become developed during the first period are headache, which is extremely common — prostration of strength, and heaviness, or stupor—diarrhœa—meteorismus—sensibility of the abdomen, particularly in the right iliac region—epistaxis—and an eruption, generally called *typhoid eruption*. One of the most remarkable circumstances is the want of expression in the countenance, or rather the expression of apathy and indifference. It is soon apparent that the intelligence has suffered a great diminution, yet delirium rarely comes on before the second period, or at all events, the end of the first. The patient generally lies on the back, being unable to sit up without giddiness. There is continued, or nearly continued, insomnolence. The headache, which almost always attends the onset of the disease, remains, with great severity, during the first week, and almost always yields at the end of this period, sometimes sooner, particularly under proper treatment; occasionally it is the prominent symptom throughout—[The common symptoms of typhoid fever are here attached, after which M. Chomel continues:]—and this symptom, connected with the meteorismus, and with respect to which authors are silent, is the gurgling under pressure in the lower part of the abdomen, particularly about the right iliac region. This phenomenon appears to me to be as uncommon in other diseases as it is frequent in this. It is sometimes met with in the first stage, but still more frequently in the second or third.

The sensibility of the abdomen is another symptom which calls for attention. The pain is seldom acute; and generally the patient does not complain, except when pressure is actually made, and that, too, pretty firmly. Its seat, also, varies: in some it is limited to the right iliac region, but in the greater number it occupies the whole hypochondrium, and sometimes extends over the whole abdomen. In this last case it occasionally is associated with painful conditions of other parts, as the chest, back, or surface of the belly generally. Some physicians pretend that they have observed the temperature of the abdomen to be higher than that of the rest of the surface; but this is not the case. I have often directed my attention to this point, and compared the temperature of the belly with that of other parts, but without discovering the alleged difference.

The respiration also exhibits some modifications in the course of this fever worthy of notice. During the first days, and sometimes even from the very beginning, we find on both sides of the chest a *râle sibilant*, which occupies the whole extent of it, but which is more marked at the lower and posterior, than at the other parts. The cough is rarely in proportion to the râle, and the expectoration is usually scanty, transparent, tenacious, and viscous, adhering strongly to the vessel. The breathing is rarely perceptibly impeded. Sometimes the meteorismus, when very great, causes such difficulty of breathing as to threaten suffocation.

The last symptom which occurs in the first period, and this at its termination, is the eruption. Of 54 cases in which I observed this phenomenon, in 1831-32, it appeared only twice on the sixth day; in all the others it did not appear till the second or even the third week.

It is very rare for death to take place during the first period. Of 42 individuals who died of typhus fever in the clinical wards of this hospital, only one sunk within the first week.

Second period.—This stage of the fever often varies in the symptoms. During the first portion (usually between the seventh and ninth days), a peculiar eruption appears, which is proper to the disease, and consists of small spots of a rose colour, disappearing on pressure, from half a line to two lines in diameter, of a round form, without elevation, or scarcely elevated, scattered over the abdomen, sometimes over the chest, more rarely on the thighs, arms, or forearm. The little spots are the more marked as the skin is whiter; in those where the skin is brown, they are sometimes difficult to distinguish. Their number can scarcely be determined, because they are not always equally conspicuous; but, to characterize the typhoid disease, they ought to amount to at least fifteen or twenty. If there be only two or three, but little importance can be attached to their presence. The eruption does not take place on all parts at once; it sometimes happens, that after having seen, during three or four days, some rose-coloured spots on the abdomen, but in such small number that they have been looked upon as of little importance, they all at once become very numerous on the chest, belly, sometimes also on the thighs, arms, back; and even, though more rarely, on the face. The duration of the eruption is not always the same; in some cases, after two or three days, no more remains—at other times they last twelve or fifteen days; but in the latter case they probably consist of successive crops, for each spot is visible only for about three or four days, and sometimes less; and at the end of this time they disappear, having previously become fainter. These spots at most give to the skin but a slight degree of elevation, and never exhibit either a conical prominence nor any vesication at the apex. They rarely appear before the eighth day of the attack. As to the rest, the following are the results of what was seen in our wards in 1830-31, and 1832:—Of 70 cases of typhoid fever, where the phenomena relative to the eruption were carefully watched, in 16 no trace of any spot could be found throughout,—making about one-fourth of the entire number. As to the date of its appearance, out of 25 in whom the eruption supervened after their admission, in 2 this took place between the 6th and 8th day; in 13, between the 8th and 15th day; in 7, between the 15th and 20th day; in 4, between the 20th and 30th day; in 1, on the 37th day. Besides these, there were 29 in whom the eruption was present when they were admitted; but in them the dates of its having appeared corresponded to the above; so that, of 54 patients who had this eruption, none had it before the sixth day, and in two it appeared so late as the thirty-sixth. These observations correspond with those of M. Louis.

If these results be taken in connexion with another most important fact—namely, that the eruption is as rare in other acute diseases as it is common in typhus, and that even when it does occur in other diseases, that it is always less abundant, it will be easily perceived why I attach so much importance to it in the diagnosis of this fever.

These spots are easily distinguished from petechiæ and flea-bites; because in the latter there is extravasation of blood on the surface of the skin, and the colour, instead of being diminished, is rendered more conspicuous by pressure, owing to the effect which this produces on the surrounding skin. In the spots of typhus, on the contrary, the redness disappears altogether, as in erysipelas, where there is evidently sanguineous congestion, and where the pressure being removed this instantly re-appears again.

Another kind of eruption is frequently met with towards the end of the second period—viz. that which has been called *sudamina*. It consists of very small transparent vesicles, chiefly in the neighbourhood of the groins and axillæ, though sometimes extending to the trunk and limbs. This eruption, though much more common in typhus fever than in other diseases, is, nevertheless, by no means so peculiar to it as the preceding, and therefore is of inferior importance in reference to diagnosis. Another phenomenon connected with the surface, is the readiness with which parts subjected to pressure, or to which irritants have been applied, run into gangrene.

[The lecturer here enters into various details on this subject, as well as on the effects of prostration and oppression of the nervous system; which, however, are well known to the English reader.]

The diarrhœa continues, although sometimes to a less extent than during the first period; but the stools now frequently become involuntary. It is at this time also

that intestinal hæmorrhages are seen to occur in a small number of cases:—sometimes these are to an inconsiderable extent, but at others they are violent, and occasionally cause sudden death. In some patients they are repeated every day during a considerable time. The attendant ought to examine the stools himself during this period; for when the blood has remained some time in the intestines it undergoes either a kind of digestion or of decomposition, which changes its appearance, reducing it to a dark, pultaceous-looking mass, the nature of which may be uncertain. These intestinal hæmorrhages are very important with respect to diagnosis in typhus fever, in which they are much more frequent than in other acute diseases: they are, in relation to the second period, what epistaxis is to the first, and may throw light on doubtful cases. The meteorismus offers but few modifications during this period,—the only important one, in fact, is where it augments so considerably as to pass into tympanitis: at this degree it is apt to aggravate the disease by embarrassing the breathing.

The disease seldom shows any new appearance, but it is usually a little diminished.

In some cases death supervenes during the second period; viz. from the eighth to the fifteenth day;—thus, of 42 patients who died in the clinical wards, 9 sank in this interval; that is to say, during the most acute stage of the disease.

Third period. — The phenomena of this period vary according as the progress is towards convalescence or death. We have already seen, that of 42 patients who fell victims to this complaint, only 10 died during the first period, all the rest being carried off in the second or third. It appears to be almost always during the last period that the febrile movement occurs, whether favourably or fatally.

When the termination is about to be favourable, the severe symptoms lose their intensity: if the patient answers questions put to him, he does so with more promptitude, and his eyes are voluntarily directed towards the person who speaks to him: this first look—this expression, which shows that he begins to rouse from his stupor, and take an interest in his own condition—this attention with which he seems, as it were, to interrogate the physician—are even signs of improvement. In other cases the kind of coma in which the patient has been plunged is changed for quiet sleep, on waking from which he is found to have recovered a portion of his intelligence.

[M. Chomel here proceeds to enumerate the symptoms indicative of a favourable termination.]

In 68 cases of recovery, where the period of its invasion and the day on which the improvement commenced, was manifested by one or more of the signs which have been mentioned, I find that it has taken place—

In 1	patient on the	8th day.
1	.....	9th.
4	.....	12th.
3	..... from	12th to 14th.
10	.....	15th to 16th.
15	.....	17th to 20th.
14	.....	21st to 25th.
11	.....	26th to 30th.
8	.....	31st to 40th.
1	..... on the	45th.

The commencement, then, of the third stage of fever is extremely variable, as the characters which mark it in the greater number of cases—that is, in those which recover—may present themselves from the 8th to the 45th day.

After having described the more common trains of unfavourable symptoms, M. Chomel continues:—In other subjects, we see, in the midst of the ordinary course of the disease—most frequently in the third period, or during the convalescence—violent and unexpected symptoms suddenly supervene. The patient is suddenly seized (when the state of the intellect is such as to admit of them being described) with very acute pains in the abdomen, with sense of fainting, followed by nausea and vomiting; and this, whatever may have been the previous form of the fever. All the other symptoms, as it were, disappear before the severity of the new phenomena which indicate the existence of peritonitis, which is at first partial, but afterwards becomes general. The pulse is small and thready; the abdominal pains continue with great intensity; and, in fact, a very severe inflammation of the peritoneum is set up, without any appreciable cause, but which is the result of the tunics of the bowel being perforated, and feculent matter effused. This perforation of the intestines is the most formidable accident which can supervene in the course of typhus fever, for it almost invariably leads to the patient's death. The numerical results, obtained during five years in the clinical wards, differ a little from those published by M. Louis. He states that he observed the perforation eight times in 55 patients who died of typhus; which would make the proportion of these accidents, to the total number of those who die of the fever, as one to seven. In the cases met with in the clinical wards here, amounting to forty-two postmortem exa-

minations, there have been only two in which perforation of the bowel has existed; giving a proportion of one in twenty-one. But by joining both sets of observations, and taking the more extended basis thus afforded, we arrive at a common medium of one in ten, which is probably nearer the truth.

A complication which not unfrequently arrives in the course of the third period is erysipelas of the face. Probably it is more frequent than perforation of the bowel; at least it has been so here; four out of 130 having been thus affected. All of them died.

With regard to those crises and critical days, of which so much used to be said, there certainly are some cases in which some of those evacuations which, in the times of humoral pathology, were held to be critical, are observed; but these almost always follow some days after the improvement has begun, and are new proofs of the convalescence—not causes of it. The only phenomenon, the appearance of which has seemed to me to precede the cessation of the febrile symptoms, and which might therefore be called critical, has been the development of abscesses externally; but the examples of this are so rare (only 6 in 80), that they cannot be made the basis of any theory so extensive as that of crises, as adopted by the ancients. As to critical days, we have seen by the table above given that there is no period between the fifteenth and thirtieth days at which the favourable change may not take place; and we are thus obliged to acknowledge that the doctrine of critical days has even less foundation than that of critical discharges.

Changes which the blood undergoes during life.—Hitherto this part of the investigation has been but unprofitable; and notwithstanding the daily progress of chemistry, the part which relates to the animal fluids has been so little advanced, and what is ascertained has so little relation to the phenomena of typhus fever, that little can be founded upon them. Confining myself to the physical characters of the blood, I shall relate what I have observed.

Of 30 cases where blood has been drawn from the veins of typhus patients,—where it was necessary to draw blood, and where exact notes were kept,—in 6 the clot was firm, and covered with buff; in 20 it shewed no buff, but was firm; in 2 it shewed a light clot on the surface, but was soft beneath; in 2 the blood was completely soft and broken down. All these bleedings were practised in the first stage, or at the commencement of the second—none during the third. In none of the six cases, where the clot was covered with a buffy coat, did this prevent the thickness nor colour which it ordinarily shews in pneumonia, in pleuritis, or in acute phlegmasia.

We may conclude from these facts, that the blood taken from a vein during a typhoid affection, does not offer an appreciable alteration, except in a very small number of cases (four in thirty); and that it rarely presents the characters which are proper to it in the acute phlegmasiæ, with which the febrile action and the intensity of the symptoms might appear likely to confound the disease.

Of the convalescence.—When the disease terminates in a favourable manner, the patient does not immediately pass from the state of danger which we have been describing to a state of perfect convalescence: the recovery is often extremely slow; and it is not uncommon to see patients, after the severe symptoms have disappeared, continue for one or even two months in a state of painful and slow convalescence. Sometimes, at the commencement of recovery, the hunger is so acute that the worst consequences would result if we were to indulge the wishes of the patient. When nothing unfavourable occurs, the patient advances gradually towards health, every day adding something to his strength. But besides the avidity with which he is wont to seek for food, and the consequent imprudences which he commits, as well as by exposure to cold or damp, there are other less obvious causes which often excite much anxiety.

I may mention first among these, that œdema of the lower extremities which is often observed at the end of acute diseases which have been tedious, and which, in fever, frequently delays the convalescence very much. Another accident, more severe in appearance, which is sometimes, though more rarely, observed, is a derangement of the intellectual faculties; which may consist in a kind of mania, which disappears when the patient returns to his wonted occupations and amusements, but which may also become more formidable. In 1831, there was a patient in Lazarus' Ward, a girl, whose convalescence began about the twenty-sixth day of a fever which had been extremely severe, and accompanied by delirium during twelve days. After eighteen or twenty days of convalescence, it was observed, without other apparent cause, and without increase in the frequency of the pulse, that a decided change took place in her character; she became vicious and cross, and her reason was soon so much impaired that it was necessary to send her to the *Salpêtrière*; whence she was discharged cured in a fortnight.

ON PERFORATION

OF

STRICTURES OF THE URETHRA.

With Cases.

By R. A. Stafford,

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[Continued from p. 288.]

The Case of Admiral Sir Herbert Sawyer, K.C.B.

About seventeen years from the present time (Nov. 1833), Sir Herbert, who is now 70 years of age, met with the following accident:—He was driving his family to pay a visit to a friend, when, having occasion to get off from the box of his carriage, his foot slipped, and he fell with the whole weight of his body on the circle of the wheel, upon his perineum. The shock was very considerable, and he suffered excessive pain. A few minutes afterwards he felt that his drawers and boots were filled with what he considered to be urine, but on examination he found it to be blood, flowing from the urethra. He was immediately placed in the carriage, and driven as fast as possible to his friend's house. On arriving there he was much exhausted, having lost an immense quantity of blood. The family surgeon was sent for, who immediately employed remedies to arrest the hæmorrhage. It continued, however, more or less for four days, when it ceased entirely. The patient, of course, was much reduced, and his urine passed away during the whole time with the most excruciating agony.

About a month from the accident, Mr. Cline was sent for, who endeavoured to pass instruments into the bladder, but failed in the attempt. From this time, as the urine continued to pass tolerably free, nothing more was done until a year or two afterwards, when Sir Everard Home was consulted. Sir Everard tried to pass instruments, but he could not succeed. He then employed the caustic, but without effect. Since that period, most of the eminent surgeons of the present day have been consulted, but none could accomplish the introduction of an instrument into the bladder.

Although the stream of urine has never been since the accident as large as natural, yet the obstruction in the urethra has not caused *extreme* suffering until within the last two or three years. Since this time Sir Herbert has had constant incontinence of urine, with frequent attacks of complete retention, which produced such distention of the bladder that it occupied the whole region of the pubes, and reached as high as the umbilicus; hence, had not relief been obtained, the bladder must have been punctured. The urine, for more than the last two years, has continually drained from the urethra only drop by drop, into a receptacle attached to the thigh, made for that purpose. From the difficulty of keeping this machine in its proper position, and from a constant spasmodic pain at the neck of the bladder, he was unable to remain longer than three or four hours together in bed, and when there, he seldom got any sleep; he was therefore under the necessity of sleeping in a chair, or in any position most convenient, for more than half the night. This state of suffering, day after day, and night after night, for so long a period, had so worn him, that he became enfeebled, depressed in spirits, restless, and constantly drowsy. In such a miserable state of existence life could be but of little value to him; and had it not been for his fortitude, his great resignation of mind, and the placidness of his temper, he must long ago have sunk under so heavy an affliction.

On the 11th of last September, 1833, I received a letter from him, requesting me to go to Bath to give my opinion on his case. On my arrival there, I found him much in the state already described, excepting that in addition he had an extremely irregular pulse, intermitting every second, third, or fourth beat; and he had also anasarca of the lower extremities. He informed me, likewise, that he had had dropsy three or four years ago, and that fluid had collected in all the cavities, so much so that he was obliged to be propped up in bed; from this he recovered, under the care of Dr. Blackall, of Exeter. Cataracts, also, had formed in both eyes, which a year previous had been successfully extracted by Mr. Alexander.

In consultation with Mr. Norman, of Bath, I examined the urethra, and found, as every other surgeon had done before me, that a bougie or catheter could not

be passed further than the bulb. At that part its progress was arrested, and it appeared as if its point rested against a solid mass, which, by pressure, could be forced on before it. An attempt was made to take a cast of the part, by moulding soft wax on the point of a bougie, but no impression could be obtained. It seemed to enter a blind pouch, whose extremity was perfectly smooth. Under such circumstances, both Mr. Norman and myself agreed that it would be better to attempt to perforate the stricture; for it was too evident, from the symptoms under which Sir H. laboured, and the worn-out condition in which he was, that the first attack of retention of urine he again had, would either cause his death, or render it necessary to puncture the bladder, which, in his enfeebled state, he could but ill bear. As the operation of perforation of stricture had never been in a single case attended with the slightest injury, he consented to have it performed; and I gave it as my opinion, in which Mr. Norman concurred, that from the length of time the disease had existed, from its having been produced by an accident, and from its solid cartilaginous feel, it might require several applications of the instrument before the perforation could be accomplished. He was therefore recommended to go to London, to place himself under my care. This, however, he would not consent to, having an extreme aversion to leave his home. He requested me, therefore, to visit him at Bath once in ten days, or as often as required.

On the following day after the examination, the first attempt was made to perforate the stricture, and half an inch appeared to be gained. The solid mass, however, on pressing the point of the instrument against it, went on before it, which made it impossible to tell how much the lancet had penetrated. About two ounces of blood were lost; but the pain was not so severe as might have been expected. On the following day the urethra appeared to be much as usual; I left Bath the same night. Bougies were to be passed every day until my return.

On my return to Bath, I found our patient much as usual, having taken his customary exercise, and having lived in the same manner as he was accustomed to do, excepting being, perhaps, a little more careful in diet. The bougies had been passed, but they caused so profuse a hæmorrhage that Mr. Norman very judiciously left off introducing them. The blood lost was very considerable each time. The point of the bougie appeared to touch some structure in the urethra, which instantly bled. This was exemplified on passing more than one bougie. The first, perhaps, would cause no bleeding at all, whilst the second, the moment it touched the part, appeared as if it had wounded a large vessel. The blood instantly gushed forth from the urethra in a large stream, and continued bleeding, the stream gradually diminishing, until half a pint or more was lost. The excrescence through which the urine passed (as seen in the plate) was of a very vascular character; and it is most probable that the point of the bougie sometimes touched it, and sometimes not. When it did, hæmorrhage was the result.

The lancetted stilette was again used, and it was found we had kept the ground we had already gained; but the hæmorrhage was so great that the lancet was only thrust out twice or three times. We were therefore obliged to desist, and we agreed to leave the urethra alone for a fortnight, until I next saw Sir Herbert. As before, no inconvenience arose from using the instrument. The pain was trifling, and from an hour afterwards but little was felt.

In rather more than a fortnight I again endeavoured to perforate the stricture, and at this time there was no bleeding whatever. We appeared to make considerable progress, and to have nearly arrived at the prostate gland; but still the point of the instrument pressed against a solid mass. We made every effort possible to get into the bladder, but could not succeed; we therefore thought it most prudent to desist for the present, being aware that we were probably deceived, in consequence of the solid mass being pressed on before the point of the instrument. No mischief resulted.

In ten days more I again went to Bath, and found Sir Herbert in particularly good spirits. He had taken his customary exercise, lived as usual, and he fancied he made water better; indeed, but from what circumstance I cannot say, he certainly did. On the next day we resumed the attempt to perforate the stricture, and, as before, we appeared to

make progress. We did all we could to steady, or rather to fix the part, in order to prevent it receding from the instrument, and we felt that we were all but in the bladder. The instrument was in an exact line with the course of the urethra; and it was full eight inches in the urethra, but we could not get it farther; and the lancet seemed to have no effect when thrust forward. We passed another instrument, much smaller than the other, but without making any impression; it was withdrawn, and a small elastic gum catheter passed, thinking it possible the stricture might be perforated, but that the blunt point would not follow a simple incision. It unfortunately happened, however, that the point of the catheter touched the hæmorrhagic excrescence. It immediately burst out bleeding, and so profusely, that we were compelled to give up any further attempt, fearing, if such hæmorrhage continued, that the patient, circumstanced as he was, of a great age, being enfeebled, and having a disease of the heart, might die from the loss of blood; we therefore, with the greatest reluctance, were necessitated to do no more. We consulted together whether it was advisable to make any further attempt on a future occasion, feeling that possibly some peculiar alteration of parts might have taken place from the original accident. We came to the determination of waiting until the following day, to see the result of the operation. On the next day no ill consequences ensued; the urine passed as freely as before, and but little pain was felt in the urethra. Sir Herbert still felt sanguine that we should ultimately accomplish our object; and being aware, from the opinion we had at first given, that it was not to be expected we could perforate so solid a mass, the extent of which it was impossible to know, under several operations, he consented to have another trial made. It was therefore decided to defer making another attempt for a month, and in the meantime to pass a soft wax bougie every fourth or fifth day, to keep the ground we had already gained.

In a week from the time of my return to London, I received a letter from Sir Herbert, from which the following is an extract:—

"I felt the effects of your last operation more acutely, and for a longer period, than any of the preceding, and therefore withheld any further attempt till this morning; when Mr. Norman has passed a bougie (such as you directed), without my suffering *very* much pain, a full inch further than it was accustomed to go *before* your first operation. He allowed it to remain there some little time, and then, endeavouring to press it a little further, it bent; and there was a *very slight* tinge of blood, hardly worth mentioning; but he thought it prudent not to persevere, and proposes to make another attempt on Saturday, of which you shall know the result, should it be worth postage; if not, I shall not write again until Tuesday—on the day of the next effort.

"With respect to the passage of water, I cannot say it is improved or otherwise, but much as you saw it.

"My general state is sadly nervous, which is not to be wondered at, with such a weight on my mind; though I avail myself of this favourable weather, and get a comfortable walk most days."

I was gratified at this account, because it shewed how well our patient was going on. It was not to be wondered at that he should have suffered more from the last operation, because more was done, and a greater effort was made to perforate the stricture; but the little pain the passage of the bougie afterwards gave, and its producing only a *tinge* of blood, proves there could be no inflammation at the strictured part. The bougie, also, passing "a full inch further than it was accustomed to go before the first operation," gave every encouragement for us to continue the treatment. It was natural, likewise, that Sir Herbert should feel nervous, when we consider the duration of the disease—the painfulness of its nature—the sleepless nights—the incontinence of urine, and the constant dread of retention. His being able, however, to follow his usual pursuits—such as walking, &c.—proved that his general health was not materially affected.

I did not receive a letter again on the Saturday, so I concluded all was going on well. On Tuesday the following account arrived:—he says,

"I have little to report of our proceedings to-day. Mr. Norman passed three different-sized bougies; one (the second) bent at about the end, as it did with you; the last, which was of a large size, went on certainly *full as far* as ever we have gone yet; a proof the new

passage is enlarged to that extent. The whole operation, however, was attended with very considerable pain, though it produced no blood; and I ascribe it, in some measure, to the extreme coldness of the weather—for cold affects my whole frame much. We propose now to wait till Thursday, ere we make another effort, unless Wednesday should be milder, and I feel less nervous than to-day. If we are not doing a little good, we are at least keeping all as it was; so trust the delay cannot be injurious, and even, indeed, may be beneficial."

I was not surprised to hear that Sir Herbert suffered from the severity of the cold weather, for the thermometer, at that time, was lower than it had been during the whole winter. He had informed me before that he only dreaded the cold weather, fearing that it might bring on (as it had often done) a spasmodic affection of the bladder, and retention of urine. On November 8th, I received the following account from Mr. Norman:—

"Our patient, Sir Herbert Sawyer, has not been so well. He wrote to you of my having passed the bougie three times, and each time it went certainly an inch into the new passage which had been made; it gave no pain, and there was no *bleeding* and no *irritation* after. This was the case on Monday, which was the last time it was passed; but on Tuesday he became feverish, had pain about the rectum, and the *next day* there was a good deal of spasm and effort in passing his urine; still it did pass, and has continued to do so; but the bladder certainly is never quite emptied. He is much worn by this, and his pulse is very irregular. I have no doubt this will get better, but of course it will not be possible to use the bougie, or to do any thing with instruments, for the present. He wished me to let you know this, and I was also desirous you should know his present state. I will write to you again about him in a day or two."

From this statement I did not apprehend that much serious mischief was going on; neither, as it may be seen, did Mr. Norman. It appeared to me that he had one of his usual attacks, in a slight degree, of spasm of the bladder, and that this was brought on, according to his own account, from the severity of the weather, and that it would most probably go off in a day or two. When the bougie passed, it produced no pain, and there was no irritation afterwards—neither was there any bleeding: thus proving that there could not have been much inflammation at the strictured part, particularly as the urine continued to flow through it.

The next letter I received from Mr. Norman, which was three weeks from the time I last saw him, and a week after the passage of the last bougie, he informed me, very much to my sorrow, that Sir Herbert had gradually got worse, until he became insensible, and that he had been obliged to puncture the bladder. I immediately took a place in the mail, and went down to Bath the same evening. On my arrival there on the following morning, I called upon Mr. Norman as soon as possible, who informed me that I should most probably find Sir Herbert dead. Every means that could be thought of had been adopted for the relief of the patient, but without effect. On going to the house, I found that he still lived, and he was in much the same state as described by Mr. N., excepting that, during the night, a consciousness had occasionally returned, and he knew his daughter, and some of the people about him: his breathing, however, was heavy, almost amounting to stertorous, and his pulse even more irregular than common, although it had some power in it. The pupil was dilated. If roused, he would answer; but he immediately fell into a comatose state, or would continue a low muttering to himself. I examined the perineum, but there was no swelling nor tenderness.

Mr. Norman and myself consulted together, and employed every means in our power; but, in spite of all remedies, he only lingered on until the following day, and then died.

Examination of the body twenty-two hours after death. — The abdominal muscles were first dissected, to examine the opening which had been made by the operation of puncturing the bladder above the pubes. There was a little extravasated blood between the fibres of the muscles. The parts about the opening appeared to have been slightly inflamed, but there was no appearance of suppuration, nor evidence of extravasated urine.

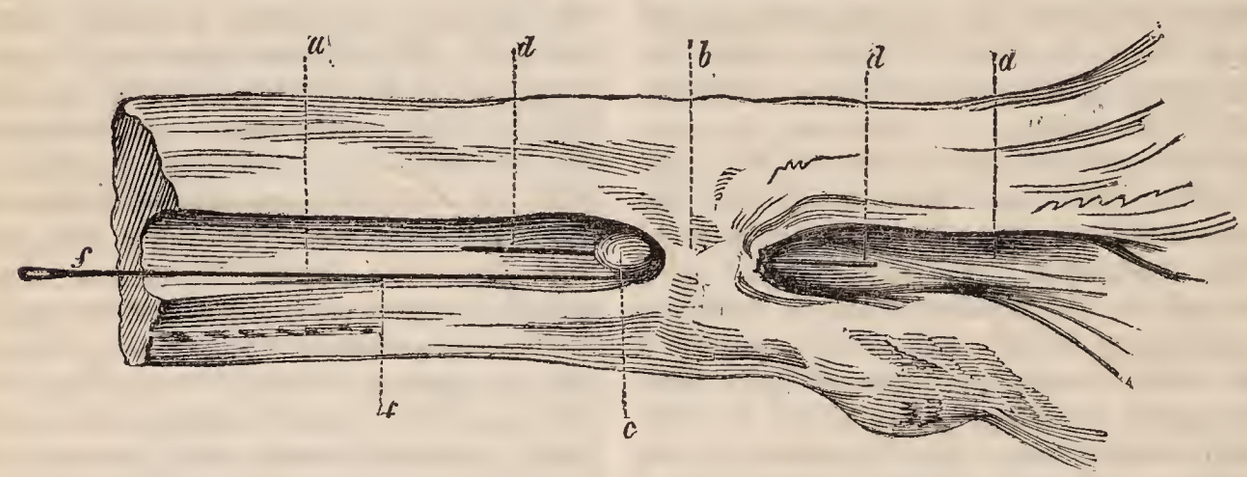
The bladder, on being opened, was found to contain about seven ounces of

turbid urine; its external surface was every where highly inflamed, and covered by a muco-purulent secretion and calcareous matter: its walls were about twice the natural thickness, and its whole volume also was twice or three times larger than natural.

The prostate gland was slightly enlarged, but not more than usual in old people; and the third or middle lobe was not increased in size.

The urethra was full eleven inches in length. It was not inflamed nor ulcerated. There was no swelling nor formation of matter, nor extravasation of urine into the perineum.

There was a stricture commencing an inch anterior to the bulb, which occupied more than an inch of the canal: it was of a very tough consistence; and at the extremity of it, nearest the external orifice of the urethra,



a, the urethra.—*b*, the stricture.—*c*, the excrescence through which the urine passed.—*d*, the bristle introduced through the centre of the excrescence.——*f*, a probe introduced into the newly-made canal.

there was a nipple-like substance projecting into the canal, resembling the glans penis of an infant, or the uvula, through the centre of which there was a small canal, which could only admit through it a bristle, and through which alone the urine could pass. This excrescence was of a highly vascular structure. On the lower side of this projection there was a newly-made canal, about one-fourth of an inch in length, which terminated in a blind extremity, and which blind extremity was about three-fourths of an inch from coming out into the urethra, on the other side of the stricture*. This no doubt was produced by the perforation of the lancetted stilette, or urethral perforator.

That part of the urethra between the stricture and bladder was elongated about two inches more than natural: it was also enlarged; and several small calculi, about the size of millet seeds, and one as large as a pea, were lodged in it.

The ureters, more particularly the right, were much enlarged, and contained urine.

The pelvis of each kidney, also, more particularly the right, was extremely large, and so thin as to resemble bladder; each of them contained some concretions of lithic acid. The cortical substance of both was very pale, thin, and wasted, being not more than half an inch in thickness. There were some small serous cysts on their surface.

The lungs were perfect.

The heart was greatly diseased, the left ventricle being irregularly hypertrophied or thickened. The right ventricle was considerably dilated. The mitral valve was thickened, and some part approached to ossification.

Portions of the aorta were hard and discoloured, and approached to ossification. This artery, also, was greatly dilated, particularly the abdominal aorta. The right iliac artery was obliterated.

The brain and the arachnoid membrane were generally inflamed, and some parts of it were opaque. The substance of the brain was vascular, being studded by the bloody points of enlarged blood-vessels. The ventricles contained serum.

The stomach, intestines, and liver, were healthy. Most of the arteries of the lower extremities were ossified.

Remarks.—The whole history of this case, taking it from its commencement to its termination, is extremely interest-

* The preparation is in the Museum of the Bath Hospital.

ing. The stricture being produced from an accident, the continuance of the hæmorrhage for so long a time afterwards, and the impossibility of ever introducing any urethral instrument into the bladder, are all points of interest. It is not an unusual occurrence to find stricture brought on from an accident such as Sir Herbert met with. Sailors, for instance, as in Thomas Clerk's case, fall from the rigging of a vessel, and strike the perinæum against any projecting body on deck. Those who ride much are liable to the same accident: they are constantly battering the perineum against the saddle, or by some sudden jirk are thrown against the pummel, and thus, from the inflammation produced in consequence, give origin to the disease. I have observed that this kind of stricture is generally of a much worse description than those brought on from gonorrhœa, or from any other cause; and I attribute it, not only to the coat of the urethra itself being altered in structure, but also to the parts external to it becoming indurated and consolidated.

The hæmorrhage having continued so long after the accident, shews either that some vessel in the urethra was ruptured, or that the canal itself was torn. Such an occurrence, after so much violence, and so great a shock having been received at the part, might naturally have been expected. It may be observed, that the same happened in Clerk's case; the hæmorrhage from the urethra continued for three days afterwards. The impossibility of passing a bougie into the bladder at any time after the accident is remarkable. In the early stage of the disease, this circumstance cannot be easily accounted for, unless the accident produced some transposition of parts; but in the latter stage there can be no difficulty in explaining it. The dissection of the urethra proves that the introduction of any urethral instrument would have been impracticable, if not impossible.

The immediate cause of death in this case, no doubt arose from serous apoplexy, brought on by the irritation and pain produced by the recent inflammation of the bladder, and the long and continued irritation of the urinary organs generally, for so many years. Serous apoplexy is one of the commonest terminations of disease of the urethra and bladder, and particularly when it has been of long duration, and when the individuals who are the subjects of it are of a great age. The sufferings of patients thus situated, are so great, so painful, so constant, so harassing, that their life is gradually worn out. Both by night and by day they are in constant distress; they cannot, perhaps, void their urine in any other manner than by drops; they have a frequent inclination to do so, without being able to empty the bladder; it passes probably involuntarily from them; they are obliged to rise up from their bed every quarter or half hour, to attempt to relieve themselves; their rest is broken, and just as they have fallen asleep they are probably awoke again, from the distressing desire to make water. In short, their whole occupation is in attempting to relieve themselves of the urgency of the symptoms of their complaint. In this dreadful state of anxiety and misery they continue, until they become enfeebled and exhausted. The brain at length sympathizes with the pain and irritation of the urinary organs; urinary coma, as it is termed, comes on; water is formed in the ventricles; and serous apoplexy is the result.

On reading the account of the post-mortem examination of the body in this case, we can only feel surprised that Sir H., with so much disease of organs so intimately connected with the functions of life, should have lived so long. Independently of the disease of the urethra, which of itself was sufficient, by the constant and harassing pains, and the long and continued suffering it had occasioned, to cause death, we may observe the following morbid changes had taken place:—

The bladder was in a high state of inflammation, being three or four times more capacious than usual; and from old disease, its coats were twice the natural thickness; calcareous matter was deposited on its inner surface, and its whole appearance evidently bore marks of long and continued disease. The ureters and kidneys were much diseased, the former being considerably dilated, and the substance of the latter being almost entirely obliterated, the cortical part being not more than half an inch in thickness, and the pelvis being so enlarged and thin as rather to resemble bladder than its natural structure. The brain had suffered

both from previous and recent disease. The arachnoid membrane was in many parts opaque and thickened, and in others inflamed. The substance of the brain was studded by the bloody points of enlarged blood-vessels, and the ventricles contained serum. The heart was enlarged, the left ventricle being generally, but irregularly, hypertrophied, some parts of it being thicker than others, and the right ventricle dilated. The valves of the aorta were ossified, and the aorta itself, throughout its whole course, but more particularly in the abdomen, was dilated to nearly twice its natural size. Almost all the arteries of the body also were ossified, and some were obliterated. It is needless to say that such a state of heart and arteries might at any time have been productive of sudden death. Besides these, there were other morbid changes, of a minor description, in various parts of the body.

It will be clearly seen that no mischief arose from the operation. There was no inflammation at the strictured part—no ulceration—no formation of pus—and no extravasation of urine into the perineum. In fact, excepting the stricture itself, the urethra was perfectly healthy.

This description of stricture is perhaps unique: the nipple-like projection, through which only a bristle could be introduced, and through which alone the urine could pass, being so regularly formed, and resembling the glans penis of an infant, or the uvula, is a morbid change which I have never before witnessed in the urethra; nor have I met with any one, however experienced he may have been in morbid anatomy, who has. How to account for its formation would be difficult; whether it was a congenital formation, like nævus—whether it was caused from the accident—or whether, from the extreme closeness of the stricture, the sides of the contracted part were, by the pressure of the urine, gradually forced forward, and thus the projection was formed, it would be difficult to determine. The manner in which it projected into the centre of the canal, however, and the peculiarity of its formation, was such, that it rendered the obstructed part totally impervious to any instrument. We cannot be surprised, therefore, at the failure of surgeons of such eminence, who had attended Sir Herbert, in passing a bougie or catheter into the bladder. The treatment which was adopted appears to be the only one which offered the slightest chance of relief; and it is only to be regretted that it had not been sooner attempted, when the patient had not been at so advanced a period of life, and so enfeebled and worn out by the disease.

[To be continued.]

OBSTRUCTION BY A BILIARY CALCULUS.

To the Editor of the Medical Gazette.

Sir,

By giving an early insertion to the following, you will oblige,

Yours most obediently,

Thomas Ross.

Wakefield, Yorkshire,
May 31, 1834.

In confirmation of the opinion given in your valuable journal of the 10th instant, by Dr. W. P. Borrett, of Norwich, respecting cases of gall-stone not being exempt from danger to life, as was most satisfactorily proved by the post-mortem examination of the fatal case therein related, I beg leave to record the substance of one which occurred at an earlier period, and during my apprenticeship.

In the spring of 1823, in the absence of my master, I visited a poor woman living at Brooksbank, in the immediate neighbourhood of this town, who, on my arrival, presented the following symptoms. Pulse at the wrist scarcely perceptible; extremities quite cold; countenance indicative of great suffering; powers of articulation and swallowing nearly gone. She was laid upon her back, the thighs being drawn up towards the abdomen, with an occasional moan and slight attempt to vomit. The whole surface was of a dirty-yellow hue, and bedewed with a clammy moisture. There was a slight shrinking on pressure upon the body, but no swelling or tumidity whatever. She died in less than four hours from my first visit.

It was evident nothing could be done here; and the only information I could collect was from her neighbours, who said that the bowels had been evacuated, and that she had also made water during the day: they also stated that she was attacked, only the day

before my visit, with pain in the stomach and vomiting, and that something had been procured from the druggist's for her relief; further adding, that in their opinion she had taken poison; which supposition was most probably grounded on the suddenness of her death. The post-mortem investigation took place on the day following her decease. The stomach and intestines were perfectly healthy, as were the pancreas, spleen, and kidneys: the contents of the pelvis were also healthy. The liver was darkish in appearance, being engorged with blood; but there was no disease at all of its structure. Gall-bladder empty. On tracing its duct, and that of the liver, there was nothing unnatural; but on arriving at the ductus communis choledochus, the cause of death was pretty manifest. About the centre of this common conduit, a biliary calculus was found firmly impacted, which created some difficulty in extracting. The only appearance of disorder was a slight inflammatory blush in the coat of the tube, immediately surrounding the stone. The contents of the thorax were perfectly normal. The head was not examined.

In conclusion, I have only to add, that the impression I received from this case has always led me to give a guarded prognosis in similar circumstances.

PATHOLOGY OF HYDROCEPHALUS.

To the Editor of the Medical Gazette.

SIR,

SHOULD you consider the following remarks on hydroeephalus worthy of insertion in your valuable journal, I shall feel obliged by your giving them publicity.—I remain, sir,

Yours most respectfully,

JOHN GRANTHAM.

Crayford, Kent,
June 10, 1834.

Serous membranes surround the vital organs, and are composed of innumerable absorbents and lymphatic vessels interwoven together; the blood-vessels are few in number; and what are generally called arteries of the pleura and peritoneum, &c. are no more than trunks twining over their external surface, susceptible of being detached whenever these membranes are displaced, being connected with them, and not concerned in their structure, to which the absorbents, exhalants, and cellular tissue, almost exclusively contribute. Undoubtedly there are communieations between the arterial system and the serous membranes through the exhalants, but nothing respecting the nature, the disposition, and even, in some degree, the functions of these vessels, has yet been correctly ascertained.

The vitality of serous membranes is very distinct from that of adjacent parts: inflammation does not destroy them; a fluid is always (in health) found on serous surfaces. Inflammation acts by suppressing this secretion in the first periods of inflammations. When the exhalants are filled with blood accidentally admitted, serum flows no longer from their free surface: it is during this stage that adhesions take place. If they do not form, either on account of motion, or from any other reason, and if the inflammation does not subside by resolution, then what happens in an open wound takes place in the serous surfaces—they suppurate. Now this suppuration is never attended with uleeration and erosion of their substance. Let their purulent collections be never so considerable, these membranes will constantly remain uninjured; their tissue will only be more or less thickened; they emit pus in the same way as their natural serous fluids—that is to say, by exhalation. I also observe that absorbents frequently terminate in the neighbouring veins: whether the veins absorb or not, is at present a disputed point. The vessels are excited in the following degrees: the absorbents first, the lymphatic glands second, and the veins last. It is a fact, that exhalation forms tumors, and absorption removes them; and from M. Fodéra's experiments, it is inferred that exhalation and absorption take plaee by transudation and imbibition, and depend on the capillarity of their tissues.

Three circumstances take plaee in the vessels of an inflamed part,—two connected with the capillaries, and one with the large vessels: in the first change, the capillaries, which conveyed a colourless fluid, contain red blood; the second change consists in the inerease of the diameter of the vessel; in

the third change, the arteries and veins are alike all over distended. This appears explicable on two prineiples,—inerease of animal heat, and interruption in the eireulation of the eapillaries. This, as I have already said, shows a direet tendency to produee suppression in the exhalants. In inflammation, the vessels, the blood, and the nerves, undergo a ehange. I do not say that inflammation does not take place in a neighbouring organ, as the brain, liver, &c., during effusion, but that inflammation eannot and does not directly cause effusion.

Cloquet, Orfila, and Professor Dumeril, have all stated their belief that hydroeephalic disease in children, and cerebral fevers in adults, are very distinet. Dr. Niehol considers effusion as more under the influence of the nervous system than the arterial. Dr. Yeates most ably advocates the doetrine that nervous irritation from an unhealthy condition of the alimentary eanal prevents absorption. Dr. Mason Good eonsiders hydrocephalus "to arise from two remote causes, inflammation and debility:" this must imply direet and indirect effect. Dr. James Carmichael Smith states debility as the cause of serous effusion, and never saw a ease that required the loss of blood. He further remarks, that serous effusions, the result of inflammation, are always turbid, and mixed with flakes, whilst the surfaee of inflamed membranes are eovered with coagulable lymph, the membranes thiekened, and adhering to the eontiguous surfaces. This is also Morgagni's opinion. Dr. Blaekhausen, of Bremen, disseeted sixteen cases of hydrocephalus, in all of which the mueous tunic of the stomach was softened, and the internal surfaee of the intestines studded with diseased follicular glands, through whieh pus escaped; the other abdominal organs were healthy. Dr. Golis, Dr. Yeates, Dr. Cheyne, and Mr. Abernethy, all state their belief in the doetrine, that the primary cause of hydrocephalus exists in the stomaeh and intestines; and from what I have observed in drunkards who have died, the arachnoid membrane in sueh cases is always more or less affeeted. I would also add a statement made by Haller (El. Phys. vol. vii. s. 1, parag. 9)—viz. that if the kidneys refuse to secrete, urine has been found in the ventricles of the brain.

The deductions which I am led to make are these:—that inflammation is not a direct cause of serous effusion; but that whatever tends to stimulate the exhalant vessels, and to interrupt the absorbents, is more properly the cause of sueh a condition.

BRISTOL INFIRMARY AND MEDICAL SCHOOL.

To the Editor of the Medical Gazette.

Sir,

The surgeons of the Bristol Infirmary have not, at present, the least intention of trespassing further on your pages, or offering any reply to the observations you have thought proper to introduce into your leading artiele of last Saturday (which only eame to my notice yesterday), with respect to the memorial lately presented by them to the Parliamentary Committee on Medieal Education, &c. &e. My colleagues, however, avail themselves of this opportunity of acknowledging how mueh they feel obliged by the publieity your columns have given to the sentiments contained in that document.

Neither would the "Associated Lecturers of the Bristol Medieal Sehool" have requested the favour of you to lay before the professional public the aeompanying representation, in reference also to medical education, but that it may, in some respeets, be considered a reply to, or in some degree an indirect explanation of, some of the points advanced in your last leading artiele. I am further indueed to offer it to your notice, in the full expeetation of its reeeiving from your pen a eontinuation of similar opinions to those already promulgated by you on this subjeet. A bold, candid, and impartial objeetor to any system or plan, is always desirable, for by that means its originators obtain the opportunity of correeting, and eonsequently of perfeeting, the objeet they have in view; how far you have sustained this eharaeter in the observations you have been pleased to make, we beg to leave with the Parliamentary Committee and the profession to pronounce judgment.

It has not escaped my notice, that your eritieism is founded ehiefly upon the aid it derives from the surgeons of the Birmingham Hospital having de-

clined to meet the views entertained by the surgeons of the Bristol Infirmary; and as you have not alluded to any other support in this important question, I may venture to conclude that at present the gentlemen of that institution stand alone *. This coincides with my own information, after an extensive correspondence with most of the surgeons of the different provincial hospitals. The surgeons both of the Royal Infirmary, Manchester, and Gloucester, have already announced that they concur most fully in our sentiments, and will cheerfully co-operate with us in any measures which may be deemed most advisable. This combination of effort is not only very encouraging, but, we trust, will have its due influence with the Parliamentary Committee, in recommending to the legislature that the obnoxious regulation complained of may be annulled, or so modified as not any longer to embarrass and interrupt provincial medical education.

I presume you cannot have any objection to publish this letter, with the accompanying documents, after the publicity and importance you have thought fit to bestow on a subject certainly of no ordinary interest, both to the profession and society generally.

I have the honour to remain, sir,
Your most obedient servant,
WM. HETLING.

Clifton, Bristol,
June 8, 1834.

[We readily give insertion to the preceding letter, but really cannot do so with the documents sent along with it. The memorial addressed to Mr. Warburton is in substance, if not in words, identical with that already published in our number for May 3, page 190. With respect to the prospectus of the Bristol school, we can only give it publicity in the usual way, upon our wrapper. We take leave to add, with reference to Mr. Hetling's suggestion that we should continue our observations, that as yet no attempt has been made to answer those already offered. When this has been done, it will be quite time enough to follow them up.—ED. GAZ.]

* Mr. H. has since written to inform us, that the surgeons of the Bath United Hospital entirely concur with their brethren of Birmingham.—ED. GAZ.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

An Essay on the Relation of the Theory of Morals to Insanity. By T. MAYO, M.D. Fellow of the College of Physicians, and late Fellow of Oriel College, Oxford.

WHOEVER has turned his attention to the subject of insanity, or lunacy, or what is popularly termed unsoundness of mind, must have felt the vagueness in which he was soon involved; and, if of a speculative turn, he must have been induced to form such definitions as might serve to bring the matter under consideration within reasonable bounds. In general there is but one sort of derangement recognized—that in which there is a perversion of some of the intellectual faculties: but this will by no means comprehend that state in which human beings are not unfrequently found, where the reasoning powers are not perceptibly impaired, but there is a deficiency of moral principle — the fertile source of all their irregularities —of their crimes. Paley, we know, was of opinion that man is not naturally a moral being; a position which he attempted to establish by appealing to the depravity of savage nations. This, however, Dugald Stewart successfully controverted, by attributing all such examples of the non-appearance of a moral principle, to the absence of those circumstances by which it might be brought forth and duly developed. "Did it ever enter into the mind of the wildest theorist," says this distinguished professor, "to imagine that the sense of seeing would enable a man, brought up from the moment of his birth in utter darkness, to form a conception of light and colours?"

But, however untenable the position—however mischievous the doctrine, that man is naturally destitute of a moral sense—it would seem not only possible, but highly probable, that there are many individuals in this predicament. We speak of man, generally, as a reasonable being, though the individual exceptions are neither rare nor few: and so we may also descant, in general

terms, on the moral nature of man, though there can be little doubt that instances are not infrequent of persons who, from natural deficiency of some moral feeling, are not properly to be deemed responsible agents,—so far as by so deeming them we imply the presence of an universal principle.

Dr. Mayo, in the interesting pamphlet before us, holds that there are cases in which the agent necessarily falls into vicious habits, or criminal acts, from want of a particular moral sense, he being thus destitute of a standard by which certain forms of good and evil can be estimated. Aristotle was of this opinion, when he described a class of men in whom the βέλτιστον, by which he denominates the moral sense, "has not indeed been corrupted, but has never existed." It is curious how the philosophers of mind have so constantly overlooked this circumstance—no doubt from their great zeal to generalize, and to deal in sweeping theories. There has been the hint in the Ethics of Aristotle for above twenty centuries, yet has it remained a barren topic for commentators and succeeding moralists, till our present author, with a practical object in view, has called attention to the importance of taking it into consideration.

In the character of Tiberius, as depicted by Tacitus, it would seem gratuitous to assume that a moral feeling existed. Cæsar Borgia is adduced as another example from history; and Lord Wharton exemplifies the same position, as he is described by Swift as being "without the sense of shame or glory, as some men are without the sense of smelling; and therefore a good name to him is no more than a precious ointment would be to these." But, without having recourse to examples of this sort, selected from the records of history, we may quote a couple of parallel or contrasted cases from common life. Dr. Mayo gives them as instances coming under his own cognizance.

"Mr. A. was born in a respectable station, and is in possession of a good fortune, of as much, at least, as he has allowed to remain out of a good fortune. He has a wife and children, and as many friends, or rather associates, as his convivial qualities retain for him, in spite of the hardness of his character. He has always been profusely extravagant; for his passions and appetites have compelled him to squander money, which he would probably have hoarded, if his selfishness had taken that turn. His temper is at once stern and violent; and all who know him expect that the dispositions of his will must prove him, to the last moment of his life, utterly unjust. If he had sufficient courage, he would rob or murder; for his cupidity is under no moral check; but he is naturally very timid, and owes to this circumstance his freedom from such acts as the law construes into crimes. Such is Mr. A., and such also, or as nearly such as the distinctive points which separate all individuals will allow, is Mr. G. his neighbour. But there happens to exist a peculiarity in the latter, which materially alters the course of his life, and its results upon others. Mr. G. was observed to talk very much to himself. This excited attention; and, on further inquiry, it was discovered that he was habitually under the influence of false perceptions, and that he considered himself solicited by certain voices, audible only to himself, to perform those actions which indeed flowed naturally enough from his own evil dispositions. Mr. G. was accordingly recognized as a lunatic, and placed under restraint. Thus the family of Mr. G. is secured against the results of his moral character; and his fortune will descend according to the principles of law; thus dealing out a justice to others, and a protection to *him*, which would have been refused, but for this hallucination."

Parallel instances to the foregoing are of every-day occurrence, and seem to have been so for ages; yet the measures which would appear to suggest themselves in consequence have met with no attention. We readily adopt a coercive treatment where we fancy an intellectual aberration to be present; but we take no steps to provide for those cases in which the moral feelings are in a morbid or an evanescent condition: nay, we make the unfortunate persons, who are in this predicament, as accountable as other people for their actions. Lord Ferrers was hanged for a murder committed, not certainly in a state of intellectual derangement, as his conduct both at the time of the murder, and subsequently at his trial and execution, clearly showed; but if we consider the imputed crime with reference to a moral principle—of the

existenee of which in his lordship there is no evidence—the whole circumstanees assume a new eharacter; and we eannot but maintain that there was eommitted in his case a great judieial error.

In most cases of deliberate suieide we have examples of this moral form of insanity on whieh our author insists. "No intellectual delusion," says Dr. Mayo, "need be present when self-destruetion is coveted. But there must be an extinetion of that moral sense, which revolts from it on grounds independent of fear. Owing, however, to the systematic neglect of moral symptoms, the suicide is seldom reeognized as possessing his destruetive tendeney until he has made an attempt upon his life; often, therefore, until all measures must be too late. It is to be hoped that there exist means of improvement in our acquaintance with the symptoms of a disorder which is at present not recognized until it is fatal."

Several other instances are referred to by our author as tending to establish his position; and his suggestions as to the conduet which we should pursue, on admitting the existence of moral derangement, though not very definite, seem just and praiseworthy. The expediency of ascertaining the existenee of the state in question by positive proofs is obvious, and indeed more imperative than when we have to deal with a merely unsound understanding, inasmueh as demonstrations of folly are less formidable than those of erime.

The sort of restraint to which persons labouring under moral insanity should be subjected, is a question of deep and vital importanee: at present, as the author observes, there is no medium between a well-regulated sehool and a mad-house; and as to the applieation of the jury system to cases in which commissions of lunaey are sought for, we fully agree with Dr. M. that it is often singularly absurd and misehievous.

We reeommend very strongly, to all who take an interest in these matters, the perusal of Dr. Mayo's pamphlet. The author himself eonsiders it as affording a mere outline of the important subject of which it treats; but we think there is in it abundance of *materiel* for refleetion, the result of which eannot but prove beneficial to the community.

MEDICAL GAZETTE.

Saturday, June 14, 1834.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

VALUE OF SCOTCH DEGREES IN ENGLAND.

A TRIAL lately took place at the Dorset Assizes, the results of which involve eonsiderably the interest of physieians with Seotch or Foreign degrees praetising south of the Tweed. A gentleman who had obtained the doctorate at St. Andrew's, was slandered—or supposed himself to be so, by another physician; indeed, the words applied to him seem to have been very strongly depreeiatory of his professional attainments—branding him, in faet, as "an impostor," "ignorant," and as "unfit to praetise physie." The jury do not appear to have attaehed mueh importance to these speeimens of the *odium medicum*, nor to have treated professional character as a very valuable eoncern, having only mulcted the defendant in the sum of ten pounds sterling. Nevertheless, they admitted the principle in awarding even these very moderate damages; but small as they were, the losing party thought them too much, and employed eounsel to move for a rule to shew cause why the verdiet should not be set aside, and either a non-suit entered, or a new trial granted. The ground of this applieation, and the decision thereupon of the Lord Chief Justice Denman, are the points to whieh we wish to direet attention.

It was urged that the plaintiff had a Seotch degree, but no license from the English College of Physicians; that the jury had found for the plaintiff *quasi* a legal praetitioner, but that a Scotch physician was not entitled to

practise in England without the College license; and consequently that he had been protected in a capacity which the law does not recognize. The only answer to this which the counsel for the plaintiff urged, was, that his client had a St. Andrew's degree—was a physician in Scotland, and consequently equally a physician in England, the two countries being united. When the case had been heard, Lord Denman said, that before a man could recover for slander against him in any business, it was first necessary to shew that he carried on such business lawfully; but whether a Scotch diploma, without an English license, did or did not so legalize the party, he was not prepared to say. This, which was merely an adjournment of the cause on the part of his Lordship, in order that he might clearly ascertain the law of the case, was forthwith construed, by some of our contemporaries, into the expression of an intention, by Lord Denman, to give judgment in opposition to what his predecessors had declared to be legal; his remarks being made, as we were told, "notwithstanding the recorded decisions of several of the judges;" while, again, we were farther favoured with the information (not by Lord Denman, but by the worthy and sagacious editor) that "the verdicts given under the direction of the judges, against graduates in medicine and members of the College of Surgeons, *were contrary to law*." That the Chief Justice did not take the opinion of this great [crowner's quest] law authority, instead of that of the Judges of the land, must be attributed to some peculiarity, not to say obliquity, in the constitution of his mind; but it so happens that his lordship, after having duly weighed the case, and satisfied himself of the law, came to a decision diametrically the reverse of that which our contemporary instructed him to give, and pronounced that which follows:—

"The court had (his lordship said) taken time to consider the question raised in this case, which was, whether a person who had obtained the degree of doctor of medicine in a Scotch university, in the College of St. Andrew's for instance, could, without obtaining a license from the College of Physicians in London, maintain an action, in the character of a physician, in this country. Their lordships were of opinion that he could not; and they came to that conclusion on the words of the statute 14 and 15 Hen. VIII., c. 5. The first section declared who should practise as physicians in London. The third section of that statute recited, that in the dioceses out of London it was not likely there would be found men fit to practise medicine, as required by the first section; and it enacted, that henceforth no man should practise in such place "without a letter of license from the College of Physicians, except the graduates of the Universities of Oxford and Cambridge, which both accorded him all things for his advantage without any grace." This statute vested the power of granting such a license in the College of Physicians; and Lord Holt, and the rest of the Court, in the case of the College of Physicians *v.* Lovett (Lord Raymond's Report, 472), decided that even a graduate of the University of Oxford or Cambridge could not practise as a physician within London, or seven miles of it, except by letters of license from the College of Physicians. A similar opinion was also held in the case of the College of Physicians *v.* West, in 10 Modern Reports, 353, and those decisions must bind the Court in the present instance. In the latter of these cases the Court adopted the opinion, that by the last clause in the statute, none should practise in the country without a license from the President and three Elects, unless he were a graduate of one of the Universities of Oxford and Cambridge. It followed, from the authority of that case, that the plaintiff here had no right to practise as a physician in the country without a license from the College of Physicians, although there was not in the statute any penalty imposed upon him for so doing. This action, it was therefore clear, could not be maintained by him in respect of slander upon him in a

profession which by law he had no right to exercise. In the case of Smith *v.* Taylor, 1 New Reports, 203, was a dictum of Lord Chief Justice Mansfield, in which he was supposed to say, that since the Union a degree conferred by the Scotch Universities had the same effect as a degree conferred by the English Universities. The Court did not think that Lord Chief Justice Mansfield meant to imply any such thing; but, if he did, they were of opinion that the dictum was totally unwarrantable by any authority. The verdict given for the plaintiff, with 20*l.* damages, must therefore be set aside, and the judgment of the Court must be given for the defendant."

It thus appears that a physician with a Scotch degree, but without the College license, cannot recover damages for being called an impostor,—and whether the law be a good or a bad one is not now the point,—let it be only kept in mind that it *is* the law. Lord Mansfield, indeed, was supposed to have held that a Scotch degree was equivalent to an English one; but we believe that all other judges have differed from him in opinion, and the decision arrived at by Lord Denman is that on which the highest authorities have concurred. It is of importance to be aware that practising as a physician beyond the distance of seven miles from London, without a license from the College of Physicians, is a misdemeanor said to be punishable by common information; but, in fact, apparently entailing on the party only the negative disadvantage of being unable to sustain an action in his professional capacity.

This is undoubtedly an incongruous and unsatisfactory state of matters; and though we cannot, like our contemporary, imply that Lord Denman should on his own proper authority set aside the law, instead of administering it, yet we do opine that it were more natural and consistent to have medical education so regulated that the value of different diplomas and designations should be perfectly known and understood. In our opinion, no degree granted by teachers, as is done at Edinburgh and other Universities, ought to be admitted as a legal qualification to practise; and we suspect the day is not far distant when it will cease to be so. But in such case the graduates of Oxford and Cambridge ought to stand on a footing of equality with others—certainly they ought not to have any advantage over them; their degree ought not, as at present, to confer a power to practise, independently of the College of Physicians. It is one of the many grave points which deserve the attention of the Parliamentary Committee, whether this principle of acting, as a check upon degree-giving institutions, should not be retained, under certain modifications and restrictions; and among others, that of making it imperative on the graduate in medicine, so long as degrees are conferred by those who teach, to receive the license of an independent body—such license, however, being equally valid on either side of the Tweed or of the Irish Channel.

THE LATE BARON PERCY.

A BUST of Baron Percy, the eminent military surgeon, is about to be placed in the Academy of Medicine. The report on the propriety of its admission was drawn up by Desgenettes, who paid a handsome tribute to the memory of his old friend. The standing order is, that no bust or picture of a member shall be admitted into the Academy till he has been dead for at least five years. M. Percy died in 1825; and the present application has been made by Madame la Baronne, who employed David to execute a splendid marble bust of her deceased husband.

MEDICAL RESPONSIBILITY IN FRANCE.

THE Visigoths had a law, that if a physician were called in to a case, and took charge of it, he was bound to effect a cure. If the patient died, the physi-

cian was immediately delivered up to the friends of the deceased, that they might do what they liked with him. Some of the French journalists complain piteously that the laws affecting the profession in France, at the present day, are not less severe. A case lately occurred at Evreux, in which a M. Thouret-Noroy, in consequence of alleged malapraxis, by accidentally injuring the brachial artery, so as to render amputation necessary, was mulcted in heavy damages; and having appealed to a higher tribunal, had 400 fr. more laid on him, by way of interest on his former fine.

MEDICO-CHIRURGICAL SOCIETY.

Tuesday, May 27, 1834.

On Fibro-Calcareous Tumors, and Polypi of the Uterus. By Robert Lee, M.D. F.R.S. Physician to the British Lying-in Hospital, the St. Marylebone Infirmary, &c. &c.

Dr. Lee recognizes three classes of uterine diseases which are attended with alteration of structure. 1. Those produced by inflammation, atrophy, or hypertrophy, of any of the textures of the uterus; 2, those arising from tumors developed in the parietes of the organ, or from enlargement of the glands about its orifice, but not of a malignant form; and, 3, those resulting from a malignant specific action of the uterus, by which its different textures and adjacent parts become disorganized. Of the first class, the author has treated in the 15th and 16th vols. of the Medico-Chirurgical Transactions; and in the present communication he confines himself to the second class.

Fibrous tumors of the uterus are sometimes met with, not larger than a pea; but occasionally they are the size of a cricket-ball, or even of the gravid uterus in the ninth month. When cut into, they present a laminated or radiated semi-cartilaginous structure. Sometimes, however, they have a granular appearance, as if made up of smaller tumors, each enveloped in a thin capsule of cellular membrane. Their colour is generally a yellowish white; sometimes an ash-grey, approaching to dark slate. They are vascular, arteries and veins of considerable magnitude running into their substance, and occasionally cavities containing dark-coloured gelatinous fluid are found in their central parts. In one specimen (a preparation and drawing of which was exhibited), a clot of blood was contained in the cavity of the fibrous tumor. But where those tumors continue to increase without cavities, they become cartilaginous, and have neither arteries nor veins containing red blood; calcareous depositions are found in their substance, and they are often ultimately converted into a mass of carbonate and phosphate of lime, which is sometimes so hard as to admit of a polish like marble.

Bayle considers this sort of bodies as osseous, and Andral as an ossiform transformation. The latter also appears to have adopted Brugnatelli's view of the subject. In one of Brugnatelli's concretions, however, on its being divided with a hammer, the tibia of a chicken was found in its centre; the mass was ascertained to consist of phosphate of lime; and a second calculus, belonging to the same professor, contained a number of crystals of ammoniaco-magnesian phosphate. But Dr. Lee suspects that these concretions were probably urinary, not uterine.

In the case of an aged woman, whose body was examined at the Marylebone Infirmary, in 1832, there were found, under the peritoneal coat of the fundus uteri, several small fibrous tumors with calcareous deposits. A soft, yellowish-coloured, calcareous tumor, about the size of a goose's egg, was found situated under the lining membrane at the posterior part, and distending the cavity of the uterus, like an ovum at the end of the second month of gestation. The existence of any organic disease of the uterus was not ascertained in this case before death.

Dr. Lee proceeds to give an account of several other tumors of the same description which he examined. In one of them there was the curious additional peculiarity of an atrophied uterus. Atrophy of the uterus, the author takes occasion to observe, is a very rare disease, of which he has met with only two examples. In a specimen of the affection, in the possession of Mr. Griffiths, of Pimlico, the muscular structure has disappeared; and being inflated, is transparent, like a hog's bladder. This is what Walter, in 1786, described as a membranous uterus.

Some cases are mentioned by the author, in which acute pain in the hypogastrium, with ulceration of the uterus, and profuse discharges of blood and pus from the vagina, were the consequences of the presence of these calcareous tumors. In one of these cases, the patient, aged 62, after suffering excruciating pain, with unusually copious and fœtid discharge, passed from the vagina several portions of small irregular-

shaped concretions, having a worm-eaten appearance. She died, however, some time after, with symptoms of peritonitis; and on the examination of the body the fundus and body of the uterus were found extensively disorganized. A large fibro-calcareous tumor filled up the hollow of the sacrum, and displaced the rectum. A portion of this tumor, as well as several other calcareous concretions, were examined for the author by Dr. Bostock; and an abstract of the results may be seen in our last number but one.

M. Louis, in the "Memoires de l'Academie de la Chirurgie," tom. ii., has given some curious cases of this affection; and Schenkius has collected from the works of ancient authors the histories of some extraordinary cases of *stones* discharged from the uterus during life, and discovered in the organ after death: all which go to prove that the complaint in question is no novelty in the world.

But pathologists do not seem to have made any very early or very efficient inquiries into the nature and origin of these uterine tumors. Dr. Lee considers, from a recent examination of some of the preparations in the Museum of Glasgow, that Dr. W. Hunter was the first who was in any manner acquainted with the nature and peculiarities of these structures. Dr. Baillie was led to suppose, in 1787, that calcareous tumors of the uterus commence in the fibrous form; but he had only met with a single instance of the disease. Bayle, Bichât, Roux, Breschet, Andral, and others, have also, in some respects, admitted the same hypothesis; but Dr. Craigie, in his Pathological Anatomy, doubts whether the tumors be not the production of the mucous chorion.

Dr. Lee is of opinion that the greater number of these tumors never exhibit a muscular or fleshy appearance at any period of their existence (as M. Bayle supposes they do,) but that they have a fibrous structure, equally distinct, when not larger than a pea, and when exceeding in magnitude the head of the human adult.

There are sometimes several of these fibrous tumors present together in the walls of the uterus; and not unfrequently they are combined with cysts and tumors of the ovaries. They have no disposition to degenerate into the malignant form, although in persons who have cancerous affections of various internal organs. There is no reason to believe that they ever exist previous to puberty. M. Bayle says that they are most commonly met with in women in whom the physical signs of virginity are present; and that in 20 out of 100 women who have reached the middle period of life, the fibrous tumor is found on dissection embedded in the walls of the uterus. Dr. Lee has met with no case in which those tumors have been found in the fallopian tubes; and Dr. Baillie states that their existence in that situation is a very rare occurrence. Their proper locality seems to be either in the cellular membrane, under the peritoneal coat, or between the layers of the muscular or middle coat, or immediately below its internal or mucous membrane. It is not until they have attained considerable size that their existence can be guessed at during life: they then produce all the injurious, and ultimately fatal, effects of enlarged ovaries, by interrupting the circulation, and pressing on the bladder and other contiguous viscera.

Chaussier, in 1813, presented to the School of Medicine in Paris the uterus of a woman who died in labour, in which there was contained a fibro-cartilaginous tumor, as large as the fist, embedded in the walls of the neck of the uterus. It had formed such an obstacle to labour, that the head of the child was crushed to pieces in its passage through the pelvis. Several similar cases have been noticed by Dr. Merriman, the late Dr. Gooch, M. Deneux, and others.

When embedded in the muscular fibres of the uterus, the organ becomes hypertrophied, and greatly altered in shape. Dr. Lee exhibited a preparation, in which a fibrous tumor was situated under the peritoneum, and a layer of muscular fibres of the fundus uteri, which projects upwards. The patient died of other disease, at the age of 46.

Fibrous tumors occurring in the proper structure of the uterus seem to entail sterility, or at least an abortive habit, the organ being rendered incapable of the necessary developments. Death also may occur under such circumstances from uterine hæmorrhage, the body of the uterus being destitute of the power of contracting. Some cases of this description were related by Dr. Lee.

With regard to diagnosis, it can perhaps only be said that we may suspect the existence of fibro-calcareous tumors in those individuals who, being advanced beyond the middle period of life, suffer habitually from leucorrhœal discharge, and from menorrhagia, with sense of weight in the uterus and adjacent organs. The disease called irritable uterus is sometimes referable to the presence of fibrous tumor in the organ.

When the tumor is developed beneath the lining membrane of the uterus, it distends the cavity like an ovum; but by the powerful action of the muscular coats it is gradually forced into the vagina, where it

may undergo various changes of structure. Sometimes it becomes highly vascular and inflamed; it ulcerates and sloughs, and gives rise to most of the symptoms of malignant disease of the uterus.

A case of this kind was met with in the Marylebone Infirmary, in February 1832. The patient was 44 years of age, and had suffered for many months from pain in the hypogastrium, with profuse discharge of serum, blood, and pus, from the vagina. She had a peculiar sallow complexion—had become much emaciated—and died with the usual symptoms of malignant fungoid disease of the os uteri. On examination after death, the uterus was found to contain coagulated blood. Beneath the peritoneum of the fundus was seen a pediculated fibrous tumor, the size of a walnut. From the mucous surface there grew a pyriform tumor, the greater part of which passed through the os uteri, and filled the upper part of the vagina. In the muscular tissue of the uterus was another tumor, the size of a large walnut. The orifice of the uterus and the vagina were in a healthy state. A preparation of this very interesting case was exhibited, shewing distinctly the different localities which the fibrous tumor of the uterus may occupy in the progress of its development, and demonstrating, that what is generally termed polypus of the uterus is nothing but one of these fibrous tumors under the lining membrane. Dr. Lee shewed another preparation, which was presented to him by the late Mr. Wood, which, however, demonstrated satisfactorily that all polypi of the uterus have not this origin, and that the mucous polypus of Herbiniaux is a disease of the glands of the os and cervix uteri. Another beautiful preparation and drawing were shewn, to prove that there is a tumor sometimes formed under the lining membrane of the fundus uteri, which might be termed polypus, but which is essentially different in structure from the fibrous tumor previously described in the paper. This tumor consists of a congeries of small cysts, filled with a transparent fluid, the whole being covered by the lining membrane of the uterus.

In the subsequent part of the paper, Dr. Lee related cases, and exhibited preparations, to prove that when the fibrous tumor is covered only by the lining membrane of the uterus, the peduncle is slender, and consists only of lining membrane, a few small blood-vessels, and cellular substance; but that the peduncle is short and thick when the tumor has pressed before it a layer also of the muscular coat of the uterus. He is disposed to believe that it is not on the primitive state of the tumor, or polypus, as Herbiniaux supposed, that the consistence and form of the peduncle depend, but on the quantity of muscular fibre carried before the tumor. If a ligature be applied around the root of one of these fibrous tumors or polypi, it is evident, if his views be correct, that a portion of the uterus itself must be removed; and he thinks this will account, in some measure, for the fatal attacks of phlebitis, and other affections, which sometimes succeed to the removal of uterine polypi by the ligature and other methods.

When the tumor is large, and is attached to the fundus uteri by a thick neck, Dr. Lee gave cases to illustrate the fact that inversion of the organ, in the unimpregnated state, is sometimes produced by it. He also related a case which occurred at the St. Marylebone Infirmary, in which a fibrous tumor, as large as a child's head, had passed suddenly from the vagina, and given rise to appearances which strikingly resembled inversion of the uterus. The preparation of the tumor clearly showed that its neck consisted of the lining membrane of the uterus, and of a thick layer of hypertrophied muscular tissue, which had been pressed before it into the vagina.

These laborious researches on the pathology of fibrous tumors and polypi of the uterus, are, we conceive, highly calculated to dispel much of that obscurity which has hitherto enveloped their origin and their treatment.

DR. RIGBY'S

MIDWIFERY HOSPITAL REPORTS.

GENERAL LYING-IN HOSPITAL.

[Concluded from p. 368.]

PUERPERAL FEVER.

Scarcely any serious cases of puerperal fever have occurred during the year, except in an out-patient, of which case I unfortunately took no notes. Incipient symptoms, viz. abdominal pain, the uterus swollen and tender, the milk and lochia suppressed, great anxiety, &c. have shewed themselves in several instances; but they were instantly checked by a large linseed-meal poultice, a dose of calomel and pulvis antimonialis, and injections of warm water into the vagina. The following case, among several others, is a specimen.

April 11th, 1833.—Frances Clarke, æt. 31; delivered of a girl; third child; first position.

13th.—Abdomen has become very tender; the uterus is swollen and hard; the pain, which is confined to it, is without in-

termission, and greatly increased on pressure. Face flushed; headache; tongue coated with a thin white fur; pulse 106; lochia sparing and fœtid; several portions of putrid coagulum have come away. A linseed-meal poultice, which had been applied before I saw her, having produced but little relief, I ordered eighteen leeches to the part, followed by another poultice, and a powder of hyd. submur. gr. viij.; pulv. antim. gr. iv. The pain soon became remittent; this was succeeded by distinct intermissions, the duration of which increased. This being the case, and the pulse having become soft and slower, she had twenty drops of liq. opii sedativ. ex aquâ menth. pip., and the vagina was washed out with an injection of warm water at night-time.

14th.—Feels easy and comfortable; little or no pain of abdomen; pulse soft; tongue cleaner; has passed a comfortable night; bowels opened two or three times during the night. This morning has had a good deal of diarrhœa, with tenesmus.

℞ Hyd. c. Cretâ, gr. iij.; Pulv. Ipecac. comp. gr. vj. statim.

15th.—Quite easy.

Perhaps nothing is of greater importance towards forming a correct view of a case of puerperal fever, than accurately noticing the *character* of the pain; the pulse itself is not a surer guide. Were we not to attend to the character of the pain, it would be nearly impossible to distinguish the gradual transition which frequently takes place, from common after-pains to actual inflammation. We frequently see cases where, if the severity of the pain and quickness of the pulse alone were considered, we should have instant recourse to the lancet, and yet where we find that an opiate will scarcely ever fail to relieve the one and moderate the other. This is *not* inflammation; the pain is not constant; it has either complete intermissions, or at least evident remissions; and by pressure very gradually applied, so far from being increased, it will even be relieved. On the other hand, the pain of inflammation is constant; and apply the pressure how you will, it invariably produces an increase of suffering. Thus, when we succeed in diminishing an attack of inflammation, more especially uterine phlebitis, the first change will be seen in the *character* of the pain: at first it becomes remittent; by degrees it intermits entirely for a few moments; the intervals become longer and more frequent; and thus it gradually disappears.

The state of the lochia, and vaginal discharges in general, have not received that attention which I think they merit, and in many instances are productive of much constitutional derangement. Where, from weakness, or a mistaken notion that it is injurious to rise from the horizontal posture, the patient has preserved this position for some time after labour, the lochia is apt to collect in the vagina, as also any coagula which would otherwise have been expelled. From the temperature of the parts, and the action of the external air, they soon become more or less putrid, and cannot but exert a highly prejudicial effect upon the system. In many cases of uterine phlebitis, which is by far the most common form of puerperal fever which occurs in this metropolis, I have distinctly traced the inflammation of the uterine veins to this cause; and for the last three years have made a point that, wherever it was possible, the patient should sit up in bed to suckle her child, and *always* kneel to evacuate the bladder. By thus occasionally assuming the upright posture, the lochia has had the opportunity of draining away, and any coagula lodging in the passages have been expelled. The relief thereby afforded I need not describe, for it is too well known to every practitioner. Wherever there has been an attack of abdominal pain after labour, with the slightest fœtor about the patient, injections of warm water into the vagina have been had recourse to, and in many severe cases of puerperal fever have proved a most invaluable remedy; the tenderness and hardness of the uterus have subsided, the lochia has returned, and an amendment in all the symptoms, so marked, that in one almost hopeless case the patient would not allow the syringe to be out of her reach.

During the months of June, July, and August, when the weather was at times very sultry, a number of patients were seized, after labour, with intense pain of the head, great heat of surface, pulse above 130, and an irregular patchy efflorescence, occasionally attended with sore throat. The duration of the rash was very irregular, going in and re-appearing two or three times during the attack. In some cases no desquamation of the cuticle followed, but in others it was as extensive as after a smart attack of scarlatina; in some the rash was scarcely perceptible, although the heat of skin was intense; in others the rash was very vivid, and the symptoms mild.

I have some hesitation in giving the following case, on account of the unavoidable minuteness of its details, and great length; but it is so interesting, that I trust I shall be excused occupying so much of your valuable pages.

Aug. 7th, 1833.—Elizabeth Gray, æt.

29, delivered of a girl; second child; third position. Came in on the 18th July; a delicately-formed, almost Creole-looking, Irishwoman. Over the umbilicus I could hear the peculiar whizzing sound of the placental circulation, corresponding with her own pulse; and a little further to the left I could distinctly hear a small clear pulse, beating 32 double strokes in the quarter of a minute, or 256 in a minute. No pains coming on, she left the hospital. Returned on the 7th of August. Could not hear any fœtal pulsation, but the placental sound was in the same place. Os uteri about an inch open; head low; no liq. amnii between it and the membranes; sutura sagittalis crossing in the right oblique diameter; a fontanelle to be felt towards the left foramen ovale, but could not reach it sufficiently to ascertain which it was. The child was born with the face turned towards the back part of its mother's left thigh. Had a slight attack of fever, without pain of abdomen; it partook much of the scarlatina character of the febrile attacks which have lately prevailed.

16th.—Has been purged during the night, with tenesmus.

℞ Pil. Hyd., Pulv. Ipecac. comp. aa. gr. v. M. ft. Pil. ij. statim sumend.

17th.—Bowels quieted by the medicine.

19th.—Bowels have been again severely relaxed, by which she is much weakened. Pills repeated without effect. At half-past five P.M. she had a smart fainting fit; I saw her at six. The countenance much altered, the eyes sunk, and the skin beneath them very dark; the lips as if tightly strained upon the teeth. The evacuations which poured away from her were of the true cholera character; the pulse was very weak, but the extremities were warm. Some tinct. cardam. comp. and tinct. catechu, were given, but were soon rejected great weakness; four grains of calomel, and one grain of opium, were taken, and the astringent mixture repeated; but these were again rejected. She was removed to a room by herself. At seven P.M. an effervescing draught, with a little sp. ammon. arom. and tinct. cardam. comp. was given, and seemed to quiet the stomach and give much relief; pulse very small and weak, the artery feeling evidently reduced in size. This was repeated, with some tinct. catechu, but soon after rejected. Effervescing draughts of sodæ carb. ʒss.; acid. tart. ϑj.; potass. chlorat. gr. x.; and a little sp. ammon. arom. and tinct. cardam. comp., with a small quantity of fluid, were repeated several times, but she continued to vomit and purge during the intervals. The hands became cold and moist, the fingers shrivelled, the skin dark, and the nails livid; the thirst now was excessive; the pulse scarcely perceptible, except during the expirations, and then feeling like a small thin thread: the face became cold, and the tongue also. She drank several large draughts of toast and water, and barley-water in which a quantity of sodæ carb. and potass. chlor. was dissolved. A starch enema, with laudanum, had been retained, and kept the bowels quiet for a time; but now large quantities of rice-water fluid came away at short intervals. Half a table-spoonful of sodæ mur., with tinct. opii, was dissolved in a small quantity of warm water, and thrown up the rectum, but it returned immediately; this was repeated two or three times, but not retained. A large sinapism, which had seemed to relieve her at first, was again heated and applied to the abdomen; and she was allowed to drink as much of the above-mentioned drinks as she wished, adding some sp. ammon. aromat. to every cupful. The stomach now remained quiet for some time; and some egg and brandy were given. The breathing became gradually less anxious, the pulse more perceptible, and the icy coldness of the hands abated. A small dose of hydr. c. cretâ was given about four in the morning; and the egg and brandy continued in small quantities. The cramps in the legs also ceased, and she slept for a short time. As morning approached she slept again; the pulse rose perceptibly; the hands became warm; no vomiting or purging. The egg and brandy were continued with evident benefit; has drank very largely, and slept; voice better.

At seven she had a copious evacuation of rice-water fluid, mixed with small flakes of membrane-like matter, and here and there a small trace of well-coloured fæces. She continued during the day without vomiting or purging; at four P.M. she felt an inclination to evacuate the bowels, but nothing came away except a little bloody fluid, which proved to be lochia. In the evening she had a fæculent evacuation; she took freely of egg and brandy, and barley-water; pulse full and soft, but weak.

℞ Quinin. Sulph. gr. ij. h.s.

21st.—The change is very remarkable; the features are recovering rapidly, although the line under the eyes is still very deeply coloured; pulse full and soft; feels very comfortable, but weak. Took some more quinine, with sulph. acid, and five grains of hyd. c. cretâ, during the day.

23d.—Bowels have been relaxed; still she is comfortable, and gains strength.

℞ Pil. Hyd., Pulv. Ipecac. comp. aa. gr. v. M. ft. Pil. ij.
℞ Quinin. Sulph. gr. ij. mane et nocte.

So much has been said on the subject of cholera, that I shall venture but very few observations. The catechu evidently did harm; and I cannot help thinking that if the salines had been given earlier, they would have succeeded better. The treatment which appears to have been of greatest service in this case, was the powerful sinapism to the abdomen. So far from irritating the stomach, the copious draughts of cold fluid produced great relief.

The lochia returning so soon is curious, and shews how quickly the functions return to their action, when once the equilibrium is restored. This was one of the two only cases of cholera which occurred in the hospital; the other patient was the night nurse, a delicate, unhealthy woman; and this case terminated fatally in twelve hours.

The last case with which I shall trouble you, is one of those remarkable instances of self-deception which are not very common, and which sometimes put our powers of diagnosis severely to the test.

July 18th, 1833.—Sarah Smith, æt. 22, a coarse, dull-featured young woman, rather inclined to corpulency; mother of two children; has not been pregnant for four years; came in this evening with pains in the abdomen, which she considers as labour pains; menstruated last October; was upon the labour-bed when I saw her.

As the pains were chiefly in the abdomen, which was very soft, and as her bowels had been opened at two P. M., I ordered her a draught of liq. opii sedat. et liq. antim. tart. aa. ℥xv. ex aquâ menth. pip.; and being undressed, I applied the stethoscope to the abdomen. I tried the instrument, and then the ear alone, but in vain; for I could distinguish neither the fœtal nor the placental circulation. As I remarked that the abdomen inclined very much to the side which was lowest, I asked her if it appeared to roll about as she moved, suspecting that the child was dead. My suspicions were confirmed by her saying that it felt like a heavy weight, which tumbled over as she turned from side to side. Upon feeling the abdomen it appeared unusually soft, although fuller and larger than natural; but having little confidence in an examination through the abdominal parietes, I concluded that this was from the flaccid state of the uterus and its contents (supposing the child to be dead), and took no further notice, waiting for the event of her labour to confirm or contradict my diagnosis. In about an hour afterwards the midwife examined per vaginam, to ascertain the presentation before I left the hospital; and feeling something unusual, requested me to examine. To my surprise I found the cervix uteri at least two-thirds of an inch long, firm, the os uteri slightly open, its edges forming a long fissure running transversely; in other words, the mouth and neck of an unimpregnated uterus. I examined her again standing, and convinced myself that she was not pregnant.

I had her lie upon her back, with her knees elevated; and having now once more carefully examined the abdomen, ascertained that the uterus was not above the pubes. The spleen felt large and hard, but I could not satisfactorily detect any other prominence. Two days afterwards she had a slight feverish attack, which was relieved, and she left the hospital. This patient was so convinced that she was pregnant, that she could only be induced to suppose her labour would not come on quite yet; and with this impression she went out. I need scarcely add, that she has not returned.

The stethoscope here was so far useless, that it only justified me in supposing the child was dead; for until I had examined the state of the os uteri, I could not decide with certainty as to the absence of pregnancy. Inclined as I am to give the stethoscope its due share of usefulness, an accurate knowledge of the difference between the os and cervix of the unimpregnated and gravid womb, is of the utmost importance to the accoucheur; and attention to every variation in their form and size cannot be too strongly insisted upon.

APPARENT DEATH,

Which was continued for Twenty Days.

By Dr. Schmid.

A young man died in the hospital at Paderborn, who could not be buried until three weeks after he had breathed, at least to all appearance, his last breath. It was not till the twentieth day that the characteristic phenomena of death became manifest. The circumstances of the case were these:—This young man had been a little time before cured of a tertian ague, when he re-entered the hospital, presenting some signs which caused an apprehension of phthisis, without however presenting any well-marked symptoms of this disease. In other respects, no disturbance in his health. On the day he died, his eyes were suddenly opened, and for some minutes we found an irregular beating at the pulse. Several small wounds resulting from cau-

terizations, to which we then had recourse to rouse him, suppurated the second, third, and fourth day. On the fifth, the hands of the body were turned back: from the fifth to the ninth day there exhaled from half the body an abundant sweat free from odour. Towards the end of the ninth day, there appeared over a considerable part of the dorsal region serous bullæ, similar to those of pemphygus. The limbs still preserved their natural suppleness; and on the eighteenth day, the lips still retained their red vermillion colour. For nine days the forehead continued furrowed with vertical wrinkles, and all this time the countenance preserved an expression never presented by the face of a dead body. The body was kept for nineteen days in a warm room; it exhaled not the least fœtid odour, and there was observed on no part of its surface any cadaveric lividity. The emaciation was not very considerable, a circumstance which, if it had existed, might have served to explain these different phenomena.—*Hufeland, Journal de Pract. Heilkunde;* and *Dublin Journal,* May 1834.

WOUND OF BOTH FEMORAL ARTERIES,

Without Hæmorrhage.

An artilleryman, while standing with his legs asunder near a cannon which he was preparing to sponge, was struck by a bomb, which burst, passed between his thighs from behind forward, without fracturing either femur, and tore all the soft parts situated at the middle third of the internal and posterior side of these members. No hæmorrhage ensued, though the two femoral arteries appeared amid the havoc of the soft parts, completely divided, and exhibiting a motion altogether erectile. The pulsations of both were very perceptible superiorly, till within three or four lines of the divided extremities, where a sort of resistance to pressure was felt, as if the orifices of the two arteries had been stopped by a foreign body. It was true torsion of the vessels. It is almost needless to add, the patient did not survive.—*Edinburgh Med. and Surg. Journal,* April 1834.

ANOTHER CONCOURS.

Another concours is being held at Paris, to fill a vacancy which has occurred in the *Bureau central d'Admission.* There are six candidates—MM. Danyau, Halma-Grand, Lenoir, Lepelletier de la Sarthe, Malgaigne, and Sanson, junior.

WEEKLY ACCOUNT of BURIALS,

From Bills of Mortality, *June* 10, 1834.

Abscess	4	Hooping-Cough	10
Age and Debility	35	Inflammation	36
Apoplexy	9	Bowels & Stomach	1
Asthma	12	Brain	2
Cancer	6	Lungs and Pleura	7
Childbirth	4	Insanity	7
Consumption	92	Jaundice	1
Convulsions	30	Liver, diseased	3
Croup	1	Locked Jaw	2
Dentition or Teething	12	Measles	10
Dropsy	17	Mortification	3
Dropsy on the Brain	22	Paralysis	8
Dropsy on the Chest	1	Scrofula	3
Dysentery	1	Small-Pox	1
Epilepsy	2	Spasms	1
Erysipelas	2	Stone and Gravel	1
Fever	10	Stricture	1
Fever, Scarlet	10	Thrush	1
Hæmorrhage	1		
Heart, diseased	4	Stillborn	28

Decrease of Burials, as compared with the preceding week . . . 16

METEOROLOGICAL JOURNAL.

May, 1834.	Thermometer.	Barometer.
Thursday . 29	from 35 to 67	30·10 to 30 09
Friday . . 30	39 70	30·07 30·06
Saturday . 31	43 69	30·08 30·12
June.		
Sunday . . 1	49 77	30·17 30·24
Monday . . 2	50 80	30·19 30·10
Tuesday . . 3	48 70	29·99 Stat.
Wednesday 4	41 65	29 95 29·78

Wind variable, N. E. prevailing.

Clear till the 3rd; rain at times on the 3rd; and a steady rain during the evening of the 4th.

Thursday . 5	from 40 to 69	29·76 to 29·89
Friday. . . 6	42 69	30·00 30·06
Saturday . 7	43 69	29·98 29·92
Sunday . . 8	41 72	29·86 29·81
Monday . . 9	40 69	29·74 29·70
Tuesday . 10	49 69	29·66 29·64
Wednesday 11	46 63	29 64 29·63

Prevailing wind, S.W.

Showery during the 10th and 11th, otherwise generally clear.

Rain fallen, ·275 of an inch.

Charles Henry Adams.

NOTICES.

We must decline the publication of M. de Moscati's letter. Dr. Elliotson's reply to the *professional* part of M. de M.'s pamphlet we thought it but fair to give,—setting before the reader both statements at the same time: but this is no reason that we should now extend our courtesy to the admission of a rejoinder. We refuse to lend our pages to the effusions of bitterness.

Cosmo's paper is at our publishers', with the desired direction.

The signature of a Junior Student is appended to a well-intentioned paper; but both the subject and manner of treating it are unsuited to this journal. We suppose the writer belongs to some Tract Society.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JUNE 21, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XXXVII.

DIFFICULT LABOUR.—INSTRUMENTAL.

When we reflect for a moment on the horror that any man must experience when about to perform the operation of craniotomy,—if he entertain the least suspicion that the child is alive,—we cannot wonder that many plans have been suggested for the purpose of superseding the necessity of adopting it. The principal of these are four in number:—the *Cæsarean operation;* the *section of the symphysis pubis; controlling the growth of the fœtus in utero;* and the *induction of premature labour.*

THE CÆSAREAN OPERATION

consists in dividing the abdominal parietes, cutting into the cavity of the uterus, and extracting the child, placenta, and fœtal membranes, through the incision thus made.

History.—The name which designates this operation seems to be derived from the mode in which it was performed,—by cutting instruments applied to the mother's person, *à cæso matris utero,*—and it would appear that it was called THE Cæsarean operation, κατ' εξοχην, in consequence of the extensive incision necessary, the delicacy of the structures divided, and the danger attendant upon it.

The earliest writers on medicine are silent on this subject; thus, no mention of it occurs in the works, either genuine or spurious, of Hippocrates, nor of Celsus, Paulus Ægineta, Avicenna, or Albucasis. Pliny, indeed, tells us, that the elder Scipio Africanus—the vanquisher of Hannibal—who dictated humiliating terms of peace to Carthage, the formidable rival of Rome—was introduced into the world by this operation; from which circumstance he, of all the Romans, was first distinguished by the appellation of Cæsar; and that Manlius Torquatus owed his life to the same means.

It is generally supposed that Julius Cæsar was the conqueror ushered into life in this unnatural manner, and that from him first sprang the surname which the Roman emperors inherited. If this were the case, his mother Aurelia must have survived the operation, since she died while he was prosecuting the war in France; or, according to Suetonius, while engaged in the conquest of Britain. There is nothing, however, in history to warrant us in adopting this belief, nor, indeed, positively to refute the assertion of Pliny.

If we were inclined to dip into mythological mysteries, and trace the operation to its remotest origin, we should learn that the God of Physic himself,—as is sung by Ovid,—was cut out of the womb of his mother Coronis, by Apollo, after he had destroyed her by an arrow, for her infidelity. The grave assertions of the naturalist, indeed, in this respect, may be as fabulous, possibly, as the visionary flights of the poet; but there is no question that, even before Ovid's time, the operation had been put in practice on the dead subject.

The earliest account of it extant in any medical work, we find in the *Chirurgia Guidonis de Cauliaco,* which was written about the middle of the fourteenth century; but the author only speaks of it as resorted to after the mother's death. The same mention is made by Parè, who limits its adoption to those cases in which the woman died undelivered. Rousset, however, who was cotemporary with Parè, published in 1581 a separate work on the

subject, entitled *Traité Nouveau de l'Hystero-tomotokie, ou Enfantement Cæsarien.* This was translated into Latin, and a supplement added, by the celebrated Caspar Bauhine, in 1601, and contains a number of cases in which the operation was followed by the recovery of the woman. The first successful operation, according to Bauhine, was performed at Siegenhausen, by a cattle-gelder named Alespachen, on his own wife, about the year 1500. She afterwards bore several children naturally.

About the middle of the next century, Olaus Rudbecke, who was a professor of physic at Upsal, the oldest university in Sweden,—and founder of the Botanic Garden there, which became afterwards the scene of Linnæus's labours,—a skilful anatomist, and supposed to be the discoverer of the lymphatics in the liver,—is said to have performed the operation so successfully on his own wife, as to have saved both her and her child.

Principally, perhaps, owing to Rousset's publication, it became very frequently, and, indeed, generally adopted, in different parts of the Continent. Baudelocque has, at some pains, collected a mass of well-authenticated cases; and he has recorded 136 recoveries out of 226 operations.—M. Simon wrote two papers on the subject, in the *Mem. Roy. Acad. de Chirurgie* of Paris, in which he gives an account of sixty-four successful cases; and he mentions thirteen women, on all of whom the operation had been repeated; some having undergone it five or six, and one even seven times.

There is no question that many of the cases to which implicit credit has been allotted, are not founded in truth; and, among others, we may enumerate the instance of Jane Seymour, wife of Henry the Eighth: for although Dionis and Mauriceau, in France, as well as Hull, and some others, in this country, give credence to the rumour, there is little doubt that the story was fabricated, to swell the list of that licentious tyrant's barbarities. Rapin states that the unfortunate queen died two days after her labour. There is every reason to believe that Hume's and Lingard's account is more correct, and that she survived twelve. If so, it is very improbable that the Cæsarean operation was performed on her person. The story runs, that it was supposed a natural termination of the labour could not take place; and the officiating attendant, on informing Henry of the circumstance, inquired of him whether he willed that the mother's or the child's life should be saved; to which he replied, with his accustomed coarseness and brutality, "Save the child by all means, for it is easier to get wives than children." Such rumours may have no better foundation than the license which is on all hands granted to poets; and on which the bold daring of Shakspeare's hero, Macbeth, was principally built, as exemplified in the celebrated passage—

Macbeth.—"I bear a charm'd life, which must not yield
To one of woman born."
Macduff.—"Despair thy charm;
And let the angel whom thou still hast served,
Tell thee, Macduff was from his mother's womb
Untimely ripp'd."

O'Meara relates that the labour of the Empress Marie Louise also was lingering, and it was feared either that the child must be sacrificed, or the Cæsarean section performed; that Dubois put the same question to Napoleon, who desired him to forget the Empress's station, "and to treat her as he would a shopkeeper's wife in the Rue St. Denis; but if one life must be saerifieed, to save the mother."

I have before mentioned, that out of nearly thirty instances in which this operation has been performed in the British Isles, in two only has it proved successful, as far as the preservation of the mother was concerned; and I endeavoured, on that occasion, to account for the great disparity in the result between these and the continental cases. I have also attempted to lay down a rule, limiting the cases of pelvic distortion, or tumors, in which it may be necessary. Bearing in mind, therefore, that in Britain we never substitute it for craniotomy by choice, but only have recourse to it when no other mode of delivery is practicable, I shall proceed to consider the most approved method of performing it.

Mode of performance.—The patient need not be removed from the bed; but lying on her back, with her head and shoulders raised by pillows, she should be brought to the edge, so that her feet may hang down towards the floor. The bladder having been perfectly emptied, and the apartment brought to a temperature of at least 80 degrees of Farenheit's thermometer, an incision of about six inches in length must be made through the abdominal parietes below the navel, parallel with the linea alba, a little on the right or left side of that line; to be determined by the convenience of the operator, and other circumstances of the case. Another incision, similar to the first, must be made into the cavity of the uterus, the hand introduced, the membranes torn, and the child extracted by the feet, with all convenient speed; the placenta must be abstracted also through the same opening, as quickly as is consistent with safety. On the uterine cavity being evacuated, the organ will con-

tract more or less perfectly; hæmorrhage will thus be prevented, as well from the divided vessels as those over which the placenta had been attached; and there will be no need of sutures to bring the edges of the uterine wound together. The abdominal parietes, however, will require two, or perhaps three, sutures: the surfaces must be dressed according to the common principles of surgery; the heat of the apartment gradually lessened; a tolerably powerful opiate administered; and other means used to allay irritability, and avert inflammation or fever. A warm bath should be in readiness, and the proper requisites prepared to resuscitate the child, if animation be suspended.

The cautions which we have particularly to attend to in the performance of this operation, are, *first*, not to divide the tendinous expension of the recti muscles forming the linea alba; because we should not expect union to take place so kindly in that lowly organized structure, as in the body of the muscle itself; nor to make the incision so much towards the side as to endanger wounding the epigastric artery. *Secondly*, not to allow the naked surface of the uterus to remain exposed for a longer time than can be helped, and especially not to handle the organ more than is absolutely neeessary. *Thirdly*, not to make the incision at the side of the uterus, that the largest vessels may not be divided. *Fourthly*, not to let much time elapse between the extraction of the child and placenta; and, *fifthly*, to be most careful that none of the intestines become strangulated, by passing through the aperture into the uterine cavity.

And the dangers which we have to fear, are, the exeessive shock which such a formidable incision must produce on an unhealthy, debilitated, and perhaps exhausted frame; hæmorrhage both from the uterine vessels and those supplying the abdominal parietes, and subsequent inflammation.

It is very possible that the patient may sink rapidly after the operation, from the sudden shock experieneed by the nervous system; but this has seldom occurred. Less frequently still has it oeeurred that hæmorrhage has destroyed; for, eontrary to what we might *à priori* have expected, the bleeding has generally been comparatively trifling. Hull tells us that in both his cases the loss of blood was but small. The same remark applies to two of Mr. Barlow's cases, although in one the placenta was attached directly over that portion of the uterus through which the incision was made. Lauverjat, a French author, towards the end of the last century, who performed the operation five times,—three of which cases terminated favourably for the mother,—recommends even that the loss of blood should be artificially promoted, by separating the placenta, and placing a warmed drinking glass over the denuded surface; "that such a quantity of blood may escape as is judged necessary to unload the uterine vessels." He confesses that this advice is very different from that previously given by those authors who had written on the subject; for they all, fearing excessive hæmorrhage, insist on the necessity of avoiding the placenta. Lauverjat, indeed, may possibly have carried his principle to too great an extent; but as he had himself operated in five cases, his opinion proves that the principal objection of Parè and others, who dreaded the excessive hæmorrhage they fancied must ensue, was speculative and hypothetical.

By far the greatest number of deaths have happened from inflammation supervening, and in some cases terminating, in gangrene: such, then, is the evil we have principally to dread. It was suggested by the ingenious Dr. Aitkin, of Edinburgh, that the injury produced on the peritoneum was the effect not so much of the violence it suffered from the incision, as of the introduction of atmospheric air into the abdominal cavity, and the irritation consequent on its admission; and he proposed—to obviate this chance of danger—that the operation should be performed while the patient was in a warm-bath. I am not aware whether his suggestion has ever been adopted; but I am inclined to think the inconvenience attending such a mode of proceeding would render the operation much more difficult and complicated. Besides, it is very questionable whether his position be correct; for the abdominal cavity in dogs and other animals has been injected with air introduced into the tunica vaginalis, and passed through the ring, without any other inconvenience being sustained beyond what the bulk and distention produced; and it has been found that in time the elastic fluid was absorbed. From these experiments, as well as from observations on the human subject in the case of accidental wounds of the abdomen, there is good reason to think that the cause of danger is the actual incision, and not the admission of atmospheric air.

Prevention of future conception.—Blundell suggests, if we are called upon to perform this dreadful operation, that we should endeavour to prevent the possibility of the woman again conceiving; under the belief that a married woman will not have sufficient resolution to separate herself from her husband's bed, although her life would be likely to be the forfeit. He therefore proposes, that after the child is extracted, we should destroy the continuity of the

fallopian tube on each side, by removing a small portion of its substance. By this means we should not take away desire, but prevent the possibility of conception. As a reason, he states, that, on the continent, the operation has been repeated on the same woman in many instances; that Mr. Barlow's patient recovered, and might have again conceived.

Although in Britain we restrict this operation on the living subject to such extreme disproportion as must render its performance very infrequent indeed, yet the case is widely different when the mother has expired, and any suspicion is entertained of the child's survival. Should sudden death occur in labour, or during the last two months of pregnancy, it would be the bounden duty of the attendant surgeon, after having stated to the friends the probability of saving the child's life, to proceed, without delay, to extract it by the abdominal incision; and if such means were used within twenty or twenty-five minutes of the mother's decease, the result would most likely be favourable.

SECTION OF THE SYMPHYSIS PUBIS.

A second means proposed for the purpose of superseding the necessity of the destruction of the child, is the division of the symphysis pubis, called, after the name of the proposer, M. Sigault—the Sigaultean operation.

Until of late years, it was supposed that the ligaments of the pelvis gave way during parturition; and impressed with these sentiments, and having imbibed the opinions of one of the older anatomists, "*ossa pelvis tutò secari possunt*," in 1768, M. Sigault, then a student, proposed to the Royal Academy of Paris to enlarge the pelvis under contraction, by cutting the symphysis pubis. The suggestions were referred to a committee of that learned body; but these gentlemen having taken into consideration that when the bones were separated by disease, the effects of that separation were dreadful, and that permanent lameness was the result, reported that the operation was not justifiable, but gave the proposer great credit for his ingenuity. Although, however, Sigault received such a rebuff at the outset, he was not to be deterred from his purpose; and in September, 1777, he, assisted by M. Alphonse de Roy, performed the operation on a patient named Souchet. The operation is described as being simple; the child was born alive, and in six weeks the patient was shown to the medical faculty quite recovered. But notwithstanding the supporters of symphyseotomy boasted of the case as one of perfect recovery, the bladder was so much injured that she was never able to retain her urine so long as she lived. Among some manuscript papers of the late Dr. Dennison, which came into my possession, I find a note that he had seen this patient while in Paris, and should have considered her quite well, "had not his nose informed him that she could not retain her urine."

The praise bestowed on M. Sigault in Paris and throughout France was quite unprecedented; a medal was struck, by order of the faculty of Paris, bearing a motto, "*Nova Lucina*," to commemorate the event; a royal pension was granted to him, and the applause he received was perfectly extravagant: greater exultation he could scarcely have enjoyed if he had devised a method to remove female nature beyond the pale of all the pains and dangers connected with parturition. In no other country, perhaps, but among our enthusiastic and volatile neighbours, would an operation of the kind, resting on one solitary trial alone, have given rise to so universal a triumph. Soon it was repeated, with various success, on the continent; thirty-six cases are well authenticated, of which fourteen women died, and half the children were still-born. It has only been performed once in Great Britain; Mr. Welchman was the operator, and published an account of the case in the London Medical Journal for 1790.

Dr. Denman states, that when the accounts were brought to England he had a conference with John Hunter on the subject; and it was determined, as far as the safety of the woman was concerned, that if the good contemplated could result, the section of the symphysis itself would not warrant opposition to it; but with regard to its utility, it was necessary that experiments should be made to establish that point. These experiments were afterwards made by Dr. William Hunter, and it was proved, that, by a simple division of the pubes, although the bones spontaneously separated somewhat, very little space was gained; but it required that they should be wrenched asunder, to the imminent danger of the sacro-iliac ligaments and joint, and especially also to the bladder and its attachments. In cases of distortion, I have already proved that the diminution of space is principally in the conjugate diameter; and it was found that, in order to increase this diameter one inch, the pubes must be separated three inches; and to increase it half an inch, there must be a separation to the extent of two inches. It was proved, also, that if a separation of an inch and a half only took place, laceration of the sacro-iliac ligaments occurred; and it was presumed that this must be necessarily fatal.

It must be evident from this statement, that the operation is not justifiable in cases of the more deplorable distortion of the pelvis, and therefore, in England at least, cannot supersede the Cæsarean section: nor is it justifiable in the smaller degrees of diminution, because in them craniotomy can be performed; and we have laid it down more than once as a maxim, that even the life of the fœtus must be sacrificed, if necessary, to preserve the woman's structures from serious injury. The section of the symphysis, then, can neither become a substitute for the Cæsarean operation, nor for craniotomy; it is now never thought of in England as a means of delivery, and is also totally exploded from Continental practice. But an operation somewhat similar has lately been suggested by the Italian physicians, and in one instance at least adopted, of which case a full report will be found in *Il Filatre-Sibezio* of last year. It consists in sawing through the rami of the pubes and ischium, *on each side* of the symphysis, and by this means enlarging the antero-posterior pelvic diameter. The patient's name was Negri, and the operation was performed on 30th March, 1832, by Dr. Galbiati, of Naples. The following summary of the case I give you, because I trust we shall never again have an opportunity of learning the particulars of so horrible an operation.

The patient was 30 years old, and pregnant for the third time, having on the two former occasions brought on abortion voluntarily. Her stature did not exceed two feet and a half in height; she appeared in good health, but had suffered much from rickets. The space between the pubes and sacrum was not more than one inch and a quarter (French) at any one point. These observations were made before her labour commenced, on her first admission into the hospital; and a consultation of the medical profession was held, to determine whether the Cæsarean section should be adopted, when uterine action began; or whether this new operation should be put in practice. The majority were in favour of the latter mode, in consideration of the result of some experiments Dr. Galbiati had made on the dead body. Accordingly, about twelve hours after pains came on, having with some difficulty obtained the patient's consent, he proceeded to the operation. He cut down upon the rami of the ischium and pubes on the right side, as near the acetabulum as he could, detached the periosteum, and divided the bones; he then terminated this step of the operation by symphyseotomy. It lasted an hour and a quarter; no material parts were injured, and the patient did not seem to have suffered much under it. The membranes soon after broke, and the uterus then acted strongly. On the morning of March 31st there did not seem any urgent symptom, but the head could barely be felt. In the course of the day, however, uterine contractions became feeble, and continued so throughout the night. On the morning of April 1st, fever and symptoms of exhaustion had supervened, and the head remained still entirely above the brim. A fresh consultation was held, and it was determined to divide the rami of the bones on the left side, as had already been done on the right. At first she would not submit, but at 4 P.M. the second operation was accomplished. The head was then brought into the cavity of the pelvis by the forceps, and it was discovered that the child was dead; the head was perforated, and the delivery perfected. The extraction lasted half an hour. The patient was much exhausted, never rallied, and died within twelve hours of the termination of the case. A fœtid sanies issued from the vagina during life, and dissection proved that gangrene had taken place within the pelvis to a considerable extent.

The reason Dr. Galbiati gave for cutting away only one side of the pubes in the first instance, was, that he had found by experiments on which he seemed to rely, that if the pelvis contained only one inch (French), both *rami pubis* required removal; but if more, one would suffice.

After this detail, I presume you will none of you be inclined to adopt Dr. Galbiati's method of delivery under contracted pelvis; and I think you will also congratulate yourselves, that notwithstanding the opprobrious epithets which are daily showered on the professional officers of our eleemosynary institutions for the relief of the sick poor, by some portion of the medical press, the records of British surgery no where furnish a parallel to this operation, or to the mistaken calculation which led to its performance.

In the lesser degrees of deformity (putting those aside which would require our having recourse to the Cæsarean section, and which fortunately occur so rarely as to place them almost beyond the bounds of calculation), it becomes a great object that we should be prevented the necessity of being compelled, for the preservation of the woman's life, to destroy each child that she conceives; and with this view the third expedient has been adopted, viz. the

CONTROLLING THE GROWTH OF THE FŒTUS.

It was with justice supposed that if the growth of the child in utero could be suspended, it would pass through the contracted space so much the more readily;

and that this might be accomplished through the medium of the mother's system. The idea first originated with an old friend of my father's, the late Mr. Lucas, of Leeds Infirmary; and Assalini relates a case in which abstinence was usefully enjoined. Analogical reasoning was also brought to bear on the question; and it was argued that if cows were kept in a luxuriant pasture, their calves were much larger and stronger than if their diet was more spare. The same system was therefore recommended in the human subject. This possibly may be the case with cows; but even if it be, the principle, unfortunately, does not hold good with regard to our own species: both the observation of disease, and direct experiment, prove the contrary. We observe that women labouring under the last stage of the most debilitating diseases, such as phthisis, often bring forth plump and well-nourished children. We remark, also, frequently, that the vomiting which usually attends the first weeks of pregnancy continues almost uninterruptedly during the entire period, so that scarcely the whole of any meal is retained upon the stomach; and that the patient becomes much emaciated under the debilitating effects consequent: yet the nutrition of the fœtus is not interfered with, but it is born strong, hearty, and of full size. But the question has been settled by experiments made directly for the purpose; and although occasionally the starving system may possibly have some influence, it is now admitted that it can by no means be generally depended on.

The other method, then, to obviate the repetition of such a dreadful expedient, consists in the

INDUCTION OF PREMATURE LABOUR.

First indicated by nature.—Nature herself first pointed out the most likely means of remedying the evils which disease had entailed. It could not but be observed that when women with small pelves went into labour prematurely, the fœtus passed with little difficulty. Thus practitioners were led to reason on the subject, and to endeavour to induce uterine action before the termination of gestation, in consideration of the amazing growth of the fœtus during the last two months, and the probability that a child born after the completion of the seventh, would be reared. Denman records, that in 1756 a solemn consultation between the obstetrical practitioners in London took place on the subject, in which the morality, safety, and utility of the means, were fully discussed.

As to the *morality*, there can be but one opinion. If the life of the fœtus can probably be saved, and if much danger can be averted from the mother, the morality, as a surgical means of procuring a great benefit, must be self-evident. Should premature labour or abortion be induced, to screen an individual from the slander of the world, or to cast into oblivion the evidence of the gratification of a criminal passion, then, indeed, is murder committed in law and reason: but as our object, under the circumstances now treated of, is to save life, and as probably two beings may at the same time be preserved to society by the means proposed, the profession now entertains no question as to its morality, when imperious necessity dictates it.

With regard to the *safety* also, we must all be agreed; for how much more likely is the woman to survive, having passed a child, after a comparatively short labour, which may weigh 5 lbs. or 5½ lbs., and but little ossified, than if she produce one at the full time, weighing 7 lbs. or more, whose osseous system is well developed, after a difficult and protracted struggle, terminated by instrumental delivery?

The *utility*, indeed, as far as regards the preservation of the child's life, becomes a separate question; and Baudelocque has reasoned speculatively against it. He says, that when the liquor amnii is discharged after the rupture of the membranes, there must be such pressure on the body of the child and the funis umbilicalis as to destroy its life; especially as the os uteri will most likely relax with difficulty. It certainly is true that more children are born dead, after the induction of premature labour, than if they come into the world at the full time; but provided we can snatch only a proportion from death, still our object is in a very great measure gained. Dr. Hamilton tells us, that out of twenty-eight successive cases in which he induced premature labour, he has been able to save twenty-four children; and in one patient he performed the operation upon ten different occasions. Much greater success has attended Dr. Hamilton's endeavours than I can boast of. In my practice, more than one-half have been born alive, and might live to maturity. It has occurred to me during the last twelve years to have been compelled to induce labour prematurely forty times. This may seem, perhaps, a very large number; but it must be recollected that the extensive charity which has supplied the principal part of these cases, embraces the districts of Spitalfields and Bethnal-green, which parishes, I will take upon myself to assert, contain more females with deformed pelves, than are to be met with over the same quantity of square acres in any other part of this kingdom. When, also, it is taken into consideration, that in most of the patients the operation has been repeated, and

that some have undergone it five and six times, the subjects of it will be found to be comparatively few. Out of these forty, one was a twin case; and of the forty-one children, twenty-three were born alive. But suppose even that the child should be born dead, still we are giving to the mother the best, and the child the only, chance of life; and we save the mother, at the same time, a great deal of personal suffering.

Premature labour was first induced with success, in London, by Dr. Macaulay, about the year just mentioned, 1756.

Difficulties in effecting the object.—The difficulties which we have to contend with in endeavouring to save the child under the proposed plan, are certainly great; and the following may be enumerated. *First*, the pressure on the navel-string may destroy its existence, as advanced by Baudeloeque. There can be no doubt that as long as the membranes are whole, however strongly the uterus may act, the pressure on the fœtal body and funis is inconsiderable, owing to the quantity of fluid which the womb contains. But as soon as the water is evacuated, when the parietes of the uterus come into close contact with the body of the child, it is very possible that the funis umbilicalis may suffer such injurious compression as to destroy the child's life; and this will therefore be looked upon as one of the chances militating against success.

Secondly, children are more frequently found to present in a preternatural position previously to the termination of gestation, than after the full time is completed. At a particular period of pregnancy the fœtus assumes a definite posture, from which it seldom after varies. What this precise period is, I have no direct means of judging; probably it differs much in different cases; but the fact is undoubted that *cross births* are more frequently met with under premature labour, either spontaneous or artificial, than in full-timed pregnancies. Of thirty-three cases which came under the knowledge of Dr. Merriman, in which premature labour was induced, fifteen presented preternaturally, and only one of these children was born living. The same observation I have myself made, though the proportion has not been so large; for of the forty-one children just alluded to, fourteen presented preternaturally; and Dubois has recently stated, that in the *Maternité* at Paris, out of 112 children born before the completion of seven months, in 51 cases the pelvis offered itself, and in 5 the shoulder; making a total of just one half preternatural presentations. Thus, then, if the shoulder or breech present, we shall have little chance of saving the child; because,—besides the ordinary cause of danger,—the pressure on the funis umbilicalis must be great when the head is passing the brim; for I presume on there being a want of space to warrant a recourse to the means used. Mr. Barlow, indeed, states that preternatural presentations are more frequently met with under distortion of the pelvis than when that organ is well formed. His is a good practical authority, and this remark also coincides with my own observations. Until I became acquainted with Mr. Barlow's opinion, I looked upon this as an accidental occurrence; and I do not know yet that it is regulated by any precise laws.

The *third* difficulty we have to contend with, is the chance of deception regarding the period of pregnancy at which the operation is performed. Women are very liable to be deceived in their reckoning; they may fancy they are advanced farther than is really the case, and their representations may induce us to bring on uterine action before the fœtus has acquired such a degree of perfection as to enable it to sustain independent existence:—or, on the other hand, the patient may have been pregnant before she was aware of it; and we may delay the operation until it is too late—until the child is of too great a bulk, and too strongly ossified, to pass through the particular pelvis which the woman possesses; and we may consequently, in the end, be compelled to resort to the operation of craniotomy; as has occurred to myself in one instance. Though these difficulties, then, are some drawback to the success of inducing labour prematurely, yet they are by no means such as would induce us to discard the benefits it holds out.

Practised by the Ancients.—It is a fact no less deplorable than astounding, that, previously to the wide spread of Christianity, most of the nations of the globe were in the constant habit of destroying their new-born infants, and that the parents themselves were the abettors or actual perpetrators of this horrid and unnatural cruelty.

Nor was this revolting crime confined, as might have been expected, to those regions only where passion triumphed over reason and morality; but, on the contrary, we find it most frequent in the fairest and best cultivated portions of the globe, and in proportion as the people had advanced in the refinements of civilization.

Three principal reasons may be assigned for the prevalence of this barbarous custom:—religious superstition; fear lest the population might increase beyond the means of subsistence; and the difficulty and trouble of rearing a sickly or deformed infant. Thus, then, the unnatural private feelings of the parent, and the erroneous

policy of the legislator, equally conduced towards the promotion of this enormity.

Both the sacred and profane historians bear ample testimony to the voluntary destruction of the offspring, as well before as after birth. The procuring abortion, indeed, was cultivated as an art by the ancients, particularly the Romans, at the period of their greatest power; and in the Satires of Juvenal we find this custom noticed as almost universal amongst the rich; without however being accompanied by such expressions of horror as might have been expected from his severe and chastening pen. This omission alone would prove in how light a point of view this nefarious act was considered.

The following is the passage I allude to; it will be found in the sixth Satire:—

"Tantum artes hujus, tantum medicamina possunt,
Quæ steriles facit, atque homines in ventre necandos
Conducit. Gaude, infelix*, atque ipse bibendum
Porrige, quicquid erit. Nam si distendere vellet,
Et vexare uterum pueris salientibus, esses
Æthiopis fortasse pater."

Ovid dedicates the 13th and 14th Elegies of his second book of Amours to his mistress, who had endeavoured to make herself miscarry, and persuades her from committing such an act again. He characterizes the art of procuring abortion a vile invention—an accursed design. In the 14th Elegy he says—

"At teneræ faciunt, sed non impunè puellæ,
Sæpe, suos utero quæ necat, ipsa perit."

There can be no doubt that the preservation of personal symmetry, which indeed Ovid pointedly alludes to, and the trouble of a family, were the motives that induced the Roman ladies to have recourse to means for getting rid of the fruit of conception, before its life was made evident to their senses; and it was not till Christianity, by her mild and humane precepts, obtained a sovereignty over the minds of this people, that the custom was abolished.

Tertullian, the celebrated Christian writer of Carthage, in his apology for Christianity, having censured his countrymen in the strongest terms for the practice of murdering their live children, says, "But Christians are now so far from homicide, that with them it is utterly unlawful to make away with a child in the womb, when nature is in deliberation about the man: to kill a child before it is born, is to commit murder by advance; and there is no difference between destroying a child in its formation, and after it is formed and delivered; for we Christians look upon the ovum as a man in embryo; he is a being like the fruit in blossom, and in a little time would have been a perfect man, had nature met with no disturbance."

While the destruction of the ovum was not regarded as a sin to be abhorred, but rather as an art honourable to science, and useful in application, we cannot wonder that the practice, if it had been really devoid of personal danger, should be so extensively resorted to; because we cannot presume that the mother is actuated by the same affection towards the being in her womb, of whose life she has had no evidence, as she must be to her mature infant, brought forth into the world under great suffering and danger to herself, and capable of sustaining an independent life.

It is curious to observe that the very means which the ancients resorted to, for the purpose of procuring the evacuation of the womb, are exactly those which are practised in the present day; for Tertullian, while reprobating the custom, has described the instrument by which the ovular membranes were punctured, in these words—"Est etiam æneum spiculum, quo jugulatio ipsa dirigitur, cæco latrocinio, εμβρυοσφακτην appellant; utique viventis infantis peremptorium." And Ovid, who lived nearly 250 years before Tertullian, alludes to the same operation in the following passage, addressed also to his mistress, who was pregnant by him:—

"Sine crescere nata;
Est pretium parvæ non leve vita moræ,
Vestra quid effoditis subjectis viscera telis,
Et nondum natis dira venena datis?"

* Infelix *marite*.

SCARLET FEVER AN INFLAMMATORY DISEASE.

BY

JAMES ANSLEY HINGESTON, ESQ.

AFTER the perusal of a paper on the Pathology of Scarlet Fever, by Mr. W. Hammond, which appeared in the Medical Gazette, (*see* p. 130, *ante*), I am incited to produce the following observations of my own.

Mr. Hammond considers that disease as inflammatory, and states the appropriate mode of treatment to be antiphlogistic. With that gentleman I agree in his opinion. But *my* observations must be delivered with diffidence, since they affect to be subjoined in corroboration of the testimony of one who is my su-

perior in age, rank, and experience. Nevertheless, it is a test of truth, when the same object appears in the same light to two different observers from two different points of view; and more especially when, as in this instance, the evidence of the witnesses cannot be invalidated by the charge of any preconcerted design. These observations, indeed, had their rise as far back as the the year 1832, and have ever since been increasing in volume and importance—an importance to be weighed only by those who feel the responsibility of life and death.

1. Eruptive diseases are matters of but little moment, so long as the eruption continues fully out upon the skin. The crimson dappled hue of measles, and the bright velvety red of scarlet fever, are beautiful to the eye of a medical man, who apprehends no danger while the skin is thus active. To these eruptive diseases there is allotted a certain definite course through which they must run, progressively stage by stage, before they terminate in a reduction to health. Nature herself, in general, manages these cases well, and, if not interfered with, brings about a recovery both spontaneous and sure: but when her work is roughly handled, and her appointed times of accession and advance, of station and change, interrupted and marred, then she seems to abandon her charge, and leave the case entirely to the superintendence of the officious physician. And thus it is that medicine is sometimes scandalized, by its existence as a science being confounded with the accidental or malicious abuse of its virtues. The physician cures not an eruptive disease by checking its course; he checks only the inflammation which grafts itself upon the original disease.

2. Of scarlet fever, the treatment, in its mild form, is simple enough. On its first appearance, it is proper to act upon the bowels by a single dose of calomel and rhubarb, but then to pause, and not to purge again till the fever has passed by. As the fever develops itself, the acetate of ammonia, with camphor mixture, may be administered; the bowels regulated by rhubarb in the confection of orange-peel; and the stomach supplied with light food, such as milk and water, bread, and tea. If there is much thirst, it may be quenched with acidulated drinks; if the throat is very sore, it may be gargled with diluted vinegar. Under any circumstances, it is right to keep the patient under the bed-clothes, and not to be too busy with medicine till the eruption begins to fade; but as soon as the skin is efflorescent, purgatives must be resumed; and, according to circumstances, the purging should be repeated at intervals throughout the ensuing fortnight. It is at this period of the fever that the tongue becomes foul, the mouth clammy, the stomach sick, and the ankles swollen: it is now that the internal organs suffer. The throat may ulcerate, and a long train of ugly symptoms baffle every effort of skill and medical prudence. In general, however, all these symptoms may be prevented by mercurial and saline purgings towards the termination of the fever.

3. At the time when scarlet fever has attained to its climax, inflammatory symptoms may develop themselves. The child is red from head to foot, but the redness is dusky, especially on the face; the eye is dull, the lips chapped, the teeth dry, and the skin burning; the tongue is red and coarse, like an ox's tongue when newly boiled for table; the pulse is hurried and small, the urine scanty, and the bowels suddenly inactive, or draining away scanty dark stools: the child lies tossing on its back, whimpering, and crying for drink; and *now* is the time to bleed. The loss of blood from the arm, mercurial purges, saline medicine, and no food, constitute the right treatment; which, if it be adopted with decision and collectively, alters the phase of the malady in twenty-four hours: the skin relaxes, the kidneys excrete, and the bowels act better. Omit this treatment, and dropsy and death is the consequence.

I could relate cases in detail; which, however, few would read, or, if they read them, would soon forget. My object is to enunciate a truth, drawn from a microscopic inspection of cases in the mass.

4. There is a form of scarlet fever not generally dwelt upon with sufficient earnestness, since it is not the less dangerous because its eruption is the less visible. Among several children of a family attacked, one child shall exhibit nothing except a sore-throat fever for a day or two, without eruption, and a white tongue, dotted with red erect

papillæ. This indisposition quickly passes away; the child may have drooped for twenty-four hours, then as quickly recover, and the mother and nurse shortly disregard it. Some physicians have said (I quote from memory, without recollecting to whose works I should refer) that its doubtful character may be determined by the anserine, though colourless, state of the skin: yet, according to my experience, this test is not to be relied upon, and the indisposition remains suspicious only on account of its origin and connexions. But the notable point about this form of scarlet fever is, that the recovery is languid for the space of a week, the eye heavy, and the tongue white; and that there is no cutaneous efflorescence. In one case which happened within my knowledge, and passed away thus seemingly propitious, meningitis started up at the end of a fortnight, and required, for its subduction, prompt and extensive depletion. These cases, insulated, no one could pronounce to be scarlet fever; and yet they might doubtless become centres of infection, and do require especial surveillance in the progress of their recovery.

And so much for mild scarlet fever, when viewed under the aspect of an inflammatory type. There are symptoms during life, more than equivalent to the demonstrable changes of structure to be shewn in the body after death: for when we find that specific signs in the course of a disease demand a specific treatment, and that that treatment is successful, what more could all the morbid anatomy in the world place at our disposal? I do not undervalue that branch of anatomy; but the physician is called upon to act in the chamber, when the body is *not* dead—when its disease will *not* stand still—when its structures can *not* be seen—and when internal changes are *not* to be discerned. Like a ship at sea in the night, the great deep upon which she is tossed is not to be regarded, but the bearings of her tack must be made according to the signals of the light-house on the beach.

Let no one suppose that, from what has been here stated, every case ought to be blooded. Not so; there are many cases which do well enough without any medical attention at all. So mild are some forms of scarlet fever, as to require merely a saline and a laxative, and, if the eruption halts, to be expedited by small and repeated doses of ammonia. Ammonia, indeed, is always serviceable, for it would seem to throw out the eruption, and to allay the febricular excitement. But the mildest form may become quickly inflammatory; and, after all, the kindest scarlet fever is but a shabby disease, since, on taking its farewell, it often leaves behind it something to be sorry for. The glands in the neck behind the ears enlarge, or the bronchia are irritated, or the liver is suspended in its action, as well as the kidneys, or a state of cachexy ensues, to be ameliorated only by time, attention to the bowels, and change of air. The ugliest symptom, after a fair aggression of the malady, is the œdematous puffing of the back of the hands and insteps; which would demand (such is my opinion) the instant loss of blood—more especially if the child is constantly picking at its nose or the bedding: for my conjecture, nay, my conviction, is, that this local œdema is the pathognomonic sign of inceptive inflammation, or at least of a general inflammatory diathesis; which is confirmed by the good effects that ensue from the loss of blood, as well as by the tough crassamentum of the blood itself. At all events, such a symptom would indicate an antiphlogistic treatment. Unceasing restlessness, sudden startings, an injected conjunctiva, a very red tongue, and muttering, added to the foregoing signs (all of which may supervene in the course of a day and a night), warn us not to stand idle while they disclose the last phases of this tremendous malady.

5. And now for the malignant scarlet fever, when the eruption does not appear fully on the skin. The non-appearance of the eruption happens generally to persons out of health, as to their stomach and bowels, at the time when the poison of scarlet fever falls upon them.

A child is taken suddenly ill—pale, cold, sick, sighing, with a slight purging; he looks as if he would die, being so very sunken. After a few hours the skin becomes hot, dry, rough, and colourless; the lips are parched, the cheeks patched with red, the eyes glazed, and the intellect gone. The pulse is excessively quick and undulating, and the action of the heart tumultuous; the præcordia is painful, the urine scanty, and the fæces green, inodorous, and knotty. The child either

tosses about incessantly, or else lies still and comatose; and exhibits in succession subsultus tendinum, colliquative sweating, delirium, and death. And this scene may pass away in a few short hours or days, so swift is the passage of scarlatina maligna.

However, this speedy catastrophe is not common. The more usual mode of this sudden attack is that of a partial recovery from the first depression, followed by a *short* stage of *inflammatory fever* at first, and next by a long and slow degeneration of all the powers of life.

6. The re-action puts forth an imperfect eruption, not velvety and smooth, as in the mild scarlet fever, but rough, and in patches, on the *inner* sides of the limbs, the *outer* sides of which become as harsh as if they had been chafed by the flesh-brush. A bounding pulse, and the exalted energy of the brain, predict the approaching assault of inflammatory action. The tongue, red and rough as usual, is covered with dry erect papillæ; the lungs labour, the heart throbs, and the intellect is dreamy and half-awake.

7. From this transient state of distinct excitement, the disease rapidly drops down into a form of asthenic fever. A purging comes and goes; sordes collect about the teeth; the pulse is hurried and fluctuating; and the flesh wastes. The patient lies tossing on the bed, muttering and naked. The throat, the pharynx, and the chambers of the nose, ulcerate; the cervical glands inflame and suppurate imperfectly, and the skin excoriates about the anus and testes.

8. This fever leads to dropsy. The skin peels off in flakes from the fingers and lips, and in abundant scurfy scales from the body and limbs. As soon as the transpiration of the skin is interrupted by its exfoliation, the œdema ensues; first on the insteps and backs of the hands; from thence slowly ascending up the limbs to the trunk, the face, and at last to the splanchnic cavities. All the functions of life become throughly deranged. On the skin over the spine and tuberosities of the ischia, foul ulcers open; and the like calamity happens over bones touched by the opposite joints, or worn by the unavoidable pressure of the bedding. Long before the end, every evil sign infests the body; all the concomitants of marasmus, and all the forerunners of death.

9. The *treatment* proper to prevent the asthenic fever and dropsy in this disease is *simple*, if it be adopted *early*, and with energy. *It is that of seizing the moment proper for venesection;* for in these cases there is, between the inceptive prostration of strength and the terminal dropsy, a time marked by a full pulse, a hot skin, and all the usual signs of inflammatory fever. In general, a painful point may be detected among some one of the thoracic or abdominal viscera, or the brain may be found unduly excited; and this is the time for bleeding and purging with calomel and neutral salts, and putting into play all those means which are known to relax the emunctories, and quell arterial action. The blood when abstracted will be found, if not always cupped and buffed, yet so firm, that by the means of a fork it may be lifted clean out of the serum in which it floats. If this mode of treatment be adopted, the case gets well of itself; a refreshing sleep usually follows upon the loss of the blood. Even young children may be bled from the arm, a mode of procuring blood far preferable to the uncertain drainage from leech-bites; and it is but fair to record the resolution with which these little ones put out their arms to be bandaged, and suffer the vein to be opened. The propriety of blood-letting can be tested by experience alone; still I know (or fancy I know), that none of those cases in which the lancet has been used, have been followed by dropsy, but that dropsy *has* followed when bleeding *has not* been exercised.

10. There are other remedies recommended and made use of in all stages of scarlet fever, from its severest to its mildest form; and these we can shortly review. 1. Gargles are always useful, for the fauces should be cleansed from mucus, and the teeth from sordes. 2. Of blisters, much doubt must be entertained as to their efficacy: they do not relieve local pain, and often leave behind them troublesome sores, which may quickly pass into gangrenous ulceration. The only time when blisters seem to be serviceable is in removing that chronic enlargement of the cervical glands behind the ears during convalescence, or when health has been restored. 3. The use of a diffusive stimulant is certain, from the beginning to the close of the illness. In the mildest forms, it is the chief and almost the only

remedy. In the inceptive prostration of strength, when, at the first onset of the malignant type, the skin is pallid, and the brain and nervous system depressed, as if from the shock of a violent accident, ammonia is the most appropriate. At this time, ammonia in full doses, and frequent sponging of the body with warm vinegar and water, seldom fail in rousing the latent powers of life; and these stimulants may be continued till the action of the skin is fully developed. But it is at this time, when the body is struck down, pale and cold, as it were, from a blow on the head, that bleeding and mercurializing, under the false notion of congested brain or lungs, is so fatally prejudicial; for now, life, already quivering under the first shock of the animal poison, might be extinguished at once by administering any the slightest depressant. The principle of treatment to be here adopted is one of restoration: it is that of exciting the heart to activity, and of inspiriting the energy of the brain; the consequent reaction is to be combated with its usual weapons. 4. Of tonics, nothing good can be affirmed. If they are beneficial at all, it is when bleeding has been omitted, and the disease is becoming progressively asthenic, with dropsy, creeping from the insteps to the thighs, and from the wrists to the arms. The mineral acids are palatable; the quinine is nauseous; and the infusion of bark, the favourite remedy of Dr. Heberden, is harmless and inefficacious. They may indeed kindle an appetite, and give some chance to the resiliency of nature, but it is a mode of temporising, which here, as in most other matters, does but postpone and insure the final mischief*.

We may therefore conclude that much is certain in regard to treatment:—1. To sustain the action of the skin in the milder cases. 2. To purge towards the close of the malady. 3. To keep a sharp look out for inflammatory signals, and to act accordingly. 4. In the severer cases, to recover the body from its first depression by diffusive stimulants and warm ablution. 5. To meet the subsequent reaction by bleeding and mercurial purgations; which mode of treatment is precisely that which the surgeon adopts in cases of mechanical injury to the brain, when the vital powers first sink under the shock, and then react, in consequence of the injury.

11. Ammonia has by some practitioners been exhibited in *all* cases; and they have maintained that it has at *all* times been equally successful in *all* forms of scarlet fever. Men of the world make large deductions from these broad assertions; for they know that there is nothing to be relied on solely in the management of any affair: our conduct must bend to change, circumstance, and expediency, as they are met with unexpected or foreseen. Ammonia, in the hands of others, perhaps, may have been more successful than it has been in mine; inasmuch as I have found it, like other remedies, useful only at certain times, on right occasions, and during stated conditions of the brain and vascular system. And, truly, were ammonia, *per se*, a specific, then might the natural history and treatment of scarlet fever be dispatched in a couple of words. But the virtues of the carbonate of ammonia are these:—that on the principle of a diffusive tonic—not stimulant—it sustains the action of the heart and arteries, promotes the eruptive process over the skin, and, instead of heating, would seem to allay the drought of fever. At all events, a foul tongue becomes clean under its use, a sore throat diminishes, and the bowels act with kindness and moderation. Yet all of a sudden it fails; for, if inflammation shall have been set up in any of the splanchnic cavities, this salt is rejected by the stomach, the patient is teazed, the pulse accelerated, the brain alarmed, and the eruption dispersed. Now these are the cases which fling us back upon ourselves, and compel us to act upon the spot, according to our own judgments and the exigencies of the moment. The cause of the disturbance is to be sought for, detected, examined, and understood, before we can proceed rationally to our business; and we might give ammonia for ever, and yet never be nearer the mark, unless, at the same time, we undertook to think for ourselves. In practice, every-day experience casts me off farther and farther from *systematic* (shall I say *scholastic?*) works on medicine; and I go back to monographs copied from nature, and even then find myself sometimes standing alone in a wilderness of disease.

1, Finsbury-place, South,
June 13, 1834.

* I have never repented of having acted decisively; but who shall express the anguish which one endures from delay and vacillation of purpose?

CASE IN WHICH THE

BURSTING OF AN ANEURISM OF THE AORTA WAS SURVIVED FOR SOME HOURS;

With the Postmortem Appearances, and Remarks on the Cause of Death.

By J. F. Crookes, Esq.
House Surgeon of St. Bartholomew's Hospital.

James Aldis, a middle-aged man, by trade a working silversmith, was admitted into the hospital on the 13th of May last.

He stated, on admission, that his neck had suddenly begun to swell about two hours previously, without any assignable cause; and that it had increased so rapidly in size, as to render it necessary to take off his neckcloth, and undo the collar of his shirt, to relieve the sensation of suffocation which was produced by their pressure.

The neck was now evidently much enlarged from the head downwards to the clavicles and sternum; the outlines of the muscles were observed, giving a uniform rotundity and smoothness to its surface, and a sensation of elasticity was communicated to the finger on pressure. On looking into the throat, the pharynx seemed to be pushed forwards against the soft palate, and presented a puffy appearance; respiration was, however, performed with ease, and he experienced no difficulty in swallowing fluids. His countenance was thin and bloodless, and his pulse feeble. He had walked from his own residence to the hospital, and afterwards up several pairs of stairs to the ward. On questioning him upon the history of his affection, he stated that he had suffered for several weeks from slight cough and occasional pains in the chest, and that he had, at different times during the last eight days, experienced difficulty in swallowing; but did not seem to consider these symptoms as having any connexion with his present state.

He was ordered to be put to bed immediately, after which it was intended to make an examination of the chest by the stethoscope. Percussion was performed at the time, and gave a clear sound throughout both lungs. About a quarter of an hour afterwards, and before auscultation was performed, symptoms of suffocation again came on, and in so urgent a form as to leave no doubt as to the propriety of endeavouring to relieve the patient by laryngotomy, which was immediately performed. The operation was speedily completed, little blood flowing from the division of the integuments; and an opening was easily made between the thyroid and cricoid cartilages, of sufficient extent to permit the introduction of a large-sized tube; it was, however, ineffectual in restoring animation. Artificial respiration was kept up, until hopes could no longer be entertained of its being attended by success.

Examination of the body, twenty hours after death.—On reflecting the skin of the neck, the cause of the swelling became apparent immediately; the cellular tissue of the different intermuscular spaces being loaded with dark-coloured blood, principally in a firmly-coagulated state. The large vessels were completely surrounded by the effusion, which had also extended behind the œsophagus, from the base of the neck to the skull, so as to separate it to a considerable extent from the spine. On removing the sternum and cartilages of the ribs, a large quantity of bloody serum escaped from the cavity of the right pleura, which, together with that still remaining in the chest on the same side, must have amounted to at least two quarts. The anterior mediastinum contained a large mass of coagulum, which seemed to extend around the trachea and large vessels to the spine, and was thence continued onwards, so as to separate the pleura of the right side, at its upper and back part, from the walls of the chest. This extensive effusion, and the firmness of the coagulation of the blood, having much obscured the different parts of the neck and chest, they were removed from the skull downwards to the diaphragm, and afterwards carefully examined, when an aneurism of the aorta, of the size of a small hen's egg, was discovered. It had commenced in the artery, at its inner and front aspect, rather below the point at which the arch terminates in the descending aorta, and, taking a direction medially, appeared to have passed behind the bronchus without interfering with it, and apparently to have become connected with the œsophagus; some of the fibres of its muscular coat appearing to be continued on to the sac.

A clean dissection of these parts was then made, and the point at which the

aneurism had burst was found to be at its upper extremity, immediately behind the left border of the œsophagus; the situation of the rupture being indicated by a mass of ragged cellular tissue, intermixed with coagulated blood. Upon opening the œsophagus, the sac of the aneurism was seen bulging into it, so as nearly to obliterate the passage by pressing the sides together; and a small spot on its surface, evidently in a sloughing state, shewed that the tumor would soon have given way in this situation. The aorta having been next slit up, the aperture was seen by which that vessel communicated with the sac—larger in size than the diameter of an ordinary quill, of an irregular form, and surrounded by large folds of the lining membrane of the artery. Small pieces of fibrine were readily drawn through the opening, and a blow-pipe passed easily from the vessel into the sac, and through the orifice by which the blood had been effused into the neck. There was no apparent disease of the heart, and the lungs and other viscera, as well as the remaining portion of the aorta, seemed healthy.

The above case appears to me interesting in several respects; but especially from the satisfactory elucidation it affords of the indefinite symptoms which presented themselves on the patient's admission. Had he been seen during the progress of the disease, I have but little doubt but that there would have been sufficient evidence of its nature; but presenting himself under the circumstances above mentioned, the case seemed very obscure. A person living for such a length of time after the bursting of an aneurism of the aorta, the quantity of blood effused, and the patient being able to walk a considerable distance, and even up stairs, during its effusion, are also points of interest. The quantity of bloody serum found in the pleura of one side, with the total absence of fluid on the other, is singular, and does not seem to be satisfactorily accounted for. It is fair to infer, I think, that it had taken place subsequently to his admission, from the clearness of the sound produced by percussion of the chest soon afterwards; and supposing such to have been the case, how can we account for its quantity, and the rapidity with which it must have taken place? The ordinary mode of serous exhalation can scarcely be looked to for a satisfactory elucidation of it; moreover, regarding it as an attempt on the part of the lungs to relieve themselves of their state of congestion, we should expect to have found an equal deposit in both sides of the chest. I am rather inclined to consider it as the result of a transudation of serum from the coagulated blood which was discovered between the pleura and the wall of the chest of the same side.

The surface of the body appeared to me to become cold more speedily in this case than under ordinary circumstances; and I should rather consider death to have been produced from the deposition of blood into the anterior mediastinum, impeding the action of the heart, than from any mechanical obstruction to the passage of air into the lungs. This view appears to be further supported by the coagulated state of the blood after death, which is opposed to the state in which it is generally found after suffocation.

June 12, 1834.

OBSERVATIONS UPON ARSENIC,

And the readiest means of detecting it as a Poison.

By Alex. Murray, M.D. of Aberdeen.

The best authorities direct arsenic to be distinguished by the following circumstances—the alliaceous odour of its fumes, (which, however, in smell, appear to me to resemble phosphorus more than garlic); the silvery stain produced upon copper, when heated in contact with arsenic; the effect of certain tests upon the solution of this substance; the reduction of the powder to the state of a metal, and the conversion again of this into the oxide of arsenic, by means of heat. It may be added, that Dr. Christison, our most distinguished authority in matters of the present kind, prefers the reduction, and subsequent oxidation, of the metal. The experiments which point out a far easier, and, I am inclined to believe, a more certain and delicate method, are as follow.

1. A small quantity of common arsenic was pressed into writing-paper by friction with a glass rod, and upon the

stain thus produced, a single drop of a solution of common nitrate of silver was rubbed by means of the rod. A phial containing common hartshorn was then immersed in hot water, and over its open mouth the paper was held (the arsenic being on the upper surface, for the sake of better observing the appearances) when the whitish spot became in a few seconds lemon-yellow. The arsenical spot being kept over the hartshorn for about half a minute more, the yellow colour entirely disappeared, but it returned when the paper was removed from the phial and held close to the water.

2. Upon an arsenical stain of the kind described, was rubbed a drop of solution of sulphate of copper, and the paper was then held over hartshorn placed as before. Almost immediately a mixture of blue and green streaks was perceived, and in a few seconds more, the spot became of a beautiful blue colour. The paper was then removed from the hartshorn, and allowed to float upon the hot water, when the blue in a few seconds was converted into a decided green.

I am disposed to believe that either of these easy experiments is quite conclusive. Jointly, they form a proof which appears liable to no possible objection. They may be varied in many different ways, as the following statement shows.

3. A spot was prepared with arsenic and sulphate of copper, (as in the last experiment), upon which a single drop of hartshorn was placed, when the whole spot after a short interval became of a green hue.

4. A spot was prepared with arsenic and nitrate of silver, (as in experiment 1.) A drop of common hartshorn being added to it, caused a fine lemon-yellow colour.

5. Arsenic was put on paper, as in the former experiments, and a minute quantity of liquid ammoniacal sulphate of copper was smartly rubbed upon the spot, when it soon became of a green colour.

6. A very small quantity of the ammoniacal nitrate of silver was rubbed upon an arsenical spot, and an immediate yellowness was the consequence.

In the first two experiments the strength of the ammonia materially influences the rapidity of its effects; and the same circumstance, in experiments 3 and 4, regulates the exact nature of the changes, which accordingly are of several different and curious kinds. It is unnecessary to detail these various appearances; but one of the most singular may be mentioned, which sometimes is observed when a drop of hartshorn is added to the mixture of nitrate of silver and arsenic, namely, a lemon-coloured ring surrounding a colourless centre. The whole spot, however, after a little time, assumes the yellow appearance. The appearances produced by putting a drop of hartshorn upon paper on which arsenic and solution of sulphate of copper have been mixed, as in experiment 3, deserve to be slightly alluded to: the central portion loses its blueness by exposure, and, in proportion as it does so, the greenish margin gets more and more of the bluish hue; and, indeed, every variety of green may be seen in this experiment. Ultimately we have in general a verdigris ring surrounding a spot whose colour is a green not very distant from yellow. Weak hartshorn seems fittest for 3 and 4, and the stronger kind for 1 and 2. When the ammoniacal sulphate of copper is used, the colour varies with the strength of the test, from a green, which may be called passing into blue, to the same colour about to pass into yellow; the former modification being produced when the preparation of copper is strong, and the latter when it is much diluted with water. When the test is of a medium strength, the colour produced with arsenic is of an intermediate kind, being grass-green, or emerald-green.

The preceding method has various advantages over the application of tests to arsenic, after it has been brought to a state of solution; and still more is it preferable to trusting to the odour of the fumes, or the alloy formed with copper. It is enough, however, to compare it with the mode of proceeding at present the most esteemed—viz. the reduction and subsequent conversion of the metal into the oxide of arsenic.

In the *first* place it may be mentioned, that, by any of the methods suggested by this paper, it is easy to detect a quantity of arsenic much less than the most skilful chemist can reduce to the metallic state. Few persons can depend upon *reducing* 1-20th, or even 1-10th, of a grain of arsenic; but I feel convinced that it is *easy*, by the methods here suggested, to detect 1-500th part, and *possible* to detect 1-1000th part of a grain, and perhaps a smaller quantity of arsenic in the form of powder. As a

proof of this, I have just now taken one carefully-weighed grain of white arsenic, and spread it out as much as possible upon writing-paper, and, by means of the ammoniacal nitrate of silver, I have, with great ease, and even in a hasty manner, made upwards of 400 separate and distinct yellow spots. Many of these are double the size of others, and more than four times the size of what might be very easily seen; and a considerable portion of the arsenical stain is unoccupied, for the sake of making the spots clearly separate. Further, having reason to think that most of the other modes adopted in the experiments are equally delicate as the ammoniuret of silver, I feel, for these reasons, fully at liberty to make the above assertion. It is at least certain, that, following one or other of the methods suggested in this paper, we may distinguish the smallest portion of arsenic that has any chance of being submitted to examination—indeed, almost as small a quantity as can be clearly seen by the sharpest eye.

A method calculated for extremely minute quantities of arsenic is not a circumstance of slender importance, since it has not very seldom happened in medico-legal investigations that it has been necessary to examine a very minute quantity of powder suspected to be this poison, derived from articles of food with which it had been mixed; or procured from the stomach, either adhering to the surface of that organ or mixed with its contents. Farther, the methods now suggested are not only less troublesome and more delicate than the test by reduction, but they are also free from the chance of failure, to which it is in some degree liable. I am aware that the reduction-test will in general succeed, and that it may even happen to do so completely, in the hands of one who has never before tried or witnessed the experiment; but it is, on the other hand, certain, that he who has repeatedly produced the metallic crust will occasionally fail in his object, owing, perhaps, to accidental peculiarities in the flux made use of. As to Dr. Turner's ingenious process for converting the metallic crust into common arsenic, I shall not discuss its merits, as the preceding argumeuts are valid, even considering it possible to distinguish metallic arsenic with unerring certainty. It is enough to say, that Dr. Turner's process ultimately depends upon distinguishing the powder of arsenic by the eye, and it may be plausibly argued, that, having reduced white arsenic to the metallic form, we gain, at least in this case, but little, by converting it again into powder, which is to be judged of by the eye; since, if ocular inspection had sufficed, the whole steps were unnecessary.

The manner of applying ammonia, adopted in experiments 1 and 2, is new; and not only forms a rather elegant chemical experiment, but is farther a far better way, as I think, of introducing an alkali, than any method which has been previously in use. The effects of the ammonia may be thus explained in a general way. When a certain quantity of this vapour is added to a combination of sulphate of copper and arsenic, a double attraction is brought into play, which causes the formation of the green arsenite of copper. Continuing to apply the fumes, we produce the blue colour usually struck between copper and ammonia; and, indeed, it is not always easy to prevent this occurrence even from the first; for the blue is not preceded by green, unless the ammonia is cautiously applied. The blue ammoniuret of copper prevents or obscures the formation of the arsenite, which, however, becomes apparent when the blueness is removed upon dissipating the ammonia by means of heat, or by exposure to the atmosphere for some time. Again, when a combination of nitrate of silver with arsenic is exposed to hartshorn, the yellow arsenite of silver is the consequence, while a greater quantity of vapour removes the colour by dissolving the salt. Afterwards, heat restores the yellowness, obviously by dissipating the excess of ammonia.

At one time the fixed alkalies were added to the arsenical solution, after which the nitrate of silver was dropped in. In this method, however, when a phosphate exists in the liquid, a yellow precipitate takes place, similar in appearance to the arsenite of silver; and, besides, the alkali and the nitrate act upon one another, and thus create a confusion in some cases. As an improvement, ammonia was used instead of the fixed alkalies, and was added to the arsenical solutions either before or after the nitrate of silver; but this was found an uncertain plan, as the success depends upon a definite quantity of ammonia being used. To obviate this difficulty, the ammonia was added to the

test; and it was believed that the ammoniacal nitrate of silver possessed the double advantage of not acting upon phosphates, while it wanted the difficulty arising from any error in the quantity of ammonia. It does not appear to me that this test possesses fully either of the advantages ascribed to it. There is no very decided rule for the formation of the ammoniacal nitrate, and if we add too little ammonia, it does not act upon arsenic; whereas, if too much is added, the yellow arsenite is never seen. The assertion, that the ammoniacal nitrate of silver does not disturb the phosphates, is also incorrect; for it acts upon these salts and their solutions, or at least upon some of them, in the most evident and ready manner.

By applying ammonia in the gaseous form, every one of the above sources of error and difficulty is avoided. In the case of solid arsenic, this method is without a single objection, so far as is known to me; and in solutions, it is free from all the fallacies attached to the fixed alkalies, and to liquid ammonia, while there is no objection peculiar to itself. As a short rule, it may be stated, that if the addition of nitrate of silver to a suspected powder or liquid produces no change, while yellowness is the consequence of afterwards applying the fumes of hartshorn, this colour cannot depend upon the presence of a phosphate, or of any known substance, except arsenic. As to the mode suggested by experiment 2, of applying ammonia, along with sulphate of copper, to detect arsenic, whether solid or dissolved in water, it is enough to say that it seems altogether unobjectionable. It is free from the errors which may interfere when the liquid ammoniacal sulphate of copper is used, while it appears to me more delicate, and has no source of fallacy of its own.

It is not to the chemist and medical jurist alone that observations on the powder of arsenic are interesting. They are in some cases no less so to the medical practitioner, strictly so called, who is occasionally called upon to prescribe for symptoms supposed to arise from arsenic; in which case it would be, to say the least, very satisfactory to determine their cause by a process occasioning neither trouble nor delay, and by means of materials which are almost always within reach. In such a case, if any whitish powder, however little can be procured, whether adhering to wrapping-paper or from any other source, I would advise that it be rubbed upon paper by any common means—say, with a pen-knife. Then let a stick of common lunar caustic, moistened in water, be drawn two or three times across the spot; and if now no change of colour occurs, but if, upon the subsequent addition of ammonia, whether gaseous or liquid, a bright yellowness is produced, the suspected powder is arsenic. If the materials are sufficient, another trial can easily be made by substituting blue vitriol (which, to save trouble, may be used in the solid state), for the caustic; when, supposing arsenic present, blue, or a greenish blue, will be caused by the hartshorn, which colour will in no long time turn into grass-green upon exposure to air, particularly along with heat.

Those who have a recollection of Dr. Paris's work on Medical Jurisprudence, may suppose that the experiment, wherein a stick of caustic is drawn across an arsenical spot made upon paper, is derived from that source; and I do not mention the matter for the sake of contradicting this, so much as for a more important object. Paris says (vol. ii. p. 241), "drop the suspected fluid on a piece of white paper, making with it a broad line; along this line a stick of lunar caustic is to be slowly drawn several times successively, when a streak is produced of a colour resembling that known by the name of Indian yellow, and this is equally produced by the presence of arsenic, and that of an alkaline phosphate; but the one from the former is rough, curdy, and flocculent, as if effected by a crayon; that from the latter is homogeneous and uniform, resembling a water-colour laid smoothly on with a brush. But a more important and distinctive peculiarity soon succeeds, for in less than two minutes the phosphoric yellow fades into a sad green, and becomes gradually darker, and ultimately quite black; while, on the other hand, the arsenical yellow remains permanent, or nearly so, for some time, when it becomes brown."

I am very far from being anxious to discover errors in the work of Dr. Paris; but I must observe, that the above passage is by no means quite correct; and farther, that, though his experiment is

admirably calculated for the end in view, namely, for distinguishing arsenic from phosphates, it answers the purpose in a very different way from that set forth in the quotation. In fact, the value of the experiment does not consist in a comparison of the yellows (for there can be no such comparison), but shortly in this: nitrate of silver produces no change of colour with arsenic; but with a phosphate it readily occasions a beautiful yellow. Notwithstanding Paris's specific experiment, and also the general assertion of Orfila, Gordon Smith, and other chemists, both at home and abroad, that nitrate of silver causes a yellow precipitate with solutions of arsenic, I venture to say that the opinion has no kind of foundation. Nitrate of silver, whether solid or in solution, has no perceptible effect upon arsenic, either in powder, or dissolved in water; but it acts readily upon phosphates and their solutions, producing the yellow phosphate of silver. I find, farther, that a combination of ammonia and nitrate of silver, in a certain proportion, acts upon phosphate of soda, but not upon arsenic; and that there is another combination of the same ingredients, wherein the ammonia is rather more abundant than before, which produces a yellowness both with arsenic and the salt mentioned; another combination, containing a still greater proportion of ammonia, which produces a yellowness with arsenic, but not with the phosphate; while, lastly, there is a fourth combination, in which the ammonia is still farther increased, that has no perceptible action, either upon arsenic or phosphate of soda*.

* Slightly abridged from the Edinburgh Medical Journal for April.

ON PREPARATIONS OF THE ABSORBENT SYSTEM.

To the Editor of the Medical Gazette.

Sir,

One of the most difficult parts of anatomy is that of the absorbent system. The interesting nature of the research, respecting parts of so much importance in the animal economy, compensates, in some degree, for the time spent in injecting the glands and vessels, and then displaying them by dissection. After so much labour, it is, however, desirable to make the parts into preparations for reference; but the uncertainty of preserving these from decay, has deterred many from attempting an operation that has so generally produced the most vexatious disappointment.

By way of experiment, an extremity with large muscles was used; but a thinner one would have been preferred. The vessels were filled with quicksilver, after the ordinary method. When the dissection was finished, the flesh had become putrid, and the limb was immersed in a solution of oxymuriate of mercury for a month, and then placed in cold water for twenty-four hours. When it was put on a board, for the purpose of being dried, the vessels were found to have become thickened and opaque, and did not allow the quicksilver to be well seen through them. It became necessary, therefore, that the vessels should be painted, and a sky-blue colour was used for them, whilst the fleshy portions of the glands were tinged with lake. The vessels are now strong cords, and, although in a great measure emptied of quicksilver, have not shrunk near so much as might have been expected. The flesh, changed by oxymuriate of mercury, will form a firm and lasting support for them, and remain incapable of being undermined by insects; it may therefore be presumed that such a preparation, kept from the dust, will continue unaltered for many years. The tendons may be varnished, and the muscles coloured according to the usual* plan; and the whole varnished at last with copal varnish.

If, after the dissection, each vessel be encircled by a ligature of fine silk, at the interval of a few inches, it will to a greater certainty escape rupture, and only one of these portions can be then emptied by a wound or other accident.

It has been objected to dry preparations of the nerves, that strings might have been substituted for nerves: it is, however, hardly possible to conceive that any one who dissects for instruction would practise so gross a deception: here, however, the faithless inspector may be generally convinced by tapping

* See work on Preparations, third edition.

a vessel with the point of a lancet, and observing the issue of quicksilver.

I remain, sir,

Your obedient servant,

J. Swan.

6, Tavistock-Square,
May 2, 1834.

CONTRIVANCE FOR EXTRACTING A PLUG FROM THE NOSTRIL.

To the Editor of the Medical Gazette.

Sir,

I shall feel obliged to you to insert the following notice, as I think it may be found useful in cases similar to that which I shall relate.—I am, sir,

Your obedient servant,

M. D.*

Hull, June 9, 1834.

A short time ago I was called to a case of hæmorrhage from the nose, in which, after the usual antiphlogistic measures had been adopted, it was found necessary to take away the plug. Now it happened that when the plug was introduced, which was done with considerable firmness, no string had been left attached to it, by which to withdraw it. Bougies of different sizes were tried by the surgeon in attendance, but in vain. The plug was tightly fixed, and being of the shape of a wedge, whenever force was applied to the bougie, it slipped to the side of the plug, and ceased to act in the required direction. At length it was determined to pierce a metallic bougie at its extremity, and to make an opening also in the side of the bougie to meet this, about half an inch from the point. The thread from the nostril was then introduced into the opening, at the extremity of the bougie, and passed out at the other, and thus it was used not only as a director to the plug, but at the same time to keep the plug and bougie in apposition. The point of the bougie was thus effectually prevented from slipping off the plug, which, on the application of sufficient pressure, was readily removed.

Thus an accidental omission *may* lead to an improved practice, in dispensing with the withdrawing thread, which always excites nausea and irritation, and which adds to the discomforts succeeding the operation, by delaying mucus and blood about it.

* The writer has given us his name.—Ed. Gaz.

ON THE WANT OF UNIFORMITY IN THE STRENGTH OF SEVERAL SOLUTIONS, TINCTURES, &c. IN DAILY USE.

To the Editor of the Medical Gazette.

Sir,

There are many preparations constantly ordered in prescriptions, for which there are no directions in the Pharmacopœia, and for which there are, therefore, no universally-adopted proportions.

Having frequently observed the inconvenience, and even risk, which this want of uniformity produces, and having heard the sentiments of medical men at different times on the subject, I shall feel obliged by your insertion of the following remarks.

Among the most important are the solutions of the salts of morphia. The variation in the strength of these solutions, as prepared by different druggists, almost entirely prevents practitioners from prescribing those medicines in that form, which is a much more accurate mode of dispensing them than weighing them in substance.

Magendie, whose formula is generally adopted, directs the solution to be made thus :—

℞ Morph. Acetat. grs. xvj.; Acid. Acetic. ℥ij.; Aq. Distil. ℥vj. Misce.

but I am aware that some druggists prepare it differently, and I have heard of cases in which patients have taken wrong doses in consequence.

It is rather singular that the sulphate of morphia, which is in daily use in Paris, is seldom, if ever, prescribed here; while the muriate, which we employ more than any other preparation, is not mentioned in the French works.

There is a syrup recommended by Magendie, and quoted in Jourdan's *Pharmacopée Universelle*, which is a very safe and convenient mode of administering the salts of morphia in minute doses.

℞ Morph. Sulphat. grs. iv.; Syrup. Simpl. lb.j. M.

Of course the acetate, muriate, and citrate, might be prepared in the same proportions.

When we consider the beneficial effects of these preparations in cases where all other remedies fail, and also the serious consequences of improper doses, it must be admitted that it is very desirable to remove every obstacle to their general use, as well as to guard against any probability of mistake in their administration.

The tinctures of quinine and iodine, and the solution and ointment of hydriodate of potash, are likewise involved in some uncertainty. It is true that forms for these preparations are to be found in different French works, but these do not always agree; and moreover it is not likely that all druggists have these works constantly at hand: those who have, may, in some cases, substitute other forms which they think better; and there is, therefore, always a chance of misunderstanding on the subject.

The tincture of quinine of Magendie, and the French Pharmacopœia, is a solution of 6 grains of sulphate of quinine to 1 ounce of alcohol*. Rennie, in his supplement to the Pharmacopœia, orders 4 grains to an ounce.

By which of these forms should the following prescription be prepared?

℞ Tinct. Quinin. ℥ij.
Capiat. Cochl. ij. min. ter die.

A better proportion would be 8 grains to the ounce, for in that case a teaspoonful would contain exactly a grain; and the addition of a few drops of acid would prevent the turbidness of the tincture, which is occasioned by the comparative insolubility of sulphate of quinine in the neutral state. A compound tincture is kept by some druggists, which is found beneficial in many cases where acids are objectionable.

℞ Quin. Sulph. gr. vjii.
Tinct. Cinnam. ℥j. Misce.

The tincture of iodine, taken from Magendie, is about 40 grains of iodine to 1 ounce of alcohol*.

I believe this to be the strength intended by medical men, but it is evident that it is not universally adhered to, by the statements I have frequently heard from them of the different effects produced by the tincture from different shops.

Magendie's liq. potas. hydriodat. is about ½ dr. to 1 oz. of distilled water†.

The ointment is one scruple to one ounce of lard. Comparing this with the mode in which it is generally prescribed, it is a very weak ointment; as we seldom see less ordered than a drachm to an ounce, and frequently two drachms, or even more.

I have observed, on mixing the ointment, that it does not so soon become discoloured, when lard is used, as it does when spermaceti ointment is the vehicle. Whether the discolouration is desirable or not, must be ascertained by experience; but I had occasion, some time ago, to speak to a physician on the subject, who thought it rendered the ointment more efficacious. It would follow, from this conclusion, that the iodine ointment is a more potent application than the above—viz.

℞ Iodin. 3ss.; Adipis, ℥iss. misce‡.

The ioduret of iron has been frequently used lately, but it is a very uncertain preparation. I have seen specimens from several chemists, but have never met with two alike. Indeed, it is a compound so liable to decomposition, that it is almost impossible to keep it uniform; and it is therefore going out of use very fast.

Most of the preparations of colchicum are made according to no settled proportions. The vinum colchici of the Pharmacopœia will not keep§; the druggist is therefore obliged either to adhere to the former form, and use wine, or to add a larger proportion of spirit. Some object to the fresh root, as being liable to vary, in strength, with the season, soil, &c. and therefore use it in the dry state, as recommended by Dr. Thomson.

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* It will be observed, that the reduction of the French to the English weights is inconvenient, and likely to give rise to errors. The above reduced to troy weight is Sulph. Quinin. 4.92 grs. to 7 dr. 52.5 grs. of Alcohol.

* Or more correctly, Alcohol 7 dr. 52.5 grs. to Iodine 39.36 grs. troy.

† Potas. Hydriod. 29 52 gr. Aq. Distil. 7 dr. 52·5 gr. troy.

‡ Iodine, gr. 29·5; Adipis, 11 dr. 48 gr. troy.

§ The vin. ferri of the last Pharmacopœia has the same defect. The best mode of preparing it is according to the old form—with sherry wine.

The vin. sem. colchici, tinct. colchici, and acet. colchici, are still more indeterminate, there being no directions for making them in the Pharmacopœia, and the forms by which they are made differing, to my knowledge, above 50 per cent. in strength. It is therefore impossible for medical men to calculate with any accuracy on the effect of these remedies, since the same prescription taken to six shops will in all probability be prepared in as many different ways.

The tincture of cubebs is occasionally ordered in prescriptions, but there is no form of it in any standard work; one of the best proportions is the following—

℞ Pip. Cubeb. ℥iv.; Sp. Vini. ten. Oj. M. &c.

The dose being a tea-spoonful two or three times a-day.

The variation which exists in the hydrocyanic acid, partly for want of a determinate process, is too notorious to require any comment.

Many other preparations might be mentioned which are subject to similar inconvenience, but the above are those which occur to me at the present moment as being the most important.

It will be admitted, that all compounds ordered in prescriptions should be prepared by a clear and decided form given by the College; it being the duty of the druggist to follow the directions of the physician, and not to consider what would be a proper dose, or whose proportions to adopt. Nor should it be expected of him to seek for this information in different foreign works, which some may neglect to do for want of opportunity, and others to save trouble.

As the next Pharmacopœia is not likely to be completed for some time, it would be an advantage for an appendix to be published, to describe new medicines, and set doubtful matters at rest.

If the perusal of these remarks should be the means of turning the attention of any one to the subject, who can suggest any more easy or effectual remedy for the deficiencies alluded to, the object which I have had in view will be attained. I am, &c. &c.

PHARMACOPOLA.

London, June 9, 1834.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Précis Elémentaire de Physiologie. Par F. MAGENDIE, Membre de l'Institut de France, &c. Quatrième Edition. Brussels, 1834.

IT will scarcely be expected of us to offer any thing like an analysis of a work which has reached its fourth edition. The success which the Physiology of M. Magendie has met with, sufficiently indicates its merit, and may be looked upon, in some sort, as a homage paid by the public to one of the ablest of the French physiologists—by some considered as the actual successor of Bichât. It is certain that the author, with a few others, has been mainly instrumental in rendering physiology in France what before his time it could scarcely be said to be—the *history*, rather than the *romance*, of life. Few have been more skilful than M. Magendie in applying physico-chemical science to physiology; and for his adroitness and great ability in pursuing the experimental method, his reputation has long been firmly established. There is an admirable passage in his preface, which shews the enlarged and truly philosophical spirit with which he has not only conducted all his researches in detail, but combined his rich materials into a splendid whole: but we must be content to refer the reader to it. In some of M. Magendie's theories, there are, it is well known, certain points which must be generally held apocryphal, and there are probably certain blemishes here and there; and the phrenologists, of course, will not easily forgive the author for the liberties he has sometimes unmercifully taken with their notions; but these we cannot, and we fancy we need not, now more fully allude to. We have been principally induced to notice the work of M. M. at present, on account of the form in which it now for the first time appears,—in a single volume, clearly and indeed beautifully printed,—issuing from the Brussels press, and at a price which puts it within the reach of every student. With such attractions we can strongly recommend it to all who are not possessed of the bulky and expensive edition of Paris.

MEDICAL GAZETTE.

Saturday, June 21, 1834.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

MEDICAL ST. SIMONIANISM.

THE HOMŒOPATHIC MISSION.

Two or three very deluded and conceited persons have been trying of late to make a little stir in London in favour of homœopathy, but they have fared like the missionaries of the St. Simonians: they were allowed to say all that they had to say—they were listened to civilly—but took nothing whatever by their motion. Time was when idle speculations like that of Hahnemann would have met with a warmer reception in this country: indeed, the practical part of it—the infinitessimal doses—might have a chance still in the hands of a clever quack; but as a theory, homœopathy, the outcast of Germany, and the spurned of every continental philosopher who bestowed any attention upon it, could not for a moment endure the ordeal of the Baconian principles on which medical science has of late years become firmly based in England. Brunonianism had its day, but is now only to be found in some of the schools of our continental brethren, to whom we have bequeathed it in return for many favours of a like kind. Animal magnetism, and tractors, also had their summer: nothing was then to be heard of but the virtues of invisible agency; mystification beyond that of Hahnemann went down without any scruple; but, alas, for the homœopaths! those glorious days are gone, never more, we trust, to return.

The truth seems to be, that medical men in this country, after having been long plagued with wandering among theories and idle schemes, have grown wiser in their generation, and look with suspicion on all the propounders of new systems. Luckily, too, the advocates of these new lights are generally stupid and ignorant people, who, being dazzled in proportion to their ignorance, only plunge into absurdities, and expose themselves at every step. They talk of "facts," and appeal to "facts," as if they knew or could comprehend what *medical facts* mean. They blazon forth the "celebrated men" who belong to their own persuasion, forgetting that the said men ought to be better known and appreciated by their professional brethren, than by them who are as unacquainted with the state of the profession as they are with the principles which they profess to support. The really celebrated men, meantime, who have rejected their revelation in the lump, are of course completely overlooked. But there is nothing in this very unusual or unnatural; it is only a repetition of what constantly occurs, on the attempted propagation of any new idol worship.

It is not a little curious, while it speaks volumes for the wretched plight of the thing itself, that the only two strenuous and energetic, however wrong-headed, advocates who have ventured before the public here, in support of Hahnemannism, have been persons not of the medical profession—one a doctor of civil law, the other a divine. With the former (Borthwick Gilchrist, LL.D., of India-House celebrity) we mean to have as little to do as possible: the filthy grossness, as well as the pure ignorance, displayed throughout the pages of his voluminous pamphlet, have protected it from exposure hitherto; richly-merited oblivion, which has since seized on it, will, we doubt not, do the rest. But Dr. B. G.'s successor is one whom we are not disinclined to think a person of better pretensions. It is rather against him, indeed, that he quits his legitimate pulpit (he being an incumbent

in the church) to mount the stage as a pickle-herring for the Hahnemannian mountebanks; but we suppose him to be in earnest, and have given him a hearing accordingly. Dr. Borthwick Gilchrist addressed the public, proclaiming homœopathy as the "gospel of medicinal redemption." But Mr. Everest*, more modestly, addresses the medical practitioners of Great Britain, and, after a little courteous palavering, calls upon them to *examine* homœopathy; to pass *no opinions* on it, but to confine themselves to *facts*.

The reverend gentleman's pamphlet is not very long, but it is sufficiently so to prove that its author is a very simple person. He begins *ab ovo*—assuming not only that nothing is known in this country of the dogma *similia similibus*, but that even the career of Hahnemann is a mystery to us; for which reason he favours us with a short sketch of the medical adventurer of Meissen, and, by way of excuse for not being more detailed, adds, that "it is possible that, in a few years' time, mankind may need no information as to that *young* man." We perfectly agree with Mr. Everest that this is quite possible.

The easy credulity of our reverend advocate is very amusing. "It is doubtless well known to you, gentlemen," he says, "that all over the continent of Europe, and in America also, homœopathy *is said* to be flourishing; *it is said* to have performed, and to be daily performing, cures which are almost miraculous; *it is said* to have surpassed polypharmacy, &c." And what *is said*, is evidently implicitly believed by Mr. Everest. He will excuse us, however, if *we* do not believe it, and if we inform him that the *on dit* of the flourishing condition of Hahnemannism, to which he attaches so much faith, is *not* so very well known among us. We have looked into the list of practitioners in Dr. Quin's Pharmacopœia, which professes to give the names of all those celebrated Hahnemannians in every part of the globe—"*qui magnam in laudem pervenerunt;*" but, conversant as we are with the names of distinguished practitioners both at home and abroad, we confess we cannot charge our memories with ever having heard before of even half a dozen out of the whole catalogue; and, indeed, if we were to judge of the degree of celebrity of the foreign disciples of Hahnemann by that of the *two* individuals set down as the *only* homœopaths in London, we should think that Dr. Quin would have acted more wisely had he committed his list to the fire, and contented himself with a good round general assertion of their vast numbers and respectability in all quarters. Let the reader judge for himself. The homœopathic practitioners in London are "Belluomini," and "Dunsford, *Chirurgus*." We give them *literatim*, yet still we are sorry we must plead our ignorance of the parties. Mr. Everest, however, is apparently not so much in the dark as we are: we observe that in enumerating the "supporters" of homœopathy, he says:—"*We know* of Peschier and Dufresne, at Geneva; Trinks and Wolf, at Dresden; Des Guidi, at Lyons; Mühlenbein, at Brunswick; *Quin*, *Dunsford*, and *Belluomini*, in London; Bigel, at Warsaw; and many others." *He* may know all these "celebrated men," as he repeatedly calls them, but we are obliged to say *we* have not that honour, and we fancy many of our readers are in the same predicament.

Mr. Everest, as we have already noticed, condemns *opinions* on homœopathy, but appeals to the evidence of *facts*, and challenges us to *examine* homœopathy in Hahnemann's "own mode of arguing." We suspect that few will be found

* A Letter addressed to the Medical Practitioners of England, on the subject of Homœopathy. By the Rev. Thomas Everest, Rector of Wickwar, Gloucestershire.

so silly here as to take up the Hahnemannian gauntlet. Mr. E., however, has no right to assume that homœopathy and its principles have not been examined as thoroughly as they deserve, or as any other flimsy thing of the same sort, cast upon the waters of medical science, can merit to be noticed. Would Mr. E., meanwhile, condescend to tell us what he calls *facts?* We happen to know what have often been attempted to be pawned on the world as facts in medical controversies. Our advocate talks in this way:—"Bring him to dates and facts, it is his own mode of arguing, and he cannot complain; tell him that sulphur 30 never did cure an ulcer; that arnica montana 18 did not abate fever, and keep down swelling in the case of a fractured thigh; that you tried them often, and they failed invariably." Now, let us take this *fact* of arnica. How do we know that it abates fever, and keeps down swelling, in the case of fractured thigh? Hear a case of Dr. Dufresne's, quoted with great satisfaction by Mr. Everest to establish his fact.

"Un homme âgé se cassa une cuisse; j'en fis la reduction et appliquai un appareil; puis je lui fis prendre arnica 6 et huit jours après encore arnica 6. Il n'y eut ni fièvre, ni douleur, ni symptôme anormal."

That is to say, a thigh is broken; it is set and duly bandaged; and it gets well without any accident. What did homœopathy perform in this? Why, homœopathy administered two *minimissimized* doses of arnica, one after the surgical treatment, the other in the course of eight days; and lo! homœopathy prevailed. The arnica did not indeed set the thigh, or produce union of the divided bone; but it *kept down fever*, and *the parts did not swell.* What a wonderful and indubitable *fact!*

But we will not waste our time on such trifling. Let the homœopathists pursue their harmless foolery. If they confine themselves to such treatment as this, nobody will quarrel with them: but they must not obtrude their silliness upon other people, with a view to medical proselyting, or the creating a sensation. Above all, the public must not be dunned by such people as the Gilchrists and the Everests, with their effusions of mingled weakness and presumption. We are sorry to take our leave harshly of the latter gentleman; but we really think he might find more useful and fitting employment than that of endeavouring to preach up principles which he fancies he is able to appreciate, and affecting to undervalue doctrines of the very elements of which he shews himself profoundly ignorant.

Mr. Everest is evidently a novice in the art of advocating quackery, or he would have sense enough to avoid the trite, though still disgusting, practice of employing texts of Scripture for the purpose. When the late Dr. Solomon used to advertise his "Cordial Balm of Gilead," he always descanted on its virtues by beginning with the text—"Is there no balm in Gilead? Is there no physician there? Why, then, is not the health of the daughter of my people recovered?" Mainauduc propounded his quackery in terms highly evangelical, or rather, we might say, truly blasphemous. Even B. Gilchrist deals in scriptural lore; but we are amazed at finding in Mr. Everest's pamphlet applications of passages of holy writ to the homœopathic figments. Some of those which attracted our attention, verge not a little, we must say, on the borders of profaneness. In the very text assumed for his motto, the disciples of Hahnemann are impliedly compared with the apostles of Christianity; and in a subsequent page the reverend gentleman wrests a text from the Gospel of St. John, so as to apply to Hahne-

mann the words that were uttered of the Son of God!

Mr. E. may be very familiar with scriptural phraseology, and the language of the Bible may flow to his pen in rich profusion; but he ought to be on his guard, and reserve it rather for his sermons, than for letters concerning a vain philosophy. The following passage from an able writer supplies a hint which we seriously recommend, in parting, to Mr. Everest's notice, as well as to amateur quack-advocates in general. "The operations of sickness and of healing," says Southey, "are alike mysterious; whence arises the predilection of many enthusiasts for quackery, and the ostentation which all quacks make of religion, or of some extraordinary power in themselves."

PROVINCIAL EDUCATION.

COUNTY HOSPITAL, WINCHESTER.

THE following document has been forwarded to us, in the shape of a circular. We think it very creditable to the parties from whom it emanates; and the suggestion touching the propriety of preliminary examination in town, deserves particular attention from the authorities who have such an arrangement in their power.

We have received a copy of the proposed memorial to which the surgeons of the Bristol Infirmary invite our assent, and also the strictures of the Birmingham Hospital surgeons thereupon; and we beg to state, that while we agree that there is much justice in many of the objections of the former, as to the little credit given to the provincial hospitals as schools of medicine and surgery, we must still allow that there is great truth in the remarks of the latter, especially in those which are opposed to the idea that a residence in London might be altogether dispensed with.

As a medium between these conflicting opinions, we beg very respectfully to hazard a proposition for the consideration of both the above parties, as well as the profession at large, and also the Parliamentary Committee now sitting, viz.:—

That the interests both of schools and pupils, whether provincial or metropolitan, would be greatly promoted by the classification of the said pupils; for which purpose it would be desirable that a board should be established, at which each pupil, on his arrival in town, or on his first entering any of the larger recognized schools, might offer himself for examination in materia medica, pharmacy, botany, chemistry, anatomy, physiology, medicine, and surgery; and in proportion to his proficiency, may be placed in a 1st, 2nd, or 3rd class; by which plan the number of lectures and the length of his future hospital attendance in London might be graduated, previous to his presenting himself for final examination at the Hall or College.

By such means we are of opinion that the meritorious and industrious apprentice would be encouraged to obviate the third objection of the Bristol memorialists, and would have an opportunity of distinguishing himself in the commencement of his career, and thus due credit would be reflected on the master, or the elementary school from whence he came. From this source, also, we should consider the certificate of good moral conduct to be still an essential preliminary to examination.

CHARLES MAYO,
HENRY G. LYFORD,
W. J. WICKHAM, } *Surgeons.*

Winchester, May 31, 1834.

ROYAL INSTITUTION.

Friday, June 6, 1834.

Development of the Vertebral Column.

DR. GRANT gave a lecture on this subject, chiefly in explanation of M. Geoffroy de St. Hilaire's method of contemplating the bony structure of animals. A single vertebra was taken, and its various relations considered: it was shown to consist fundamentally of seven parts; the body, two perivertebræ, a superior spinous process, two paravertebræ, and an inferior spinous process. Diagrams were employed, and numerous drawings referred to, for the elucidation of the curious analogies pointed out. The perivertebræ were described as forming a ring for the protection of the nervous system, the paravertebræ for the sanguineous, and the spinous processes as affording attachments to the muscular apparatus. The modifications which these several parts assume in the different classes of animals, particularly in fishes, are of a very striking nature. The structure of the wolf-fish was especially instanced,

as illustrative of the further appendage of interspinous processes, resulting, however, clearly from the original formation. In the lower classes of animals, the rings which defend the nervous and sanguineous systems are placed in a well-defined order. In insects, for example, the nervous apparatus is placed inferiorly, the sanguineous superiorly; and the same arrangement prevails in the crustacea; but when we ascend to the vertebrated animals, the situation of the respective rings is found to be changed: that surrounding the nervous cord is uppermost, and the sanguineous apparatus is below. In short, according to these curious views of M. St. Hilaire, the *skull* itself is but a development of the vertebræ; and the *ribs* result from an arrangement of them, which is peculiar to the terrestrial mammalia: the paravertebræ in the first place become transverse processes; and the inferior spinous processes, no longer remaining compound, but reduced, as it were, to their constituents, are extended out into the bony protective apparatus of the chest. In fine, the whole skeleton, as it exists in its full perfection in man, would seem, according to this curious, but, we think, rather fanciful theory, to be the result of the development merely of certain primary bones of the vertebral column.

CLINICAL LECTURE

ON

SOME CASES OF DISEASE OF THE KIDNEY,

Given at St. George's Hospital,

BY MR. CÆSAR HAWKINS.

1. *Hysterical Suppression of Urine, with Paralysis of the Bladder.*
2. *Suppression of Urine, with Inflammation of the Bladder.*
3. *Abscess of the Kidney from Stricture* (?)
4. *Calculi in the Kidney from Stricture* (?)

I PROPOSE to bring before your notice, in to-day's lecture, some circumstances connected with a very important class of cases, of which I happen to have several under my care at present,—cases which are important not only on account of their severity and danger, but also on account of the insidious manner in which they frequently arise, and the obscurity which often prevails over the first part, at least, of their course,—I mean disorders of the kidney. You have many opportunities of observing the gradual and concealed dangers of these cases in lithotomy, and other diseases of the urinary organs, in which it so often happens that an organic disease of the kidney, unsuspected, on account of the patient's greater suffering in the bladder, is all at once aggravated, and becomes rapidly fatal, just when the operation, or the treatment of the stricture, has relieved the patient from his former tortures, and given him hopes of speedy recovery. The observation of one such case is sufficient to show you the risk of a hasty prognosis in any long-continued disease of any one of the urinary organs.

CASE I.—The first case I will comment upon is one which I take to be merely a functional disorder of the kidney. This is the history in my book.

The patient, Sarah Grover, æt. 20, was admitted into the hospital, April 15th, having been ill three years. A cessation of the menses was the first symptom she observed, which continued for seven months; after this the discharge returned, and became regular as to time. At first the quantity was small, and it gradually increased to its natural quantity,—then it became more, and is now twice as much as it should be, and continues for five or six days, during which time she has considerable pain across the back and abdomen, and in the thighs, and she feels very weak and low; there is also a little leucorrhœa.

I pause for a moment to make you observe this derangement of the uterine functions, as it affords a key to the rest of her complaints, which are *hysterical*, as they are termed; and her constitution shows them to be of what I called, in the last lecture, the low form of hysteria,—her whole frame weak and trembling,—her mind desponding and dull, and she is incapable of being roused to any mental or bodily exertion,—her face pallid,—her pulse regular and weak,—her eyelids and ankles inclined to swell, and to become œdematous. To add to her misfortunes, she has a hernia, and suffers from hæmorrhoids.

Then as to the urinary disorders.—At the time the menstruation returned, she was attacked with *retention* of urine, which has continued ever since; so that she has, for about two years and a half, been unable to pass a drop of water but by the assistance of the catheter, which has been passed once or twice every day since that time.

Now this hysterical paralysis of the bladder is a very common symptom, and is often accompanied with paralysis of the lower limbs, so that the patient has complete paraplegia. It is generally wrong

practice to have recourse to the catheter without the most urgent neeessity; and having now been done so long, years may elapse before she ean leave it off again, the bladder having totally lost the habit of exertion. It is like the hysterical loss of voice, or of the power of swallowing: there is a want of volition; and if the patient's mind is altered, or she is urged by hunger or pain, the partial paralysis will generally be removed. Since she has been in the hospital, the water has been sometimes allowed to accumulate for twenty-four, and once for forty-eight hours; and when the bladder is thus full, she has been placed in the shower-bath, in order to induce muscular contraction, but hitherto without avail.

Next, as to her renal complaint. The *quantity* of water has for a long time been extremely small, except when she has taken diuretics, of which, by her account, she has made frequent use; but even then does not exceed ℥viij. in the twenty-four hours; and often, when the catheter has been passed, the bladder does not contain any at all. On her admission the quantity was not above 3 or 4 oz., and it has varied from this to 8 oz. since she has been in. The *quality* of the urine is not much altered, for it is generally rather high coloured, and slightly scalds her as it is drawn off, and there is a little pinkish sediment occasionally; that is to say, the quantity of saline matter secreted is nearly natural, though with very little aqueous fluid; and therefore she does not suffer as from suppression of urine of a different kind, in which probably an accumulation of urea in the blood produces the dangerous and frequently fatal symptoms sometimes observed in such cases.

I believe, then, that this state of the kidney is *functional* only: there is no albumen, no mucus, nor other sediment, to make it probable that there is any organic change. This affection is called, by Dr. Prout, spasmodic: I hardly understand the term, however, as applied to the secreting texture of the kidney, and would rather call it hysterical or nervous; *i. e.* I believe it to arise from deficient nervous energy, of which the whole body partakes, but which is especially felt by the kidney; it is the same kind of suppression, in fact, that may be produced in a greater degree by cutting the nerves of the kidney; and you may observe precisely the same effects upon the secretion of the liver, both in experiment and disease.

With this view of the case, you will observe that my object has been to strengthen her system as much as possible, and encourage her to exert herself. She has taken the steel mixture three times a day, with aloetic pills at night, and a shower-bath every day, or every other day.

She has continued this plan for above a month, with the exception of a few days, when she had a feverish attack, with erythema of the face; but it seems to have produced no very material difference. At first she seemed stronger and better, and the quantity of water increased from 3 or 4 to 8 oz., but it several times diminished again. The shower-bath always seems to do her good for a time. A few days ago, besides pain in one side or other of the chest, she had so much in the lower part and side of the abdomen, that I ordered a blister to the loins, which did her some good. I suppose, however, the steel must now be changed for some other plan, perhaps for some form of terebinthenate medicine, perhaps for cantharides internally, not merely with a view to the suppression of urine, but also to the condition of the bladder and uterus. A short time since, for instance, a young woman was under my care, with hysterical incontinence of urine instead of retention, which was completely relieved by the Chios turpentine. I ought to observe to you, that a more usual effect of hysteria upon the kidney, is a great increase of the quantity of water, and a dilution of the salts, instead of a diminution in the quantity, and a concentration of saline matter, such as this girl labours under.

[Since these remarks were made, Mr. Hawkins has ordered twenty-five drops of tr. lyttæ, in infus. calumbæ, three times daily.]

Case II.—Our next case is also one of primary disease of the kidney, but with this difference, that the disorder of the kidney has actually produced disease of the bladder, instead of both being merely coexistent. This is the case of Ann Basset, æt. 57, admitted April 30th. It seems that she has been ill six months, during which time she has suffered the most acute pain in the bladder, so that she is almost incessantly straining, both day and night; the only relief she obtains being from sitting constantly over the steam of hot water. She is a good deal emaciated, and has lived a hard life.

Now this woman came to me two months ago, complaining of the pain I have mentioned, and imagining she had stone in the bladder, which, indeed, she still fancies. I examined her, however, and found only an inflamed bladder, without stone; and the introduction of the instrument gave such acute pain just close to the neck of the bladder, that I almost thought there must be ulceration of the surface of that organ. I ascertained, too,

that there was no scirrhous disease of the uterus, which might be the cause of the inflammation, and which her age made me suspect. Since that time I did not see the woman till her admission, when it was evident that the disease was in the kidney, the following symptoms being apparent in addition to those of the bladder, which alone she then complained of.

The *quantity* of water was diminished, so that she did not make above four ounces in twenty-four hours, notwithstanding her incessant desire to empty the bladder of its supposed contents; and what little was secreted was altered in quality, being alkaline, peculiarly fœtid, and of a very remarkable *greenish tint*, with a sediment deposited from it, consisting of an impalpable powder of a *blue* colour. You observed also, that there was a great quantity of tenacious ropy mucus, which, however, is secreted, not by the kidney, but by the mucous membrane of the bladder.

There is, then, in this case, an altered state of the secretion of the kidney, possibly with some organic change; but of this we have no direct proof. You observe, however, that she has no pain referred to the kidney, but all her sufferings are experienced in the bladder; because the urine having become alkaline, is rendered irritating to the mucous surface, which secretes a great quantity of mucus to defend itself from the effects of the urine, and is constantly acting to get rid of the irritating cause. The bladder is, in fact, in a state of chronic inflammation. You will find, however, that a much less alteration of the urine than is here present, is sufficient to cause the same reference of the disease to the bladder, making it highly necessary to examine the urine in all cases of apparently diseased bladder. For instance, the urine in young children is very often a little too acid, the consequence of which is, that they are brought to you sometimes complaining of all the symptoms of stone, sometimes of paralysis of the sphincter, and incontinence of urine; and either of the cases will be cured by some alkaline and bitter medicine, which quiets the disordered secretion of the kidney, and sets the stomach right.

Besides the alkalescence of the urine, our patient shews us a very singular deposit, which I do not recollect to have seen before, and which I can scarcely explain to you. You know, however, that besides lithic or uric acid, some other acids, such as nitric, are sometimes present in the urine, the effect of which is to change the uric acid into what is called purpuric acid, which is deposited, along with salts, in the form of a pinkish sediment, from the mixture of purpurates with lithate of ammonia. In the Medico-Chirurgical Transactions you will find an account of a case in which the urine became quite black, and Drs. Marcet and Prout proposed to call the principle on which it depended melanic acid; and some other peculiar acids have been described, which seem for the most part to be modifications only of the lithic acid, by the presence of other acids with colouring matter. The greenish tint of the water, and the blue deposit, in this case, are probably owing, therefore, to some slight variation of the purpurates of ammonia, which are evidently modified as to their tints by many circumstances of admixture.

You will see at present that this patient has been very much relieved by the treatment adopted, since she makes water much less frequently, and with comparatively little pain, so that she has become capable of moving about again with facility, and sleeping comfortably. The urine is almost entirely free from mucus, and is secreted in much larger quantities, and generally reddens the litmus paper, though there is still so much ammonia with the acids, that it is soon rendered alkaline, and smells offensive from the first. The peculiar blue deposit and greenish tint continue, however, little altered; so that, although the disorder of the bladder has been relieved, the disease of the kidney is by no means cured.

The treatment adopted has been the use of the warm bath, the soap and opium pill at night, and the exhibition of a mixture containing—

> Decoct. Pareiræ Bravæ, ℥ij.; Acid Nitric dilut. 3j.; Træ. Opii, ɱvj. 6tis horis.

She has used this from May 6th to the present time (May 22d), with the benefit you have witnessed, and must continue the same plan a little longer. What further we shall have occasion to do for her must depend on the state she may be in hereafter; possibly blisters near the kidneys may be of service, with small doses of cubebs powder, or extract of colchicum and opium. But of this we must judge by and by.

But disorders of the kidney are much more common as a *consequence* of disease of some other part of the urinary organs. Whatever circumstances cause any obstruction to the passage of urine for a considerable time, will ultimately cause organic changes in the kidney, whether stone, or diseased prostate, or stricture, or whatever the nature of the difficulty may be.

[Mr. Hawkins then described the usual course of such diseases, which were seen in several preparations on the table,—shewing inflammation and softening of the kidney—the formation of abscesses—the

destruction of the kidney, and its conversion into membranous cysts—the wasting of the organ—with the occasional presence of calculi.]

Of the effects of stricture upon the kidney (Mr. Hawkins proceeded), there are two good examples now in the house.

Case III.—The first of them shews you at present the ordinary symptoms of disorganization of the kidney, when in a quiescent condition. It is the case of John Weighell, æt. 49, admitted as long ago as August last. At the time of his admission this man had suffered from a very bad stricture for twenty years, which would not, at first, admit the smallest catgut bougie; besides which, he had a hard cartilaginous tumor enclosing a cavity in the perineum. Some progress was made in the cure of this, when he was very near dying from sudden increase of the renal disease; since his recovery from which I have again succeeded in passing instruments through the stricture, though not into the bladder, as the point is obstructed in what appears to be an abscess in the prostate gland, as a good deal of pus is evacuated separately from and before the urine, the passage of which is occasionally blocked up by the matter.

The symptoms of disease of the kidney are now nearly the same as when he was admitted. In the first place, if you examine the urine, you will find that it contains *albumen*, since it is rendered cloudy and opaque by heat, or by the addition of nitric acid. You are not to conclude, however, from this symptom alone, that the kidney is organically affected, as was at one time imagined, so that the operation for stone would scarcely be performed, if it were present. But if the specific gravity is increased, and there are other symptoms present, then the presence of albumen becomes one means of diagnosis. Secondly, you will find that there is a somewhat *increased* quantity of pale opaline urine, which is highly *alkaline*. It has never been otherwise since I first saw him, although he has taken very considerable doses of nitric and phosphoric acids, with pareira brava and other bitters, combined with opium and hyoscyamus, at different times, since he has been in the hospital. Thirdly, you will find that there is a small proportion of *peculiar mucus* from the kidney. You must distinguish this from the ropy semi-purulent secretion that he sometimes has had, which is formed from the bladder in a state of chronic inflammation. What I mean is a powdery matter floating slightly in the water, not so heavy, and not so globular as pus, but not diffused through the urine, or settling at the bottom, like mucus from the bladder; it is something, as it were, between mucus and albumen in appearance.

These are all the symptoms of renal disease that he has just at present; but you will recollect that he has at different times complained of pain in the loins and sides of the abdomen, *i. e.* in the course of the ureters. This pain has been slight, however, and has always been relieved by a blister, which he has had several times.

It is singular that very considerable renal disease should frequently produce so little pain; sometimes, however, it is more severe, and requires cupping,—cautiously, however, in a weak constitution like this man's; sometimes a seton, or an issue, is advisable, when the pain is constant; or the occasional use of tartar emetic ointment. Sometimes, again, in the condition of kidney which is now before you, small doses of Chios turpentine, or cubebs, or tincture of cantharides, are of service.

But further: you have seen Weighell very nearly dying of a sudden increase of renal disease, to a return of which he is still liable at any time. It was in January last that he became low-spirited and out of health; then he had occasional rigors, pain in the back and groins, with more difficulty in making water; then in a few days he had a great quantity of blood in the water, which came suddenly, and continued for two or three days; and before it ceased, there came away with the urine a very large quantity of pus, which continued for some little time, and then ceased. While he was at the worst, he lay in a state of listless half stupor, with a quick, feeble, intermitting pulse, and the brown tongue of typhus fever; and he had occasional delirium, and was frequently crying from extreme depression of mind, when not asleep and stupid. These symptoms continued, more or less, for nearly a month, when he began to revive and recover strength; and the symptoms I have mentioned, both local and constitutional, and as connected with the urine, gradually went away, and left him as you now see him.

Now, what I believe to have happened during this time, was the formation and bursting of an abscess in one kidney, and that probably the right, from an affection of respiration, with pain on that side, which he laboured under for a few days. The discharge of pus from the kidney occurs in three different states—first a quantity is secreted from the tubular structure of the kidney, and from the infundibula and pelvis, without any cavity like that of an abscess, and while the cortical substance is only inflamed. I have seen this discharge

take place suddenly, and to the amount of many ounces daily; so that it seemed almost impossible that it could have happened from the secretion from a mucous surface only; and yet dissection has shewn that it did so. Secondly, you find small quantities of pus partially confined in cysts, consisting of the infundibula and tubes, enlarged and dilated; these cysts communicating with the excretory tubes. Thirdly, you find circumscribed abscesses in the kidney, not communicating with the excretory tubes: even these, however, you can often trace to the commencement of the tubes, where a drop or two of pus, confined by adhesive inflammation, become the origin of larger collections of matter.

I judge that Weighell had a circumscribed abscess, from the rigors, and so on, *preceding* the purulent discharge, and because there was also a large quantity of blood before the pus, as if the wall of an abscess had been ruptured to give exit to its contents. If I am right on this point, the abscess has since probably filled up.

The treatment which was employed consisted, as you saw, of the exhibition of stimulants to the extent required, nitrous æther, aromatic confection, and tincture of hyoscyamus, and small quantities of wine and brandy occasionally: not ammonia, you observed, however, (which is so good a stimulant in general), on account of the alkaline state of the urine, and the great irritation which this produced in the bladder and urethra. He had also a blister to the right side; and when in the state of greatest oppression, almost amounting to coma, I ordered a blister to the nape of the neck, from the effects of which he felt himself that he derived great advantage. It is a very powerful remedy in that curious stupor and coma that sometimes attend abscesses in the kidney, and which are frequently the symptoms from which the patients die. I once thought that this singular state of half coma was peculiar to the abscess of the kidney, and might be connected with a collection of urea in the blood, since the same phenomenon is present in cases of suppression of urine, and is caused by cutting the nerves of the kidney. I am induced to believe, however, that it has not this origin, as I have seen it in many cases of abscess from other causes, in quite as marked a degree, and lasting quite as long; and many persons who die of abscess in the kidney have no such symptom. In our present patient, too, the quantities and qualities of the urine were not altered during the presence of the stupor; while our next case presented no such comatose symptoms, although there was nearly a complete suppression of urine.

Case IV.—Let us now proceed to the next case, which presents an instructive history to you in several other points besides the disease of the kidney, of which he is now nearly dying, and to which I shall confine my observations. John Colston, æt. 44, was admitted into the hospital nearly at the same time as Weighell, in August last. This man had been in St. Thomas's Hospital eleven years ago, with an effusion of urine, which had destroyed half the penis, and had also left a mere cicatrix in the whole length of the scrotum, caused by the sloughing of the urethra to this extent. Yet, notwithstanding such a serious warning, the man entirely neglected himself till he came to the hospital, hardly able to pass a drop of water, and with fistulous openings in the perineum. You will have observed, that after having dilated this long cicatrix, as well as the original stricture beyond, I was at last obliged to make an opening in the perineum; through which it will be better for him to make water, keeping it open by a catheter, than to allow the water to produce a risk of further effusion of urine in the ragged cicatrix, which is the only canal in front of the opening I have made down to the stricture.

Now as to the renal disease, you will not fail to have contrasted his case with his fellow-sufferer's in this respect—that till very lately the urine has been unchanged; it is even now properly acid; it has never contained any albumen, nor mucus, except that which has been secreted from the bladder; from which latter cause it has sometimes become very readily alkaline on standing for a long time. He presented, therefore, on admission, a much better case for recovery than Weighell, though he now seems in so much more precarious a condition. In fact, till the last four weeks there has been no evidence of renal disease, except once or twice some slight pain in the back, which was each time removed by a blister.

Secondly, you will have noticed the time at which the disease of the kidney shewed itself most prominently. It was not during the long time that he had most difficulty in passing water — when the bladder retained half a pint of water after being apparently empty—when it was so vascular as to bleed from the least touch of the catheter against its interior, which was also so inflamed as to secrete a large quantity of mucus mixed with pus—but it was after all disturbance of the bladder was nearly gone; because he could make water readily and easily, without any straining, through the opening which I had made. I have before pointed out to your notice this circumstance, and the ne-

cessity there is consequently of watching for the least appearance of renal disease at the time the patient begins to be relieved from his stricture or diseased prostate, or stone.

Thirdly, you will have noticed the great pain and tenderness Colston has had in both testes and down the thighs, which takes place sometimes in renal disease; especially if there are calculi; — symptoms, however, which have not been presented to you in the other patient, and which, with other symptoms which I will presently mention to you, incline me to think that there is probably a calculus in the kidney; perhaps in the right, as there has been more pain in the testis of that side. I shew you a beautiful specimen of calculi in the kidney, which I took from a patient who died of this disease of the kidney, produced by stricture, with fistulæ, the fatal symptoms coming on suddenly, as in Colston; because it shews you how the calculi are sometimes formed. You often find small calculi in the tubuli, which drop down into the pelvis, and may escape into the bladder, or may be retained till they increase to such a size as this which I present to you, the centre of which distended the pelvis of the kidney, while several irregular projections shew the manner in which they occupied as many separate infundibula. The centre seems to consist chiefly of uric acid in the parts where it is exposed, covered by a coating of triple phosphates. Here is another portion, however, distinct from the rest, and occupying a single infundibulum, or triangular cyst, which appears to have been formed in a very different way, as it is hollow, and beautifully crystallized on the inside of the cavity; so that it would seem to have been crystallized on the interior of the infundibulum, and gradually increased by deposit on the outside; the first formation being, perhaps (as Dr. Prout has described), in a semifluid state, highly impregnated with uric acid; the calculous matter being deposited almost like the crystallization of a concentrated saline solution.

The urgent symptoms of renal disease first shewed themselves, in Colston, after a long-continued attack of a low form of erysipelas, and he has had three distinct seizures during the last four weeks, each of them exactly alike. The symptoms in each have been wholly different from Weighell's. Each time there has been almost a total *suppression* of urine, about *three ounces* only being secreted in twenty-four hours; while, in the interval of the attack, the quantity has been above *thirty ounces*. The urine is, at the same time, quite black coloured, or of a dark red colour, and contains a good deal of blood dissolved in it; coagulation is, of course, therefore produced in the water by heat. As the water increases in quantity the blood diminishes, and the colour becomes natural. This suppression of the secretion of both kidneys does not prove, however, that both of them are in the same condition; because one gland being disorganized or obstructed, the secretion of the other also will become deranged. Each time, along with some feverishness and great debility, he has had, for three or four days, almost incessant vomiting of dark, thick, unhealthy-looking secretions; sometimes exactly like stercoraceous matter in appearance, though destitute of its smell. This symptom has been somewhat alarming even by itself, from the weakness it has caused; each successive attack rendering him less able to struggle against it. Since the last attack, besides mucus from the bladder, there has also been a good deal of dark pus, which, I believe, comes from the kidney with the blood, and not from the bladder, as it formerly did, in union with mucus. This purulent secretion, however, has not come away, as it did in Weighell, in such a manner as to make me believe that there is a considerable abscess in the kidney, but rather that the suppuration has been the consequence of inflammation supervening to the irritation, perhaps, of one or more calculi: of course, however, I do not mean to be quite positive upon such a point; and, be the exact condition what it may, I think very badly of the case.

I have not much to say as to the treatment of the case, which has been, in principle, like that of Weighell. He has required to be supported, and brandy and soda-water agreed well with the sickness. What did him most good, and relieved each attack, was the giving him small doses of calomel and opium, of which he took about one grain and a half of the former, with a quarter of a grain of the latter, every four or six hours, when the symptoms were at the worst; which seemed to quiet them soon, after common remedies had failed. In the use of mercury, however, in this case, you see a material variation from the remedies employed in Weighell's case, which arose from the circumstance of the acid quality of the urine; every one's experience shewing, that, when the urine is alkaline, mercury must be very cautiously employed. It does not seem, however, to be so efficacious in this present seizure as before. Neither has a blister been of so much service in the interval between the last two attacks, as it was before; and I must use some other form of counter-irritant—tartar emetic ointment, perhaps—when these symptoms again subside.

[Since the lecture was given, Mr. Hawkins left off the calomel, and gave some prussic acid in saline mixture, with ammonia, several times daily; under which the sickness has gone off, and the urine has again become more healthy, but with pus and mucus in it.]

HUNGER A CURE FOR INTERMITTENT.

M. Medwedew, a physician of Mariampol, assures us that fasting is the best remedy that can be employed in the cure of intermittent fever. He recommends that the patient be kept from all food, excepting a little common water for drink, during three days. The treatment may sometimes be preceded by (though it is by no means indispensable) an emetic, or a purgative. But any infringement on the fasting system he holds to be prejudicial to the cure. M. Medwedew has pursued this method for the last five-and-twenty years, in Russia, and always found it effectual. One of his cases is rather curious. A boy, of fifteen, had for three years laboured under ague, with very slight intermissions of suffering. All the usual methods had been tried on him in vain. His abdomen was swollen to a great extent, so as to give him the appearance of a woman far advanced in pregnancy. He was dropsical and lethargic. Three days' fasting, however, completely put him to rights; and a recurrence of the disorder, which took place about six months after, was remedied by adhering to the same practice.—*Medicinische (Vereins-) Zeitung*; edited by *Dr. Hecker*, of Berlin.

MALIGNANT PUSTULE OF THE TONGUE.

In the Prussian official reports we meet with the case of a butcher, who, in slaughtering a beast that was considered diseased, had held his knife from time to time between his teeth, and in two or three days after found the margin of his tongue covered with black pustules. The part swelled alarmingly and most painfully; violent fever set in, and the patient was carried off in the course of sixty hours.—*Heyfelder, in the Med. (Vereins-) Zeitung.*

SELF-SUPPORTING DISPENSARIES.

We are glad to perceive that the conductors of the Penny Magazine have at length taken up the subject of Self-Supporting Dispensaries. Their labours could not be directed to a more legitimate or useful purpose. The more the principle of these excellent establishments is known throughout the country the better; and we suggested it for this reason (about twelve months ago) as one of the most desirable topics for the advocacy of the Useful Knowledge Society.

WEEKLY ACCOUNT of BURIALS,

From Bills of Mortality, *June* 17, 1834.

Abscess	6	Inflammation	27
Age and Debility	40	Bowels & Stomach	3
Apoplexy	3	Brain	5
Asthma	19	Lungs and Pleura	5
Cancer	6	Insanity	14
Childbirth	14	Jaundice	2
Consumption	107	Liver, diseased	5
Convulsions	55	Measles	6
Croup	5	Miscarriage	1
Dentition or Teething	8	Mortification	6
Diarrhœa	1	Paralysis	3
Dropsy	19	Rheumatism	1
Dropsy on the Brain	11	Small-Pox	8
Fever	12	Spasms	2
Fever, Scarlet	9	Thrush	3
Gout	1	Tumor	1
Hæmorrhage	1	Unknown Causes	4
Heart, diseased	3		
Hooping-Cough	14	Stillborn	32
Indigestion	1		

Increase of Burials, as compared with the preceding week . . . 61

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

June, 1834.	Thermometer.	Barometer.
Thursday . 12	from 47 to 60	29·62 Stat.
Friday . . 13	39 64	29·59 to 29·52
Saturday . 14	45 67	29·47 29·48
Sunday . . 15	48 68	29·52 29·61
Monday . . 16	43 65	29·60 29·50
Tuesday . . 17	42 65	29·53 29·66
Wednesday 18	46 67	29·79 29·85

Prevailing wind, S.W.

Generally cloudy, with rain at times; a heavy thunder-storm during the evening of the 14th, when hail, or rather large pieces of ice, fell, measuring $2\frac{1}{4}$ inches round.

Rain fallen, ·875 of an inch.

Charles Henry Adams.

NOTICE.

In reply to the inquiries of a Constant Subscriber, we are enabled to state that Dr. R. Lee is still pursuing his inquiries on the subject. We have not learned that there has been any change of opinion.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JUNE 28, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XXXVIII.

DIFFICULT LABOUR.

INDUCTION OF PREMATURE LABOUR.

Means adopted.—Various modes have been proposed for the purpose of inducing labour prematurely, both by internal medicines and manual operation. Of these, the only positively sure method consists in the destruction of the integrity of the ovum, to be presently more particularly described; for when this is effected, the process of gestation ceases, and that of labour soon commences. The operation requires that we should possess a most accurate knowledge of the anatomy both of the ovum and the maternal structures, and be well acquainted with the state of development which the cervix uteri assumes at different periods of pregnancy; else the most serious evils may result; as is testified, indeed, by the criminal history of many civilized nations. The os or cervix uteri may be punctured or lacerated, and inflammation, with all its direst consequences, ensue. At the Durham assizes, in 1781, Margaret Tinkler, a midwife, was indicted for the murder of Jane Parkinson, who died in consequence of injuries inflicted with the intention of bringing on abortion. On dissection, two punctures through the os uteri were found in a gangrenous state, which had evidently been the cause of death. A similar case is related by Guy Patin to have occurred in Paris; and the nefarious agent expiated her crime by the forfeit of her own life. Within the last few years, a man, professing the character of "an accoucheur," in the immediate neighbourhood of this city, was tried at the Old Bailey for the murder of a pregnant woman; in whose case, with her concurrence, he attempted to get rid of the fruit of conception. Here also the os uteri was transfixed. The scoundrel was acquitted through an error in the indictment.

I merely mention these well-known instances to show you the danger which will inevitably attend any attempt of the kind being made by persons ignorant of the structure of these organs.

I here show you a simple instrument I have contrived for the purpose of putting a stop to the process of gestation in those unfortunate cases where the formation of the woman precludes the hope that she will be ever able to bear a child at the perfection of its intra-uterine maturity; and shall briefly state the manner in which this effect can be best procured.

Duobus sinistræ manûs digitis in vaginam intromissis, index per orificium uteri inserendus est: deindè instrumentum hoc, cui nobis dare nomen αμνιοσχιστης fortasse liceat, illo digito directum, usque ad ovuli membranas adferendum; quo facto ejus punctum occultum dextræ manûs pollice intùs pressum, tenuia fœtûs involucra facilè aperit, aqua uterina per canalem argenteum libere fluit, partûsque dolores mox supervenient.

Unless, indeed, a quantity of water is present between the two layers of the fœtal membranes, the prescribed method will *invariably* induce uterine action earlier or later; but should the amnion still remain entire, gestation may, and probably will, proceed uninterrupted. The time which elapses between the operation and the commencement of parturient pains, varies exceedingly. I have known the uterus begin to act in ten hours, and I have also known nearly a week pass. We usually observe that in from forty-eight to

sixty hours uterine contraction is fully established.

Dr. Hamilton, impressed with the danger the child must sustain by allowing the uterine parietes to come, at the commencement of labour, into close contact with the fœtal body, has proposed to supersede the necessity of the puncture above described; and the following is the method he adopts:—

Ab uteri orificio membranas, digito immisso, ad pollicis circiter spatium undique separat; mucum viscosum ex cervice demovet; et irritationem adeo excitatam satis esse ad partum præmaturum inducendum existimat.

Could we always rely on success following this method, it would, no doubt, be much preferable, both on the mother's and child's account, to the one more commonly practised; but I have found it fail in most of the instances where I have adopted it. But bearing in mind the great advantage of preserving the membranes whole, I have, as on a former occasion hinted, lately been in the habit of administering a medicine, with the same intention that actuated Dr. Hamilton in proposing his method.

I have before me the notes of many cases in which labour was induced *solum secalis cornuti ope;* and I shall trouble you with a brief recital of six. The following is the plan I have generally pursued; and the formula I now give you is as good as can be used.

℞ Secalis Cornuti recentis, in pulverem redacti, ʒiij.; Aquæ ferventis, ℥viij. Infunde vase levitèr clauso per semihoram, et

℞ Liquoris Colati, ℥viiss.; Acidi Sulphurici Dil. 3ss; Syrupi, 3ij.; Spiritus Cinnamoni, 3ij. M. sumantur cochl. duo 4tâ quâque horâ.

Partûs dolores decem vel duodecim elapsis horis ægram vexare, et post singulas medicinæ potiones clare augeri, sæpe inveni; aliquando etiam oriri primo potato haustu.

Case I.—Mrs. Fisher, 9, Mead-Street, Bethnal-Green, a patient of the Royal Maternity Charity, pregnant with her fourth child. Under her first labour she had been delivered by my father, by craniotomy; the second child was born naturally, rather prematurely; of her third I had delivered her by the forceps, with much difficulty. Thursday, July 15, 1830, in the afternoon commenced taking the medicine as prescribed above; being eight months advanced in pregnancy. On Friday evening the uterus began to act with considerable regularity; the pains continued strong throughout the night, but ceased on Saturday morning, when I again saw her, and went prepared to operate in the usual manner. Finding, however, the os uteri dilated to the diameter of a crown-piece, soft, flaccid, and easily distensible, and the head presenting, I did not carry my purpose into execution, but merely ordered a continuation of the medicine, which had the effect desired; for on Sunday, at midnight, she was delivered of a live girl.

Case II.—I was requested by a gentleman having the charge of an extensive parish on the northern side of London, to induce labour for a woman named Hunt, whom I had once delivered by craniotomy, and whom also he had been compelled to deliver subsequently by the same means. She began, therefore, under my directions, to take the medicine somewhat in the form already recommended, on Saturday, Sept. 11, 1830, at 12 P.M., being seven and a half months advanced in pregnancy. On Sunday, in the afternoon, she first experienced pains. On Monday, at 3 P.M., she said they resembled those of labour, recurred every ten minutes, and in the interval she was perfectly easy. Every dose of the medicine made her feel sick, and one had been vomited.

On making an examination I found the os uteri quite close, rigid, and apparently not inclined to dilate. I desired the medicine might be still persisted in. Tuesday, 2 P.M.—Intermitting pains had continued all night; they were much stronger than on the previous day. The os uteri was now soft, and dilated to the diameter of a shilling; the membranes became tense, and protruded into the vagina with each return of painful sensation. I detected the breech presenting.

On Wednesday morning a slight sanguineous exudation shewed itself for the first time. In the course of the day the parish midwife was sent for, but she did not remain. At 10½ P.M. my friend was applied to, as the midwife could not be found; he directed his assistant to proceed to the patient's residence. The membranes broke immediately on his arrival, and in little more than half an hour the child was born dead, the breech being first expelled. The patient took sixteen doses of the medicine, having omitted it when the pains became urgent. She, as well as every other person engaged in the case, was convinced that the effect was to be attributed *entirely* to the remedy. That there was no disposition in the uterus to act previously to the means being used, was proved by the first examination I myself made, 39 hours after she commenced the prescribed plan; when its mouth was quite rigid, and so perfectly close as not to admit even the tip of the finger.

Case III.—Mrs. W., residing in the neighbourhood of the New Post Office; 4th pregnancy. The first child had been born naturally; the second and third had both been delivered by the crotchet. I saw her in her third labour, in consultation with my father and her general attendant, on which occasion we all determined that the conjugate diameter, at the brim, did not exceed 2¾ inches. Friday, Nov. 12, 1830, in the evening she began to take the medicine in the form above prescribed, supposing herself seven and a half months advanced. On Saturday and Sunday experienced uneasy sensations in the back and lower part of the abdomen. On Monday these feelings had increased, with a shooting pain down the thighs, and a sense of pressure. She described them as being not like the commencement of labour, but as though the process had made some progress. There was no coloured discharge, nor any sickness; the os uteri was very low, soft, and dilated more than to the diameter of a shilling; the membranes protruded with each pain; I distinctly felt the *head* presenting. On Tuesday morning the husband requested me to make an early visit, "as a great change had taken place during the night." I saw her at half-past twelve; the pains were then strong and regular, recurring every five or ten minues. The os uteri was almost entirely dilated; the membranes unbroken; there had been a slight "shew" since four A.M. I could not feel any part of the child's body. At two the membranous bag protruded externally; still the presenting part was not discoverable.

Although I had perfectly satisfied myself that the *head* presented on the previous day, I began to suspect that the child had changed its position, and that it was now situated transversely. I therefore prepared to turn, if I found it necessary, and ruptured the membranes; a large quantity of liquor amnii escaped, and the *shoulder* came to the brim. I immediately introduced my hand and brought down the feet. The passage of the head through the superior aperture was effected with difficulty, and occupied between ten and fifteen minutes. On its extraction, a considerable indentation was apparent in one parietal bone, produced by the projecting sacral promontory during delivery. The child gasped two or three times, but could not be restored: from its size, I judged the lady was rather farther advanced than she anticipated. She said that she felt the infant move strongly during the previous night, and suspected at the time that it had changed its posture. She took, in the whole, twenty-three doses.

Case IV.—On Sunday, March 20, 1831, I put Mrs. M——, the wife of a respected medical friend, on the plan I have generally adopted. She was about 7½ months advanced in her third pregnancy. Her first child was born naturally, after a very lingering labour; in her second labour, I delivered her by the long forceps. My father and her husband both concurred in the propriety and necessity of inducing labour prematurely. She had not taken the first quantity an hour before she experienced uterine pains; and these were increased after each dose. On Tuesday, at 2 P.M., the uterus was acting vigorously every five minutes; its mouth was dilated nearly to the size of a crown, the membranes being protruded with each pain. She had taken the medicine twelve times. I requested it might be omitted. From that time the pains began to decline, and at a quarter past 11 P.M. the uterus was perfectly inactive. She went to sleep, and passed a quiet night. At 10 next morning (Wednesday) I requested the medicine might be resumed. At 4 P.M. she was again in occasional pain. She continued the medicine throughout the day and night of Thursday, and till Friday evening. At half-past 9 P.M., the os uteri being almost entirely dilated, and the pains producing more distress than equivalent advantage, I ruptured the membranes with a pointed quill; the pains immediately assumed an expulsive character, and she was delivered of a live child at a quarter before 12 the same night.

Case V.—Mrs. M——, the subject of the last case, became again pregnant, and again I was requested to take charge of her, for the purpose of inducing labour prematurely. On Monday, November 12th, 1832, being eight months advanced, she was put under the same plan which had so satisfactorily succeeded before. In three hours pains came on, and they were increased after each dose. Late on Tuesday evening her husband called on me, to say that the os uteri was dilated to the size of a crown, and soft; that the pains were very strong; that the head was presenting, and that he was desirous of rupturing the membranes: from this I dissuaded him. On seeing her at 12 the next day, I found the os uteri almost entirely dilated, the pains strongly expulsive. I attempted to rupture the membranes with my finger, but could not succeed without using a stilette, in consequence of their preternatural toughness. An immense quantity of liquor amnii was evacuated, the head came to the pelvic brim, and the child was born alive at half-past one the same afternoon.

Case VI.—Mrs. Holliday, 9, Turk-Street, Bethnal Green, a patient of the

Royal Maternity Charity, pregnant with her third child. I delivered her by craniotomy, of her first child, February 29, 1832, and brought on premature labour, by the means now recommended, in December of the same year, her pelvis not measuring more than $2\frac{3}{4}$ inches in the conjugate diameter at the brim. On that occasion the child was born still, and she attributed its death to a violent fright which she sustained after she had begun to take the medicine; after which time, indeed, she did not feel it move. Friday evening, September 6, 1833, began the medicine, being $7\frac{1}{2}$ months advanced in pregnancy. On Sunday morning, having taken six doses, she experienced no effect of any kind from it, the os uteri continuing close and rigid; at 1 P.M. recommenced it; in the evening pains occurred, and continued all night. On Monday morning she sent for her midwife, who remained with her all day. At 10 P.M. the membranes broke spontaneously, and at half-past 11 she was delivered of a live child. She took thirteen doses.

All these patients recovered as well as after the most common labour at the full period; and in none of the cases was there the least indication of a disposition in the uterus to assume its contractile function previously to the exhibition of the medicine.

I could recite many similar instances, equally demonstrative of the power of the drug; but I consider these quite sufficient to establish the fact, and prove the harmlessness of the medicine,—at least as far as the mother is concerned;—and I look upon this method as a great improvement over any previously adopted. In some very few cases, indeed, the remedy has entirely failed in exciting contraction, and in others, in consequence of the inefficiency of the pains, I have thought it right to rupture the membranes when the os uteri had acquired a diameter equal to the size of a shilling, or half a crown; but in the majority, no manual interference of any kind has been requisite.

I have heard it objected to the plan here proposed, that this specific medicine should never be administered with a view of exciting uterine action, unless the os uteri were either entirely dilated or easily dilatable, and unless the vagina and external parts were also in a lax state: and I coincide, to the utmost extent, with the caution inculcated in this observation, under labour at full time, when retarded from rigidity of parts after the membranes have given way; for I have known serious injury result from its being improperly neglected;—as, indeed, I have stated on a former occasion. But in cases where it becomes our duty to anticipate the proper period of parturition by artificial means, it appears to me that less danger is incurred by inducing action through the agency of a medicine, in other respects of a harmless character, than either by stimulating the os uteri manually, or by partially evacuating the uterine cavity. In either case, action is induced before it is presumed that the passages are in a fit state for the escape of the fœtus:—but nature has most wisely and benevolently ordained that, as the process advances, the relaxing influence should keep pace with the power of the propelling organs. This, indeed, we see daily exemplified in those instances where premature labour occurs spontaneously, or as the consequence of bodily accident or sudden mental emotion; and I cannot imagine that greater danger to the womb is likely to result from action excited in the one way than in the other. If, therefore, we are compelled to interfere with and subvert nature's original intentions, it is evidently our duty to use those means which are least likely to occasion injury, and most consonant with the method she herself adopts. If this particular drug, without stimulating other structures, specifically excites the nervous system of the uterus to such a degree as to cause contraction in its fibres, (which I myself, from repeated trials, firmly believe), are we not procuring the greatest effects with the least possible hazard; and are we not, therefore, acting in perfect accordance with all the best-established principles of therapeutics? Does it not stand to reason that less disturbance must be occasioned to the system generally by the means proposed—if the principle laid down be correct—than by irritating the mouth of the womb to such an extent as to induce the organ to get rid of its contents?—and are we not less likely to produce a bruise in the os uteri, by procuring its dilatation while the membranes remain entire, than by letting off the liquor amnii, and bringing the bony head of the child to press upon the undilated orifice, at the very commencement of the parturient efforts?

It is not necessary for me to insist on the abuse to which a medicine possessing such extraordinary powers may be liable; for although, in many cases, we may fail in obtaining the end proposed, the peculiar effects in the instances above related are as completely attributable to its agency as purging to jalap, or vomiting to ipecacuanha.

When necessary.—Unless deformity of the person generally, and of the pelvis in particular, exist to an extreme degree, the induction of premature labour in a first pregnancy is not to be thought of; for it

is impossible to become acquainted with the *exact* size of the different diameters of the pelvis, except under labour; and in the cases ordinarily met with, no one would be justified in having recourse to so serious a measure if he had not accurately ascertained the dimensions by personal examination—and that under the most favourable circumstances for obtaining the required information.

I would warn you strongly, also, against acting on the representations of the patient herself alone; and of the following two cases, one will point out the necessity of such a caution. In the year 1825, I was applied to by a woman, of whom I had no previous knowledge, to induce labour prematurely. She stated, that she had lived at the west end of London, but had come to reside not far from my house; that two of her children had been destroyed, and that twice also premature labour had been induced by a highly-respectable practitioner in the neighbourhood where she then resided. I took the liberty of writing to this gentleman on the subject, who gave me such satisfactory reasons for what he had done, that I had no hesitation in acceding to her request. Since that time I have brought on labour prematurely for that woman on three different occasions.

In the year 1831, my father was applied to by a patient, also to induce labour. She stated that her child had been destroyed in the birth by a physician practising at the western part of the metropolis—a gentleman who holds a high rank in the profession; and that she never could bear a living child at full time. My father took the trouble to see this gentleman, that he might learn from him the particulars of the case, and was informed, that the woman was believed to be unmarried; that she had placed herself under the care of a midwife; and that he had been applied to in consequence of a violent attack of convulsions, which occurred during the labour; on which account alone he had thought it requisite to perforate the head. My father then refused to accede to her wishes; but she, still desirous of placing herself under his care, took apartments in his neighbourhood, and gestation was allowed to proceed to its termination. My father attended her. Some delay occurred in the labour, which induced him to request my assistance, and I delivered her of a living child by means of the forceps. We found a slight contraction at the outlet of the pelvis, which was the occasion of the difficulty experienced. I have great doubts that her object in applying to my father was the preservation of her infant.

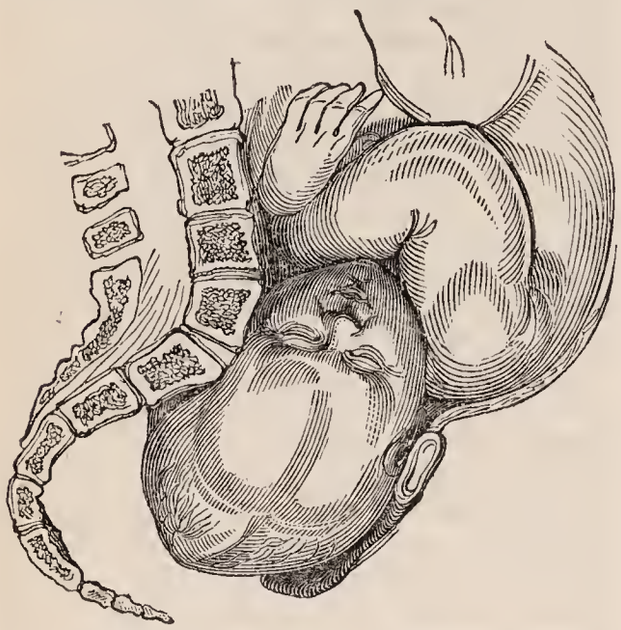
It becomes, then, a question of very great nicety, what degree of contraction would warrant us in proposing the measure we are now discussing. As it has been frequently laid down as a principle, that a child at full time may pass through a pelvis containing in its conjugate diameter at the brim three inches, we may hope, if the space exceed that dimension, that the fœtus may be born living, naturally, provided the outlet be well formed: and we should not be inclined to adopt any means by which gestation might be suspended, unless, indeed, some extraordinary circumstances called for our interference; such as the patient invariably bearing very large children, or other accidental cause equally incontrollable.

If, then, the conjugate diameter measure between $2\frac{3}{4}$ and 3 inches, we may allow pregnancy to advance to the end of eight months; if about $2\frac{3}{4}$ inches, or rather less, to seven months and a half; if about $2\frac{1}{2}$ inches, it must not proceed beyond seven months; if the space be less than $2\frac{1}{4}$ inches, it would be certainly unsafe to delay our means beyond seven months; and I would be inclined to induce labour rather sooner; because children of an earlier period have been reared. In one case I thought it right to bring on labour at six months and a half, scarcely anticipating, however, to save the child: having delivered the patient previously by craniotomy, I had a full knowledge of the very small size of the pelvis she possessed. Even at this early period, I found the child had acquired too great a bulk to pass entire, and I was obliged to open the head. I afterwards learned that this poor creature had gone to Malta with her husband, and there died in labour.

Mr. Barlow, indeed, considers the induction of premature labour an unjustifiable proceeding, except the operation be performed about the end of the seventh month of gestation; and he thinks it most warrantable when the pelvis measures from $2\frac{1}{2}$ to $2\frac{3}{4}$ inches across. His observations would even lead us to infer that he limits its use to that particular dimension; but I cannot help thinking his calculation too low, and that three inches should be the chosen limit.

It is a prejudice as old as Hippocrates, that a child of seven months' is more likely to live than one of eight months' intrauterine age; and it is still in force, not only in this country, but on the Continent. Such an opinion, however, is contrary to experience, and contrary also to analogy and all philosophical reasoning; for we should certainly expect that the longer the fœtus remained in utero, the more completely would the respiratory and digestive apparatus be perfected; and that it would be more likely to be capable of

sustaining an independent existence. This supposition, in fact, we find practically verified; and we should, therefore, delay our attempts until the last day which we think consistent with its passage through the pelvis entire and uninjured.



In this plate of Smellie, you observe the representation of a premature fœtus passing the brim of a contracted pelvis.

Other circumstances may, however, call for our interference in the manner proposed, besides diminution of the pelvic capacity: thus, if it should have occurred to the same woman, in a number of successive pregnancies, to be aware of the death of her infant at a partieular period towards the close of gestation,—about the termination of the eighth month, for instance,—and if the death was to be attributed to deficiency of nourishment, or any other cause decidedly referable to the maternal system, it would become a matter for consideration whether a chance of life might not be afforded to her future infants by the induction of labour before the usual period of their death. In such a case, however, it would be necessary to weigh most minutely every circumstance connected with the ease, and all the peculiarities attendant on it. On one occasion I was consulted by a pregnant woman for a small tumor at the upper part of the thigh, which was evidently of a malignant nature; it increased so rapidly, that it was clear, if she were allowed to attain her full period, it would in all probability have acquired such a magnitude as to preclude the possibility of extirpation. I requested the opinion of my father and Mr. Luke, who coincided with me as to the character of the disease,—as to the hazard of performing the operation under pregnancy,—and as to the danger of allowing the full term of gestation to arrive. I therefore brought on premature labour about the end of seven months: she was received into the London Hospital as soon as was safe; the tumor was removed, and she enjoyed her former health. The child presented with the breech; the labour was somewhat lingering; and it was unfortunately born dead.

Other states of disease, in which the mother's life is placed in imminent jeopardy,—provided there is good reason to suppose her danger is aggravated by the continuance of pregnancy,—may warrant us in having recourse to the induction of premature labour: thus, Dr. Hamilton tells us he has twice resorted to the expedient, with the view of preserving the mother; in one of which cases dropsy induced him, and the other, deadly exhaustion and depressed vital powers. For such anomalous cases, however, it must be evident that no general rule can by possibility be laid down. It would be right, in every instance where premature action is induced, that a wet-nurse should be engaged to take charge of the child immediately on its birth.

ACCIDENTS WHICH MAY OCCUR UNDER LINGERING LABOUR.

A rupture of the vessels of the head may occur under lingering labour, owing to the great excitement suffered, and the excessive action consequent,—producing convulsions or apoplexy. These, however, will occupy our attention hereafter. If the woman be labouring under an aneurism, it may burst, or she may die from a discharge of blood from the lungs or other parts. An internal abscess previously formed may also burst into any of the cavities; but this must necessarily be a rare case. I have known two instances where women under labour have died suddenly, in consequence of organic disease about the chest; in one a large collection of water was found in all the thoracic cavities. The heart's aetion was impeded to so great a degree that the blood could not be propelled forward in proportion as it was received, and the organ was choked. In the other, disease existed in the mitral valve, and the aperture between the auricle and ventricle would not admit the point of the little finger.

I did not see either of these cases during life, but I was present at the *post-mortem* examinations.

Rupture of the bronchi has also occasionnally happened, but it is a very rare occurrence, and I have myself never met with it. Laceration of the vagina and of the uterus may take plaee, but of these I shall speak more at length in the proper place. The bladder may burst also, or inflammation may take place in its neck, and sloughing may supervene. These, too, will demand distinct consideration.

CONSEQUENCES OF LABORIOUS LABOUR.

Exhaustion. — After lingering labours, whether instruments have been used or not, the generality of women recover tolerably well; but occasionally very bad symptoms manifest themselves, the consequence of depression from loss of power, excitement, or injurious pressure.

Sometimes the system falls into a state of exhaustion, from which it never rallies. The symptoms indicating such a state would generally be observed before instruments are had recourse to: under it the mental and bodily powers are completely worn out; the pulse flags; the extremities become cold; there are weariness of the limbs, vomiting, sunken features, and a hollow eye; probably no pain is complained of, and the expression of the face is sufficient to indicate the danger. Stimulants, nourishment, cordial medicines, opium, and æther, are the best and only means to restore the ebbing vitality.

Inflammation of the pelvic viscera.—After laborious labour, the viscera, at the lower part of the abdomen and pelvis, often go into a state of inflammation: suppuration may occur, but it is not usual; the inflammation generally terminates in resolution or sloughing. This state is known by shivering, general fever, and local pain,—by a quick pulse, white tongue, thirst, heat and dryness of skin, want of the secretion of milk, and suppressed lochia, and there is pain on pressing the lower part of the belly. If the uterus feels large, hard, and painful, most likely the inflammation is confined to that viscus; but if the pain is more general, and the swelling less circumscribed, the probability is that the disease is more diffused, and there is a greater chance of its terminating in sloughing. It is not impossible that you may confound this state with puerperal peritonitis; but it is of the utmost moment to distinguish these affections from each other, because the same depleting means are neither required in this case, nor in fact will they be borne.

Suppuration is known to have supervened by occasional rigors occurring,—by the sharpness of the pain experienced,—by the pulse increasing in rapidity and falling in power,—and by hectic. The tongue becomes furred, and there is purging and sweating, and vomiting, and wasting of the body: generally the symptoms increase, and the patient dies; but sometimes the abscess may burst into the vagina, and give almost immediate relief.

DEEP COLLAPSE.

A state of deep collapse may be produced by the extensive contusions and subsequent mortification. The entire prostration of strength, the muttering delirium and watchfulness, the cold clammy extremities, the quick, weak, tremulous, and often irregular pulse, will sufficiently characterise this state; while the purgings and vomitings, and aphthous mouth, will indicate the extent of danger.

Sometimes, the parts, rather by their own healing powers than the aid of medicine, will become restored; the symptoms will gradually abate; the different organs will slowly regain their healthy functions; and after hovering on the brink of destruction for some weeks; by a strong effort of the constitution, the patient will unexpectedly rally. At other times the parts will slough, and various will be the extent of the destruction produced. Occasionally, the bladder, rectum, and all the coats of the vagina, will become gangrenous; the three cavities will be thrown into one; and if the patient survive, of which there will be little chance, most miserable indeed must be the remainder of her life. At others, merely a portion of the mucous membrane of the vagina will slough. Constitutional irritation, various in degree, will supervene, which will cease on the healing process being established, and a cicatrix will be formed, which may impede the easy passage of the child during a subsequent labour.

Treatment.—Little can be done by medicine under this unfortunate condition. The parts may be fomented, and the strength must be sustained. The introduction of a piece of lint, soaked in turpentine and oil, has been recommended to facilitate the slough's separation. As soon as the patient is able to be moved, she should be sent into the country: a change from the close atmosphere of town to a more healthy air has often given a fillip to the constitution, has renovated the sinking powers, and put an immediate check to some of the worst symptoms, especially continued purging.

INABILITY TO PASS URINE.

Inability to pass urine after delivery is not an infrequent consequence of lingering labour. It arises from turgescence of the vessels of the urethra and neck of the bladder, or perhaps from spasm of the sphincter.

The introduction of a catheter two or three times in the twenty-four hours is necessary in every case where the bladder does not void its contents. Occasionally, when you have been obliged to use the catheter for this purpose, the patient, for two or three days, passes her water tolerably well; and after that time, you are mortified to hear that it comes away involuntarily. Under this state, after a laborious labour, you may always suspect that a slough has taken place at the neck of

the bladder, or in the track of the urethra, especially if there be a fœtid discharge, and most particularly if you should be shewn a small piece of what the nurse will call skin, that has passed; this, on washing, will be found to be a part of the neck of the bladder, sphacelated and separated. Your suspicions may be confirmed or annulled by simply passing a catheter into the bladder, and introducing your finger into the vagina, along the course of the urethra; if any portion of the catheter can be felt naked through the vagina, a slough has taken place, and the treatment must be regulated according to the principles to be laid down in an after part of the course.

PRESSURE ON THE NERVES.

Occasionally the nerves suffer; the great sciatic, which lies over a part of the sacro-iliac synchondrosis, is especially exposed to pressure, unless there be a large cushion of fat in the pelvic cavity. Pressure on this nerve, under labour, produces great pain, numbness, and cramps, and sometimes a partially paralytic state after delivery. Nerves in themselves do not possess much restorative power, although usually they regain their healthy state after labour. I never knew an instance in which permanent paralysis existed as a consequence of injury done to a nerve under labour, though I have known pain, numbness, and inability to move, continue for many weeks.

TUMOR ON THE FŒTAL SCALP.

When a child is born after lingering labour, the head having been considerably compressed, we shall usually find a circumscribed tumor in the scalp, at the vertex, or rather on one side, of which I have before spoken. Such a swelling has been often supposed to contain fluid, and I have known it proposed to be punctured, though I never saw the practice carried into effect. There is a feeling of subdued fluctuation in the tumor; but it is not a morbid growth; it proceeds entirely from the collapse of the bones, owing to the compression which they have suffered. Generally speaking, these cases do very well; there is no fluid present, or merely a small quantity in the cellular texture, that does not require to be let out by an operation. As the child grows, and the brain becomes developed, the bones in a few days acquire their natural position, the head its proper shape, and the tumor disappears. It is only necessary to apply an evaporating lotion to the part,—more, perhaps, for the sake of satisfying the mother, than for any decided advantage likely to be derived from it.

ON THE MECHANISM OF THE SOUNDS OF THE HEART;

BEING THE ANALYSIS OF THE FIRST PART OF A MEMOIR,

Read before the Academy of Sciences of Paris, 3d February, 1834,

By M. Magendie,
Member of the Institute of France, &c.

Laennec was the first who paid special attention to the study of the particular sounds of the heart, in the discharge of its function of keeping up the circulation. To him we are indebted for knowing, that during the natural performance of this function, the heart causes to be heard at each of its pulsations, two successive, well-marked sounds, called, by reason of their particular physical characters, the *dull sound* and *clear sound.* But neither the mode in which the production of these sounds is explained by him, nor the explanations given of it since his time, are, or ought to be, admitted. According to Laennec, the dull sound is produced by the contraction of the ventricles at the instant that the blood is sent into the aorta and pulmonary artery, the clear sound depending on the contraction of the auricles: the source of both the one and the other to be referred to the sonorous vibrations, developed in the muscular fibres of the heart, at the instant when they contract.

But to this explanation it may in general be objected, 1st, that it is founded on deductions drawn from the time and place where each of these sounds peculiar to the heart are more especially produced, and not on direct observations; 2d, that the production of the sound by the contraction of the muscular fibres of the heart, is a supposition quite gratuitous; 3d, that even admitting it, we cannot understand how the ventricles of the heart, distinct in their situation and different in their structure, should move constantly with such harmony as never to produce but one single sound; 4th, neither can we understand how, in certain cases, these sounds cease altogether, the heart still continuing to act and to keep up the circulation; whence it would follow that there are sonorous contractions, and others that are not so, &c.

Professor Turner, in a paper inserted in the third volume of the Transactions of the Medico-Chirurgical Society of Edin-

burgh, has particularly objected, that the clear sound cannot be attributed to the cause assigned by Laennec, seeing that the venous pulse of the jugulars, which depends in a great measure on the contraction of the auricles, presents no coincidence with this sound; which, however, should happen, if they had the same origin. But he does not himself offer any explanation of the phenomenon, declaring that he knows none.

In 1830 and 1831, Dr. Hope, relying on numerous experiments, still further objected, 1st, that the contraction of the auricles precedes the dull sound of the heart; 2d, that this contraction is not accompanied by any sound; 3d, that the clear sound of the heart coincides with the dilatation of its ventricles: and, substituting another explanation for that of Laennec, he refers the two sounds of the heart to the sudden displacement which the blood undergoes, either in entering the ventricles, or in its exit from them. But this explanation, devoid of proofs, is itself exposed to the strongest objections. Likewise, since Dr. Hope's work, other explanations have been proposed. Sometimes the sounds have been attributed to the shock of the blood against the parietes of the ventricles at the moment of their dilatation. Sometimes they have been made to depend on the shaking of the aorta and pulmonary artery, under the influence of the blood driven forwards by the ventricles. More recently, M. Rouannet, in his inaugural dissertation presented to the faculty of Paris, endeavoured to prove, by a series of arguments and some experiments, that these sounds are owing, the one to the shock given by the blood on the tricuspid and mitral valves, the other to the shock of the blood on the sigmoid valves of the aorta and pulmonary artery.

In this question, the first point, as we see, is to examine whether the sounds of the heart are developed in the organ itself, as was supposed by all the physicians who endeavoured to discover their origin, or whether they have any other cause. "It is with this view," says M. Magendie, "that I first instituted the following experiments:—

"*First Experiment.*—I listened to the heart, when exposed in a living animal, still keeping up, by its alternate movements, the circulation of the blood. The experiment first made, and several times repeated on small animals, was afterwards made on larger animals, such as dogs and horses; and every time the heart was exposed and insulated, the sound entirely ceased.

"*Second Experiment.*—As mammiferous animals survive only a short time after removing the anterior part of the thorax, and the celerity with which the observation should be made may give rise to error, I wished to repeat the experiments on birds, where the sounds of the heart are well developed, and in whom it is possible entirely to remove the sternum without immediately destroying life. When instituted on geese, the experiment yielded me the same results.

"Thus I at first attained the certainty that the sounds of the heart are not developed in the organ itself—that is to say, that they do not arise from the respective play of its different cavities, no more than from the action of the heart on the blood, or of the blood on the heart."

This point settled, the question is to discover the true origin of this phenomena.

"*Third Experiment.*—I had the idea," continues M. Magendie, "that the sounds of the heart might be produced by the well-known beating of this organ against the parietes of the chest; and I saw no physical reason which could make me reject this idea. On the contrary, I could not understand how a shock on an elastic envelope, such as the thorax is, should not be sonorous. To verify my conjecture, having laid bare the heart in several living animals, I exposed sonorous bodies to its impulse during the systole of the ventricles. Every time these shocks produced distinct, and sometimes very intense sounds, which varied also according to the nature of the body that was struck.

"*Fourth Experiment.*—I raised, with the greatest care, the sternum of a goose; immediately the sounds of the heart ceased, as I already stated: then I replaced the bone in its natural position; and immediately the sounds recommenced, such as they were before the experiment. By raising and replacing the bone, I could easily suspend and produce the sounds.

"With these experiments the following facts are connected:—

"1st. In certain animals whose chest is spacious, and the membranous attachments of the heart flexible, as happens in that species of dog called the *Spanish pointer*, it is sufficient to

make them lie on the back, and thereby separate the heart from the thoracic parietes; when the sound ceases forthwith, recommencing again when the animal resumes his ordinary position.

"2d. Air or water introduced into the chest by pushing back the heart towards the vertebral column gave the same results, so that the animal subjected to this experiment presents no sounds of the heart, though that organ contracted regularly.

"3rd. Even in man, when there exists any mechanical obstacle interposed between the heart and the thorax, the sound of the heart ceases in the same way.

"Thus I acquired, in the second place, a certainty, that the sounds of the heart were produced, conformably to what I had conjectured, by the shocks of this organ against the parietes of the chest."

This point being settled, it still remains to find the cause of the difference of the two sounds.

"*Fifth Experiment.*—I introduced a slip of metal, thin and flat, into the thorax of a dog, so as to prevent the shock of the apex of the heart against the thoracic parietes; immediately the dull sound ceased. The efforts of the heart on the metal plate were perceptibly violent, and if I ceased for a moment to press on the heart, the sound recommenced, the apex of the organ striking against the parietes of the thorax.

"Air or water injected into the left pleura, so as to keep the heart at a distance from the wall of the thorax, yielded me a similar result.

"*Sixth Experiment.*—The same metal plate being laid on the anterior surface of the right ventricle, so as to keep it at a certain distance from the sternum, the clear sound ceased immediately, to commence again the moment the plate was withdrawn. Thus I again attained the certainty, that the dull sound of the heart is evidently the result of the shock of the apex of this organ against the chest; a shock which has been designated for time immemorial, by the excellent name of *pulsation of the heart* (*battement du cœur*), and that the clear sound is caused by the shock of the anterior surface of the right ventricle against the posterior surface of the sternum, and the neighbouring parts of the thorax on the right side, at the moment of the dilatation of the ventricles.

"With respect to this double shock of the heart on the thorax, the one at the instant of the contraction of the ventricles, the other at the moment of their dilatation, it was proved to me by a considerable number of direct observations made on living animals. It is also easy to verify it in man, in certain cases of hypertrophy of the ventricles; for if two stethoscopes be placed simultaneously on the chest, the one opposite the apex of the organ, the other on the lower and left part of the sternum, with a little attention we distinctly perceive the two instruments violently raised at a very short interval, and perfectly isochronous with each of the sounds of the heart.

"To sum up; here, then, if I am not mistaken, is the true mechanism of the normal sounds of the heart:—

"In contracting, and for causes long since known, the ventricles throw the apex of the heart against the left lateral part of the thorax, and thus produce the first sound, i. e. *the dull sound.*

"In dilating, in a great measure, under the influence of the rapid *(brusque)* influx of the blood, the heart gives a shock to the anterior paries on the right of the thorax, and thus produces the second sound, *the clear sound.*

"This explanation, conformable to the general laws of acoustics, and which will henceforward, I trust, form a new point of contact between medicine and physics, is so simple and so natural, that one might ask how it did not come into the mind of physiologists at first, did we not know that in the sciences, as in other matters, simplicity and truth very rarely present themselves in the first instance to our imagination."

But because, if this explanation is the true one, it ought to account for all the particular circumstances connected with each of the sounds of the heart, M. Magendie will examine this question in the second part of his memoir *.

PERMANENT RETRACTION OF THE FINGERS,

Not FIRST DESCRIBED BY M. DUPUYTREN.

To the Editor of the Medical Gazette.

SIR,

IF the following observations should be considered sufficiently important, I shall

* L'Institut. Journal Général des Societés, &c. Feb. 1834; and Dublin Journal.

feel obliged by their insertion in an early number of the Medical Gazette.

I am, very respectfully, &c.

JOHN WINDSOR,

One of the Surgeons to the Manchester Eye Institution, &c.

Manchester,
May 23, 1834.

The history of surgical operations and inventions, by which human afflictions may be cured or alleviated, is a subject not only highly interesting in itself, as tending to shew the gradual improvements in our art, but is also satisfactory, in ascribing to different persons their respective merits in co-operating towards its perfection.

I am indueed to make these observations by having read over the first of Baron Dupuytren's clinical lectures on surgery—on Permanent Retraction of the Fingers, and again the recapitulating remarks at the completion of the course.

In the latter occur the following words:—"Nous avons debuté par une eirconstance heureuse, par un mémoire sur un sujet tout neuf; la rétraction permanente des doigts est une affeetion dont la cause avait été jusqu'alors ineonnue, et la nature au dessus des ressources de l'art: l'étude de l'anatomie pathologique a révelé, à M. Dupuytren, un traitement efficace. On a eherché dernièrement à ravir eette déeouverte à notre profession, en l'attribuant à Sir A. Cooper," &c.

The following extract from my notes of the anatomical lectures delivered by the Messrs. Cline and Astley Cooper, at St. Thomas's Hospital, will shew, I believe, that the merit of having *first* demonstrated the true knowledge of this affection is not at least to be ascribed to the justly eelebrated Freneh surgeon. The following notes were made from a lecture, delivered by the late ingenious and excellent surgeon, Henry Cline, jun., on the 8th of November, 1808; many years, as far as I ean gather from the Baron's lecture (p. 2 to 24), before he became informed of its true nature.

"The aponeurosis palmaris is found just under the skin and fat of the hand; like the aponeurosis pedis, it defends from pressure; it is attached at its upper part to the ligamentum annulare carpi; it is narrow at its origin, then spreads itself out amongst the cellular membrane of the palm, and dips amongst the muscles; it has several cross bands, tying, as it were, the processes togcther. Slitting into tendinous columns, it is inserted into the thecæ of the fingers, and bases of the phalanges.

"One or more of these tendinous columns of the aponeurosis palmaris sometimes becomes eontracted and thickened; most generally one only is affected, but sometimes more, and proportionably so many fingers are bent into the palm of the hand. The treatment is easy and efficacious; it consists in cutting through the aponeurosis with a common knife. In performing the operation, carefully dissect through, fibre by fibre, the aponeurosis, in order to avoid the blood-vessels and nerves underneath; the finger, or fingers, may be kept extended afterwards by a splint, for the flexor muscle has in some degree become shortened, and without this the disease might be reproduced.

"It may be *distinguished* from a contracted state of the flexor muscles, by *all* the fingers being then generally drawn into the hand; or if one only, there is not the same starting into the palm of the hand, when the finger is stretehed, as there is in a contracted state of the tendinous processes of the aponeurosis palmaris;—the latter feels like a very hard cord, raising the skin, but the flexors are too hard to start thus, and are also bound down by the ligamentum annulare."

Having no motive but the cause of truth in penning the above, I hope it will not be ascribed to any invidious feeling; as I entertain no other sentiment towards the Baron Dupuytren but that of admiration and respect for his talents and eharacter—a charaeter requiring no borrowed merit, or misrepresentation, for its rise and support.

OXYGENATION OF THE BLOOD.

To the Editor of the Medical Gazette.

SIR,

SHOULD the following observations be worthy of a place in the Medieal Gazette, I shall feel obliged by their insertion.

Your obedient servant,

GEO. PHILLIPS, Surgeon.

Haverford West, Pembrokeshire,
June 16, 1834.

Does the blood absorb oxygen in its passage through the lungs?

It appears to be a matter not at all yet decided by physiologists, whether the blood, in its passage from the venous to the arterial state, does or does not absorb oxygen; some laying it down as their opinion, that the oxygen of the atmosphere exerts merely a chemical agency, and produces the change from venous blood to arterial, by its affinity for carbon alone, whereby carbonic acid is formed; others contending (and I think more justly) that although unquestionably part of the oxygen becomes expended in this way, yet another portion is carried into the circulation, mixed with the arterial blood. On an analysis of the matter expired, we find carbonic acid, azote, a portion of undecomposed air, and a little aqueous vapour. No argument can be raised on this analysis, to affect the theory in the slightest manner; and I imagine a little attention to the fœtal circulation will set the matter almost beyond dispute.

According to my idea, there can be no question that the fœtus is nourished by its own blood, which is formed in due proportion to the other parts of its economy; or at least if there be any transmission of blood from the uterine arteries to the fœtal in the earliest stage of pregnancy, I conceive it will scarcely be said to take place after the fœtus has once been endowed with life. It has been proved repeatedly by experiments, that an injection thrown into the uterine arteries will pass only into the arteries of the maternal part of the placenta, and injections from the cord will affect merely the fœtal portion; yet I do not think these experiments sufficient to prove that no blood is transmitted from one to the other. We can very rarely inject the veins by any fluid thrown into the arteries; yet there can be no doubt that blood is transmitted. The fact that on a vein of the arm being opened, and a ligature drawn sufficiently tight above, to stop the pulsation of the artery, that vein will soon cease to bleed—that on a free opening being made into a vein of any magnitude, the blood will continue to flow until syncope (a state in which the action of the heart and arteries become almost suspended) supervene—justify such a conclusion; but in the circulation of the fœtus we have no such evidence. Dr. Dewees, the American writer on midwifery, states it to be his invariable practice to tie the fœtal end only of the umbilical cord: he says, not only does no hæmorrhage ensue more than of the blood contained in the fœtal portion of the placenta, but that the uterus contracts, and the placenta is expelled more readily than when both ends are secured. Now, if blood be transmitted from the uterine arteries to the fœtal portion of the placenta, surely, under such circumstances, hæmorrhage most fatal to the mother must be the invariable result. Then how does the fœtal blood undergo that change which the economy of the fœtus requires?—it must be by the placenta producing the same change in the state of the blood as the lungs do in that of an animal which has once breathed. There exists a great analogy between the lungs and placenta; the blood is conveyed from the system to the lungs by the pulmonary artery, to the placenta by the hypogastric; it is brought back to the heart by the pulmonary veins, in the fœtus by the umbilical: there are cells in the placenta which will bear somewhat of an analogy to the air-cells of the lungs, yet with this similarity of structure, how could the change take place, unless oxygen were absorbed by the arterial blood? The parts which constitute the fœtus are composed of the same elements before as after it has breathed; its blood has to circulate through fat, muscle, bone, just as in the child after birth. How, then, can it get rid of the carbonaceous matter contracted during its passage through these bodies, unless by means of the oxygen carried into the circulation by the arterial blood of the mother?

TREATMENT OF ERYSIPELAS BY INCISION.

To the Editor of the Medical Gazette.

Sir,

As my claims to originality in the treatment of erysipelatous inflammation by incision have been called in question, and as it is a practice so uniformly successful, whenever it has been had recourse to in proper time, and according to the directions prescribed in my original paper, I shall feel obliged by your

inserting the subjoined article in your next number.

I have the honour to be, sir,
Your very humble servant,
A. Copland Hutchison.

Duchess-Street, Portland-Place,
June 21, 1834.

In the fourteenth volume of the Medico-Chirurgical Transactions of London, page 77, in adverting to the treatment of erysipelas by incision, Mr. Lawrence says—"Although this practice has been probably adopted heretofore, being incidentally alluded to by O'Halloran* and Pott†, and more directly mentioned by Vogel‡, we are indebted for its recent introduction into this country to Mr. Copland Hutchison, who strongly recommended it in a paper written expressly on the subject, first published in the fifth volume of the Transactions of this Society, and subsequently re-published, in a somewhat enlarged form, in his Practical Observations in Surgery, second edition."

If this be not a direct attempt, on the part of Mr. Lawrence, to take from me the merit of having been the *first* who ever *suggested and practised* such treatment in that disease, I must be equally unacquainted with the force of language, or of its tendency.

Upon a reference, however, to the two first authorities quoted, the reader will not find one syllable on the subject, unless Mr. Lawrence means to apply the term erysipelas to gangrene and sphacelus; and as to the more direct mention of the treatment in "Vogel's Handbuch," he will find Mr. L. equally unfortunate; for the identical edition referred to (and now in the library of the Royal College of Surgeons) was published so lately as 1820, whereas my original paper was published, in 1814, in the 5th volume of the Transactions before adverted to, after I had practised the plan of treatment by incisions for upwards of five years antecedently to 1814, at the Royal Naval Hospital at Deal, as my assistants, and the numerous medical officers of the navy then attached to the Scheldt fleet and Downs station, can testify.

Mr. Lawrence's error has done me great injury, which I have reason to believe he could not have intended; and it is only now at the eleventh hour I have read with honest pride that there are some of my professional brethren who really do acknowledge me to be the author of this mode of treatment. Among that number, I am gratified to perceive that the distinguished Surgeon-General to the forces in Ireland, though personally unknown to me, has, in the true spirit of liberality and candour, given me the credit of having been the first to adopt this new treatment in erysipelas, which, he says, "some of my own countrymen have denied to me*."

It is only within the last eighteen months that I have been able to see Vogel's work, so as to satisfy myself of the facts of the case as they are here stated, and to allow time for Mr. Lawrence to correct the error himself, which, on two separate occasions, when I mentioned the statement to him, he promised to do,—otherwise this correction of Mr. Lawrence's should not have been so long delayed by me.

"* Complete Treatise on Gangrene and Sphacelus, 1765.

"† Works, 1783. Vol. i. p. 22.

"‡ There can be no doubt that, in very acute erysipelas, incisions into the part are more efficacious than any other measure, in giving issue to stagnant fluids, and dispersing the mischief. Attention ought to be paid, at the same time, to the state of the juices; and suitable internal remedies ought not to be neglected. In this way suppuration and other bad consequences may be prevented.—*Handbuch*, vol. iii. page 358."

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* Mr. Crampton's words are:—

"The successful management of this disease I look upon to be the greatest triumph of modern surgery. You all know how much stress I have ever laid on the necessity of early and vigorous measures in the treatment of this affection; and I trust that the present case will add one more to the many successful cases which we have had of this disease since our session has commenced. Among others, the boy in the large clinical ward, with phlegmonoid erysipelas of the knee and thigh from a wound; the man with a similar affection of the fore-arm, from a wound on the elbow; the woman with erysipelas of the fore-arm and arm, from a slight contused wound of the elbow, on whom I operated last week, and whom you saw this morning among the externes nearly well, are all cases of the successful treatment of this disease.

"It affords me great satisfaction in having an opportunity of rendering that justice to Mr. Copland Hutchison which some of his own countrymen have denied to him. To him we are *exclusively* indebted for this great improvement in the treatment of erysipelas. Like all great improvements, it was at first met by a denial of its utility, and then by attributing it to somebody else; but I have no hesitation in thus publicly offering my acknowledgments to him for one of the most valuable improvements which have been made in the practice of surgery within our times."—*Med. and Sur. Journal*, April 1834.

DR. ELLIOTSON'S ACCOUNT OF THE ACTION OF THE HEART.

To the Editor of the Medical Gazette.

SIR,

WILL you oblige me by inserting the enclosed in this week's Gazette?

I am, sir,
Yours faithfully,
A CONSTANT READER.

June 24, 1834.

I notice that the account given by Professor Elliotson (at page 343, *et seq.* of your No. for June 15, 1833) of the action of the heart, and the physical signs of disease of the valves, is (with one exception) conformable to the principles of Dr. Hope, as explained at pp. 44, 49, 55, and 337 of his "*Treatise on the Diseases of the Heart.*" The exception refers to the fact advanced by Dr. Hope, that a murmur accompanies the first sound, when the mitral or tricuspid valve is permanently open, and admits of regurgitation. As my own observation places this fact beyond doubt, in my mind, and as it must inevitably be true according to the principles of analogy, I beg to recal to it the attention of the learned Professor, and of those whom his authority may influence; since they will otherwise meet with cases which, being inexplicable on their principles, will breed doubts in their minds, as they did in that of the immortal Laennec, respecting the truth of the physical signs in general.

Dr. Elliotson also says, "In the experiments I saw (viz. the second series of Dr. Hope's, on the ass), the auricle contracted with all sort of irregularity—sometimes before the ventricle, and sometimes after it." At p. 33 of Dr. Hope's Treatise, I find, that, to the written question, "*Do the auricles contract before or after the ventricles?*" the answer dictated by twelve gentlemen, including Dr. Elliotson (ibid. p. 11), was, "*evidently before, being instantly followed by the ventricular systole.*" The irregularity to which Dr. Elliotson refers, takes place only when the heart is acting altogether irregularly. Of this, any one may satisfy himself by the experiments on rabbits, conducted in manner described by Dr. Hope, at p. 33, and which I have witnessed at his lectures.

CLAIMS OF PROVINCIAL HOSPITALS TO QUALIFY FOR EXAMINATION AT THE COLLEGE OF SURGEONS.

To the Editor of the Medical Gazette.

SIR,

I HAVE just perused, in your last number, for June 14, Mr. Hetling's letter, accompanying a copy of the Memorial addressed by my colleagues in the Medical School of Bristol and myself to the Parliamentary Committee appointed to inquire into the state of medical education. The paragraph which you append to Mr. H.'s letter, in which you very fairly imply that the memorial accompanying it could not be considered as a reply to the remarks contained in your leading article of the 31st of May, on the subject of provincial surgical education, inasmuch as that Memorial was little more than an echo of the statements contained in a similar document transmitted by the surgeons of our Infirmary, has induced me to trouble you with the following observations in reference to the subject at issue between us; and I shall feel obliged by your publishing them in the earliest forthcoming number of your valuable journal in which you can devote to them the necessary space.

In the first place, sir, I may congratulate the gentlemen with whom I am associated, and all who take a similar view of this question with us, at the concession you have made in allowing to the Bristol Infirmary, and "various other noble institutions" in the country, as much claim to the privilege of qualifying students for appearing at the bar of the College as can be possessed by Edinburgh, Glasgow, and Aberdeen, which at present have this privilege granted them. This admission, on your part, narrows the ground which might have been supposed to be in debate between us; and while it renders it unnecessary for me to say any thing with a view of exalting the benefits and advantages of the Bristol and other hospitals as schools of surgical instruction, reduces your argument in favour of the existing obnoxious regulation (so far as this regulation applies to students from large provincial hospitals, the only class of students in favour of whom we claim

exemption from its operation) to the simplest point of its desirableness, for the sake of insuring to the student the opportunity of witnessing a *variety* of practice, and for that correction of erroneous views which is supposed to result from the accumulated experience and the multiplied opportunities of observation and comparison enjoyed by the teachers of the metropolis.

With regard to the first of these objects, viz. the attainment of a *varied* field of observation, it may be said, that in perfect consistency it *ought to be* as compulsory on the student who has been for a certain period at a London hospital afterwards to attend, during six months, the practice of the Bristol or Manchester, or some other large provincial hospital, as by the existing regulation it *is* compulsory on the Bristol or Manchester student of four or five years' standing to prolong his studies for six months in the metropolis. But it appears to me, sir, that an error, very common to persons engaged in the metropolis, and especially to those who seldom or never have opportunities of becoming *personally* acquainted with things in the provinces, has crept into your argument on this subject;—I allude to the mistake of supposing that the provincial mind (if I may be allowed the expression) is far behind the intellect of the metropolis. Such "local habits and peculiarities of practice" as are alluded to in your article might have been substantial grounds of objection a hundred, or fifty, or even twenty years back; but the press, and the rapidity and ease of communication, both personal and by correspondence, now existing, make pretty ample provision against the possibility of the poor provincial surgeon being long ignorant of the improvements which his science makes in her most favoured seats, whether on the Thames or the Seine; though perhaps the accumulated experience of, and the vast opportunities of amassing knowledge in, the provinces themselves, may pretty well vie with the practical advantages of London, notwithstanding that the latter are by some presumed so greatly to exceed the former. Not that I have the slightest hesitation in assenting most fully to your general proposition, with regard to the beneficial effects of collision upon intellectual pursuits; nor can there be a shadow of doubt but that the man who has seen the practice pursued in a multitude of places (especially if such different spheres of observation were placed *in different countries*), and who has digested what he has seen, will be the superiorly-educated practitioner to him whose opportunities have been confined within the walls of one hospital, whether in London or elsewhere. But I contend, that if this were the ground taken by the College in enforcing the regulation in question, it would not have stopped, nor ought it to have stopped, where it does. Not only should the privilege sought for be conceded to the large provincial hospitals, with respect to students of the metropolis, but, to be operative to any important extent, the rule should have extended its requirements to the production of certificates from foreign hospitals; where a varied field of observation, and opportunities of correcting *national* (much more likely to exist than *local*) prejudices, would really be obtained. But there is no better proof that the *assumed* grounds are not those on which the College have legislated, than the fact that a young man who has been reared in a provincial hospital, and has afterwards spent a considerable period in all the most celebrated schools of France, Germany, and Italy, is still inadmissible to examination at the College of Surgeons in London, unless he shall have submitted to the farce of paying for six months' *walking* of a London hospital. And truly, Mr. Editor, I cannot think I am risking too strong a term in calling that a farce which is continually a known compliance only in name, except so far as the payment of the fees is concerned: for nothing is more notorious than the frequency with which young men go up from large provincial hospitals, to enter to the six months' walking pupilage required of them in London;—then, finding themselves jostled about by a throng so large as most effectually to prevent the possibility of their seeing any thing which can be of advantage to them, after a short time, leave town, cross over to Paris, and spend there (where opportunities of improvement are so accessible) the time required to be spent in London. Other students, whose purses are not long enough to enable them to take this course, are, on the contrary, obliged to

nates, and to prolapse before either of those parts, as soon as the membranes break; which cases, as we shall hereafter learn, require little or no artificial interference. It becomes our duty, then, as soon as a hand is detected, to examine most minutely, for the purpose of ascertaining whether the head, breech, or shoulder, be at the brim; and to act according to the information we then obtain.

Side.—The side of the child may present, and here also we have some negative as well as positive marks to guide us. The side possesses neither the roundness nor the firmness of the head, nor any sutures or fontanelles; neither is there the double rotundity nor the soft fleshy feel of the breech: we cannot distinguish the parts of generation nor the anus—we can feel no other part of the child except the ribs, and, it may be, the arm also. The side may be principally distinguished by the spaces between the ribs; and if two of these can be clearly traced, there can exist no doubt as to the presentation. The head is the only part of the body for which the side is likely to be mistaken, and I have actually known this mistake occur. If we could only feel two ribs and one intercostal space, it might be possible for us to be deceived; we might suppose the margin of the ribs to be the edges of the parietal bones, and the space itself the sagittal suture. But I have already instructed you, that if there be any doubt with regard to the presentation, under the mode in which we usually examine—with the first finger of the right hand—two fingers of the left should be introduced, or that we should even pass in the whole hand, and ascertain the presentation, rather than allow hour after hour to elapse in doubt on so material a question.

Back.—A child may present with its back, although this is a very uncommon position for it to lie in. Three or four of the spines of the vertebræ can be felt by the fingers; and we can also detect the origins of the ribs, even before the os uteri is completely dilated. When the back presents, it is of no consequence whether the part directly over the os uteri be near the shoulder or the breech, —whether we feel the lumbar or the dorsal vertebræ, the same thing is required to be done—the breech or the legs must be brought down by the introduction of the hand, and the case terminated by turning.

Chest. — The chest may present,—any point of the sternum meeting the finger. There would be difficulty in detecting this presentation by the first finger of the right hand; but by introducing two fingers, or the whole of the left hand, we shall feel the sternal bones, the continuance of the bony plane, the ribs, or rather the cartilages, at their origin from the sternum, and the intercostal spaces. This is perhaps a rarer presentation than any of those I have previously mentioned.

Abdomen. — The rarest presentation of all is the abdominal. Out of more than 120 transverse presentations in which I have myself operated, I have only met with this peculiar position once. Smellie has furnished a plate, in which he has delineated the navel as the presenting part. If the case were allowed to proceed without interference, the abdomen would be squeezed somewhat through the brim of the pelvis after the membranes break. There can scarcely be a chance of mistaking this presentation. We shall feel the large, soft abdomen, possessing no osseous formation; we shall perhaps be able to distinguish the ensiform cartilage; but a more positive mark is the insertion of the funis umbilicalis. Whenever we can feel the commencement of the cord by the finger, there exists a belly presentation most undoubtedly. Do not, however, let us suppose that all cases in which a fold of the funis comes down into the vagina must turn out presentations of the abdomen, because the cord frequently prolapses when other parts of the child are at the brim.

Modes in which the operation of turning may be performed.—Having determined that the case under a transverse presentation is not to be left to nature—that it is more likely that the uterus will rupture, or that the patient will die exhausted, than that the child will pass double—we must make up our minds to change its position by an operation. First, then, let us inquire in what that operation consists; and, secondly, what period of the labour we shall select for its performance.

Three different modes have been recommended, and they all, perhaps, enjoy their peculiar advantages.

The first is, that we should raise the presenting part, introduce the hand into the uterus, seize hold of the head, bring it to the brim of the pelvis, and convert the case into a natural presentation. The second advice is, that we should introduce the hand into the uterus, run it along the abdomen of the child, take hold of the breech firmly, grasp it with the fingers, bring it to the pelvic brim, and make it a breech case. And, lastly, we are told to introduce the hand as high as the fundus uteri, running it along the body of the child, search for the feet, and, bringing down one or both, make the child perform a complete evolution, and extract it footling. The ancients were fully impressed with the danger of transverse presentations; and in the works of Ætius, who was a compiler from previous authors,

many hints may be found relative to bringing down the feet by turning, under such an unfortunate situation. The operation, however, was not generally adopted till the time of Parey, to whom, although he be not the first suggester of this great practical improvement, is justly due the honour of having satisfactorily proved its safety and utility, and enforced its adoption, both by his precepts and example. Before Parey's time,—the middle of the sixteenth century,—there seem to have been instituted no precise rules for the management of this order of preternatural cases; it was the almost invariable practice to endeavour to bring the head over the pelvic brim, but, failing in that attempt, most writers directed that the child should be extracted in whatever manner was most practicable. In Celsus, indeed, we find it recommended, that *if the fœtus cannot be brought into a proper direction*, the head should be separated from the body, as the only likely means of accomplishing delivery. —"Remedio est cervix præcisa; ut separatim utraque pars auferatur."

Of these three modes, that of raising the shoulder and bringing down the head would be the safest to the child, because there would then be no chance of pressure on the funis umbilicalis; and it is that pressure which usually destroys the fœtus under the operation of turning;—but, although safest for the child, it is the most dangerous to the mother, as well as the most difficult to the operator; and the danger, as might be expected, is in proportion to the difficulty. The form, size, and slippery nature of the cranium, all combine to produce this difficulty. Even although the shoulder may be raised from the brim, and pushed entirely out of the way, it is no easy matter to grasp the head, so as to bring the vertex over the centre of the superior aperture; and in these attempts, which will most likely require to be repeated, lacerations and contusions of the uterus or vagina could scarcely be prevented. From the danger and difficulty accompanying this operation, it is now, I believe, entirely abandoned in England as a means of delivery under transverse presentations, although recommended by Dubois, in France, as applicable to some few cases. Thus the last reports from *La Maternité* inform us, that out of fifty-nine cases of transverse presentation, two were terminated by bringing the head to the pelvic brim.

Delivery by the breech offers an expedient second in degree safe to the child; for, as already mentioned, when the legs and feet lie up against the child's body, the funis umbilicalis is, to a certain extent, protected from pressure, and the passages having been considerably distended by the transit of the breech, are rendered in a fitter state for the easy escape of the shoulders and the head, than when the feet are first brought down. But this mode of acting is also both difficult and somewhat dangerous, and that from the same causes as embarrass the operator in the attempt to bring down the head. We are not able to encompass the breech readily; we cannot easily grasp it, so as to bring it over the pelvic brim, in consequence of its bulk and form.

The third means is by far the most dangerous to the child, but by far, also, the safest to the mother—that of grasping one or both feet, bringing the breech, through the hold they afford, into the pelvis, and extracting the fœtus by their agency. This is the mode of delivery which I myself adopt, and which I strongly advise you to follow in all cases where there is a necessity for turning the child.

Period when the operation should be performed.—The time most favourable for the operation of turning, is when the os uteri, vagina, and external parts, are perfectly relaxed, while the membranous cyst remains still entire. The operation, then, should be delayed as long as is consistent with the integrity of the membranes, and the preservation of the liquor amnii within the uterine cavity; for the presence of the water allows the easy introduction of the hand completely within the womb, and permits the child to be turned, by means of the feet, in any direction which is desirable. If, then, we have the conduct of the case from the commencement, let us operate before the membranes break.

But again, we cannot,—and it would be idle to suppose that we could,—pass our hand into the uterus before dilatation has gone on to some extent both in the vagina and in the os uteri. We could not expect to be able to effect our object, when the os uteri is not opened beyond the size of a sixpence or a shilling, unless, indeed, it be much softer than usual under this slight degree of dilatation; but when it has acquired the diameter of half a crown, or a crown, it will generally suffer itself to be dilated to such an extent as will admit the hand, without injury to its structure.

Since, then, we have ascertained that it is highly desirable, under a transverse presentation, that the position of the child should be changed previously to the rupture of the membranes, but that it is almost impossible to accomplish this object before the os uteri has acquired a certain diameter—since, also, there is great danger of the membranes breaking as soon as the bag occupies the vagina, so as to press at all upon the external parts—it becomes of importance that we should lay down

tol, Birmingham, Manchester, &c. a powerful stimulus to the surgical teacher of carrying out the benefits of clinical practice, by the discourses of the lecture-room, is withdrawn. Surgical lectures, in short, which would form so great an assistance to the pupils of our hospitals, will continue to be left by the majority of them entirely till they go to London, seeing that in six months they can attend the two prescribed courses; and thus one of the greatest advantages of lectures, viz. their serving to connect in the minds of the students principles with practice, at that most important period of their pursuits when the mind most needs such assistance and direction, will be, as it now is, to a great extent lost.

My paper has already reached a greater length than I at all contemplated when I took up my pen; and I will not any further intrude upon your indulgence than to subscribe myself, sir,

Your most obedient servant,
G. Downing Fripp.

Bristol,
June 18, 1834.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'Alembert.

The Transactions of the Provincial Medical and Surgical Association. Instituted 1832. Vol. II. 1834.

We promised to return to this valuable volume. In our former notice, about a fortnight ago (p. 355, *ante*), we took a general view of the contents, and gave the substance, or pointed out the nature, of some of the articles. Some others, however, we reserved for a more convenient opportunity; and accordingly we resume with

Dr. J. L. Bardsley's further Remarks on Strychnia.

The author, in the commencement of his paper, is anxious to correct an error into which some practitioners have fallen, in imagining that he had proclaimed strychnia, in his "Hospital Facts and Observations," as a specific in every form of paralysis. What Dr. B. has actually stated in that work is, that this remedy was employed in "*some* cases of paralysis with no benefit; in others with only partial advantage; but in the majority with complete success;" and it is added, that "whenever paralysis arises from tumors compressing the substance of the brain, or from a diseased alteration in its mass and structure, or from extravasation of a fluid in such a state or degree as will not admit of its absorption, it will be readily admitted that no benefit is to be expected from the employment of any remedy which has yet been discovered." The opinion expressed in these passages is abundantly confirmed in the paper before us, by reference to various weighty authorities, and to cases since met with by the author, or communicated to him by professional friends. Dr. Booth, of Birmingham, among others, has supplied some curious experiments, which serve well to exemplify the powers of strychnia; they fully suffice, at all events, to dissipate Dr. Christison's doubt about the action of this alkali on the brain.

"Experiment I.—Tuesday, April 26, 1831: on a shepherd's dog, about one year old, weight 24 lbs. At 4 P.M. one-sixth part of a grain of strychnia, inserted into a small piece of meat, was thrust down his throat. The animal was closely watched until 7 P.M. but no effect from the pill was observable. 27th, 3 P.M. another pill, containing one-fourth of a grain of strychnia, was given. The dog was watched as before, but no effect was noticed. The dose (gr. $\frac{1}{4}$) was repeated on the 29th and 30th, but equally without any apparent effect having been produced by it. May 1st, 4 P.M. a pill, containing half a grain of strychnia, was administered; at a quarter before five, no effect was observable; at five he was found lying on his side, with his legs extended, mouth open, and his whole body in convulsions. His mouth would frequently close with a loud snap; the pupils of the eyes were alternately dilated and contracted. Respiration was carried on with difficulty, and the action of the heart was very irregular; sometimes contracting ten or twelve times in rapid succession, then suffering an intermission of three or four seconds. The animal continued in this state till ten minutes past five o'clock, when, the convulsive action of the muscles gradually subsiding, he died.

Sectio cadaveris, 4 P.M. *May 2, twenty-three hours after death.*—The calvarium, and the exterior portion of the vertebral column, having been removed, so as to expose, at one view, the brain and medulla spinalis, the following were the appearances noticed:—

Brain —The blood-vessels injected, and beautifully arborescent. The pia mater covering the medulla oblongata particularly vascular or injected. The choroid plexus very vascular. No serum in the ventricles. In cutting into the substance of the brain, several dotted points were observed.

Medulla Spinalis.—In the bony cavity of the spine there was much congestion about the sinuses, and great vascularity of the membranes about the upper dorsal and the lower lumbar vertebræ. The dura mater of a light pink colour, and turgid. The arachnoid membrane natural. The internal structure of the cord itself was dotted with sanguineous points.

Thorax.—In the cavity was contained from about ℥ss. to ℥j. of fluid. The lungs were collapsed and congested, but did not sink in water. The heart was turgid, and on both sides filled with black blood.

Abdomen: Stomach. — The mucous coat had a slight pink blush.

Intestines.—In the *ileum* were found several patches, varying in magnitude from the size of a sixpence to that of a shilling; also an oblong patch of four or five inches in length, and a finger's breadth, apparently the seat of ulceration in the mucous coat of the intestine. It had a reticulated appearance. The aggregate and solitary glands were seemingly the parts affected. In the *colon* the same appearances were more sparingly found. In the cœcum and rectum numerous prominent pustules or pimples, with a point of depression in the centre of each, and containing a viscid fluid, were very conspicuous, giving, altogether, an appearance not dissimilar to small-pox pustules in the early eruptive stage."

"EXPERIMENT II.—May 1, 1831: on an old terrier dog. At 4 P.M. a pill, containing a quarter of a grain of strychnia, was thrust down his throat, enveloped in a small ball of fat. The animal was carefully watched for four hours afterwards, but no appearances indicative of any influence from the strychnia presented themselves. May 2, at 4 P.M. another pill, containing the same quantity of strychnia (gr. ¼), was given. The dog was watched for four hours afterwards, but no symptoms of inconvenience were observed. May 3, at 4 P.M. the animal swallowed the same dose, in the same manner as before, and was similarly watched, but no observable effects were produced by it. May 4, at 4 P.M. another pill, containing a quarter of a grain of strychnia, was administered. The animal was watched till seven o'clock (three hours after having taken the pill), but no symptoms of its having taken effect were observed. May 5th, 10 A.M.: on visiting the dog, with the intention of giving him food, he was found lying on his side, quite dead and cold; his limbs stiff and extended. A quantity of frothy mucus adhered to his mouth. His jaws were so firmly closed, that considerable difficulty was experienced in forcing open his mouth; the muscles, generally, were in a spastic state.

Sectio cadaveris, 4 P.M.—External appearances: the limbs were in a state of spastic rigidity. The *brain*, although rather gorged with blood, was not equally so with the preceding subject. The choroid plexus congested and dark-coloured. No effusion in the ventricles. Of the pia mater, some of the inflections following the convolutions of the brain were very vascular. An hydatid was found in the inferior horn of the right lateral ventricle.

Spine.—On opening the osseous canal of the spine, no effusion appeared to have taken place between the membranes of the spinal marrow. The surface, however, of the pia mater, about the cervical and upper dorsal vertebræ, was vascular. The structure of the cord itself was healthy, excepting that the lumbar portion was rather softer than the upper.

The Thorax.—The lungs, on both sides of the thorax, were collapsed, dark-coloured, and gorged with blood, and barely buoyant in water. The trachea, internally, was of a bright pink colour, and coated throughout its ramifications with a frothy mucus. The pericardium exhibited an inflammatory blush, together with recent depositions of lymph both on the reflected and proper surfaces of the membrane. The heart contained no coagula in its cavities, neither did the large veins or arteries. The heart was distended with dark blood; the blood had an unctuous appearance.

Abdomen.—Stomach: its villous coat along the greater curvature was softened, and of a brownish colour.

Intestines.—In the duodenum, softening of the mucous coat, with two or three ulcerated patches, was observed; in the ileum, particularly at its junction with the cœcum, ulcerations, having pits or foveæ, like the depressions on the rind of a rough or coarse orange, were conspicuous. The colon throughout was studded with small prominent elevations, each having a depression at the apex, and not unlike the immatured pustules of small-pox. This eruptive appearance was more particularly observable about the appendix vermiformis and rectum. The aggregate and solitary mucous glands of the intestines were undoubtedly the seats of this appearance."

"As a candid inquirer after truth," says Dr. Booth, "the preceding facts and observations have irresistibly impressed my mind with a conviction of the fallacy of the hypothetical doctrines, according to

which an entire class of vegetable poisons (the bitter strychnos) is said to possess the singular property of expending, through the medium of the circulation, their influence on the spinal marrow, without directly involving the functions of the brain, or uncombined with any injury of that organ. Paris's Pharm. (Ed. vi.) vol. i. p. 24. Christison on Poisons, (Ed. ii. p. 754.) The action of strychnia on the spine is represented as even quite *independent* of any action on the brain, "*if, indeed, such action exist at all.*" (Christison on Poisons, 2nd ed. p. 19.) To the same effect Orfila has recorded the result of an experiment, (on Poisons, vol. 2, p. 270, 2nd edit.) but I may venture to observe, that the very irritation itself, consequent on dividing the spinal marrow of animals, will keep up spasmodic action; and that, with respect to indications of the state of the brain during such experiments, we do not possess adequate criteria by which we can, in animals, satisfactorily determine the fact. In man, however, we have indisputable means of assuring ourselves of the participation of the brain in the energetic influence exerted by strychnia on the spinal marrow and its nerves." The representations made by the patients themselves, almost uniformly, of their suffering pain in the head, and vertigo, *previously* to the invasion of the spinal convulsions, and during those convulsions; the palpable indications afforded of the nerves supplied by the brain being affected in the nictitation of twitching of the eye-lids, and agitation of the muscles of the countenance, from clonic spasm, collectively demonstrate the influence of the alkali on the brain itself. This very day I had occasion to observe the existence of the cerebral influence of strychnia, in a patient affected with hysteric-paraplegia, for fifteen minutes *before* any indications of the specific spinal effects of the alkali supervened. The patient is, at present, taking forty-two drops of the tincture of strychnia every six hours. She has been under a course of the medicine during seven weeks, beginning, originally, with thirty drops, and I am happy to say that she has derived the *most striking advantage* from its cautious and persevering use, far surpassing, in a very obstinate and perplexing case, *the effect of every other varied method of treatment.*" The more immediate object in my view, when citing this case, is the perfect illustration it affords of the absolute influence of strychnia on the brain, as well as on the spinal marrow; for the former has uniformly preceded, and has, sometimes, even not been succeeded by the specific manifestations of the latter species of influence.

With regard to the mode of employing strychnia as a medicine, Dr. Bardsley says—"In the *internal* administration of strychnia, I have mostly commenced with one-sixth of a grain twice daily, and this proportion has been gradually increased to half a grain or a grain at the same intervals; but half a grain of the alkali, taken three times in the day, has generally produced sufficiently energetic effects upon the system. In the *external* use of the alkali, I have been in the habit of directing one-fourth of a grain to be sprinkled on the blistered surface, night and morning, and the quantity has been increased to half a grain, a grain, a grain and a half, and two grains, applied twice in the day."

Dr. T. Green on the Treatment of Syphilis without Mercury.

This paper is chiefly a digest of the remarks of some of the ablest advocates for the non-mercurial treatment of syphilis. We could have wished the author had given us more of the details of his own experience; but as it is, the paper is valuable as a summary of the best information that can at present be collected on the subject. The following protest against the supposed specific virtues of mercury in venereal, is worth giving at length.

"While all the facts I have now mentioned prove that syphilis can be cured without mercury, I do not, for a moment, contend that its use should be abandoned altogether in the treatment of venereal complaints. Mercury should be given in these complaints, with the same intention as if precisely similar symptoms occurred from any other cause. The well-known power of mercury, when used cautiously and moderately, to improve the general health, to remove chronic inflammations, and diseases of an indolent kind, indicates its use, in some few cases, for the symptoms of syphilis, as if they occurred from any other disease, without reference to any *specific character.* While I admit its use in some cases of syphilis, not for a moment *as a specific*, but on the general principles above stated, I contend that the cases requiring it are but few; a very large proportion of venereal diseases can be as well, if not better, cured without mercury, and *certainly more safely*, because the risk is not incurred of producing the truly melancholy results, which, in some constitutions, follow the treatment of syphilis with this mineral. I think its use, in primary symptoms, should be given up altogether, at least until there appears some indication for its employment. It cannot now

be maintained that a chancre is cured one day sooner where mercury is given, than where it is not; as far as any comparison of evidence goes, it would appear that these sores are sooner cured where mercury is not given. If this be true, we gain nothing by using this remedy, and incur the risk of inflicting a severe injury on the patient, as every practical surgeon knows well, that chancres do, sometimes, assume a gangrenous and phagedenic character under the mercurial irritation, occasionally ending in the loss of a portion, or the whole, of the penis, before the destructive processes can be arrested. The only intention with which mercury can be given, in this stage of the disease, is to prevent the introduction of the venereal poison into the system. But, has mercury this preventive influence? Unfortunately, the history of the venereal disease, under the most full and efficient mercurial treatment, proves that it has no such power. It is too well known, that if mercury succeeds once in preventing secondary symptoms, it fails altogether, in too many instances, to allow us to place reliance on it as a preventive of constitutional symptoms. It is certain that secondary symptoms occur whether mercury be given or not; but whether they follow more frequently in the one case or the other, is, as yet, entirely undecided."

In iritis alone, when it occurs as a secondary symptom, is mercury indispensable,—not, however, as the author takes care to add, as an antidote to any venereal virus, but to arrest the effects of inflammation in the eye; and, says Dr. Green, in conclusion—"While I have endeavoured to prove that mercury is not a *specific* for syphilis, I admit, to the fullest extent, its value in the treatment of other diseases. Those only who have tried this powerful remedy in inflammations of the head, chest, or abdomen, in iritis, in chronic enlargements of the testicle, &c., or as a discutient, wherever thickening or induration is to be removed, can be fully aware of its value. I think the better we become acquainted with the effects of mercury, the more shall we resort to it in other complaints, the less so in venereal diseases."

In an elaborate paper *On Chronic Peritoneal Inflammation, and its Treatment*, by Mr. E. Thomson, of Whitehaven, we have the opinions of all the most eminent ancient and modern authorities on the complaint; together with the experience of the author set forth in a series of cases, tending to prove the superior efficacy of mercury as a remedy for it. Mr. T. says—

"I have thus presented to the society a variety of cases, in proof of the peculiar effect of mercury in an affection allowed to be of the most dangerous character. The cases have been selected from amongst a great number, to elucidate particular views advanced in the remarks. That there is no remedy yet known equal to mercury, in the cure of this disease, I think must be admitted; and as it may be employed in all stages of the affection, without the slightest danger, it is one deserving of farther confidence than it has yet received. Respecting the exhibition of mercury, I can state, from the extensive employment of it, that I have never witnessed any affection of the bones, &c. produced by it, unless the system had been affected with the venereal disease previously; and I cannot resist, in this place, offering the meed of praise to Dr. Musgrave, for being the promulgator of so valuable a fact."

Case of Lithotomy by the Rectum; by Mr. Dawson, Surgeon to the Liverpool Infirmary, &c.

This case we shall lay before the reader in a very slightly abridged form.

"Robert Cox, nearly three years and a half old, of diminutive form and sickly aspect, was admitted into the Liverpool Infirmary in January 1832.

"The history of this child's sufferings, during the past six months, was detailed by his father; and it was sufficiently distressing to lead to the belief of the presence of a stone in the bladder. After a few days of repose, the sound was passed, but no calculus could then be detected. The urethra was found to be short and very narrow, admitting with difficulty the smallest instrument. The bony outlet of the pelvis was deformed; the space left between the conjoined pubie and ischiatic branches being narrowed to a degree I had never before witnessed; this space, by admeasurement, not exceeding one-third of an inch; the course of these branches, to within one-quarter of an inch in front of the anus, was nearly parallel; from this point into the tuberosities their divergence was abrupt, leaving ample space for the anus itself.

"Rigors, nausea, and complete prostration of strength, were the immediate result of this search, as, indeed, they were of every subsequent one. It is scarcely requisite to state, that these examinations were invariably conducted with all due

care and gentleness, or that sufficiently protracted intervals of repose were always allowed for the purpose.

"The act of passing his urine was succeeded by a state of weakness bordering on syncope, by the eversion of the lining membrane of the rectum, and by the escape of its contents.

"After remaining in the infirmary for a few weeks, during which period the search for a foreign body was renewed, but in vain, he was sent home to his parents, in the hope that, by their soothing attentions, his general health, which was by no means in a satisfactory state, might be improved, and in the expectation, also, that a little further delay might unfold, and thence enable me to detect, some tangible, or, at least, some intelligible, cause for his hitherto unmitigated sufferings. On his return, however, in five months afterwards, he was in no respect improved.

"His mother had entirely neglected him; she had allowed the anus to remain in a state of prolapse nearly all this time, so that the exposed mucous membrane had acquired a deep purple tint, was beset with excoriations and ulcerations, and presented, altogether, a very menacing appearance.

"Some pause was necessary, in order to bring the part into a condition to bear any future exploration.

"Taking advantage of a tranquil state of the rectum, I passed the finger, when its point was abruptly met, about half an inch within (or beyond) the fibres of the sphincter ani, by a bulging downwards, in a pouch-like form, of the upper wall of the rectum, through which membrane could be felt, but not very distinctly, the outline of a solid, and hence, probably, a foreign body, of about the size of a large Spanish olive, which appeared to be firmly seated in its novel situation.

"The bladder was forthwith sounded, when the instrument struck against a stone. The foreign body sustained by, and felt through, the medium of the anterior wall of the rectum, was hence satisfactorily identified with the calculus just detected in the bladder. Here, then, was an instance of sacculated stone. It is useless to inquire whether the pouch was a congenital or an accidental formation. One thing seemed to be certain, viz. that owing to the extreme narrowness of the bony outlet, the calculus was inaccessible by the usual lateral operation; indeed, I feared, at one time, it might be so by any other route.

"By the division, in a semi-lunar form, of the structures lying in front of the anus —that is, with the concavity of the incision looking downwards, and by a corresponding bilateral section of the imperfectly formed and compressed prostate gland, I might be enabled to approach the stone, but it would be almost impossible to extract it, without inflicting a degree of injury on that gland, and on its important connexions, that appeared to me to be unwarrantable. And then, with the division upwards (according to M. Sanson's method), of the fibres of the sphincter ani, of a portion of the anterior wall of the rectum, of the prostate gland, and of the neck of the bladder itself, I feared there must be associated the hazard, not only of forming a permanent communication between the bladder and the rectum, but also that of a loss of power in the sphincter ani; and, lastly, of dangerous infiltration of urine.

"It was manifest that, in approaching the concretion by this route, nothing need be divided but the anterior wall of the rectum, along with the lower fundus of the bladder lying in contact with it: the division of these investments might be made exactly in the median line, and to a limited extent, so as to leave, untouched, the sphincter ani in front; the undeveloped vesiculæ, and the terminations of the vasa deferentia on each side; and, I did venture to hope, the important fold of the peritoneum, should it also happen to form a part of the posterior face of the sac. The misery resulting from a permanent communication between the two outlets, seemed, in every way, unavoidable. But the grave anatomical objection remained, viz. how near to the base of the prostate gland might the reflected fold of the peritoneum advance? since, on the possibility, or otherwise, of its avoidance, the safety or danger of my patient depended.

"My colleagues, indeed, with their wonted kindness, endeavoured to assure me that my apprehensions of danger, from this quarter, would prove to be groundless; it was a possible contingency, certainly, that, in the event of this important fold of the peritoneum being dragged down so far as to be implicated in the formation of the posterior boundary wall of the sac, still all communication with the abdominal cavity might be closed, by the adhesion of its contiguous surfaces; or by the interposition of plastic lymph, so that, should its division be unavoidable, the danger of opening a way into the serous membrane might not be the result: and, lastly, I was repeatedly reminded, that children bear these things wonderfully well. I determined, therefore, to operate, whenever he should be brought into a state of health that would justify such a procedure: his present weak and irritable condition, however, forbade any immediate operative measures.

"The agony he suffered in passing his

urine, was estimated by the cold sweat, and by the paleness of countenance that always accompanied this act, which might be termed the "paroxysm of expulsion." He was not very likely to recover his health under these distressing circumstances, and every form of narcotic remedy was unsparingly, but unavailingly, administered. At length, a very simple remedy was found to answer, viz. the introduction, at stated intervals, of an elastic gum catheter, which had been prepared so as to retain a permanent curve, without the wire, and which was always thus used. His nurse became so expert in the employment of the instrument, that no other aid was ever required. From this moment pain and spasm ceased, as far as the expulsion of urine was concerned, and they never afterwards returned.

"My attention was now directed to prepare the rectum for the presence of a new and exasperating secretion; which was likely to be a work of time and of difficulty. I did not apprehend any serious inconvenience would result from the reflux of the urine, along with the fæces, into the bladder.

"Every variety of astringent, and of sedative injection, was used, but each was abandoned, in its turn, as worse than useless. At last, the injection of cold water alone was found to fulfil every indication; it appeared to soothe the mucous membrane better than any thing else. Twice every twenty-four hours the rectum was filled with it, and the decided comfort the child experienced was very soon made evident in his improved appearance. The bowel, in losing its irritability, entirely recovered its healthful tone and function, and, among other beneficial results, he got rid of a chronic diarrhœa, to which he had been liable.

"In the space of a few months he had become stout, florid, and cheerful, and was, in fact, in more robust health than he had ever before been. Weeks and months were allowed thus to pass away, the child seeming too happy to be disturbed, and I was in no haste to interfere with the comforts he was now enjoying. Indeed, could the same degree of attention have been afforded him in all future time, the question might have arisen, whether any operation at all was justifiable? But of this there was no chance, and the operation was undertaken in January, 1833.

"*The operation.*—He was bound by no ligatures. The nurse supported him on a pillow on her lap, in the usual position. A gum-lancet, having its anterior edge rounded and very keen, was laid flat on the finger, which, thus armed and oiled, was introduced through the anus, so as to reach a point a short distance beyond the recto-vesical pouch, when its edge was turned upwards, and a decided cut made, by drawing the instrument from behind, forwards, in the median line, through the walls of the pouch, and up to the stone, on the hard surface of which the edge of the lancet was distinctly felt to grate.

"I may mention, that the back part of the blade of the instrument was blunted, which allowed the point of the finger to project beyond it, and which was thus at liberty to direct, as well as to execute, the intended incision. After pausing a few moments, to allow time for the retraction of the divided structures, the finger was again passed, when the calculus was felt to be entangled among a mesh of elastic fibres; hence a second section became necessary, in effecting which the speculum was employed.

"The calculus was now found seated in the upper or vesical region of the sac, whence, having been displaced by the finger, it fell into the rectum, from among the valvular folds of which, after eluding the attempt once or twice, it was finally withdrawn by the help of Pellier's double silver wire, which served the purpose of scoop and lever. No blood was lost. The operation lasted nearly five minutes.

"The calculus was found to be much smaller than we had been led to expect. I have omitted to state that, during the operation, a quantity of hardened or dried mucus escaped, along with the fæces, through the sphincter ani; it resembled, in form, the peeled skin of a small apple, and it struck us at the moment that this substance might have been coiled round the lower moiety of the concretion, something after the manner of the cup of the acorn, and might, in this way, have added to its apparent magnitude.

"The child passed two liquid stools on the day of the operation, in each of which a clot of blood was found, equal in quantity to about two drachms. He passed a very tranquil night. During the four succeeding days the evacuations from the rectum were chiefly made up of urine. About this period of the after-treatment, 'Clot Bey' visited our infirmary; he expressed great interest about the child, but he gave me no hope whatever of the ultimate closure of the communication between the two outlets. A very slight degree of tenderness was detected, on pressure, at the lower part of the abdomen, on the second day after the operation; but this was very promptly and properly treated by the house-surgeon, Mr. Simon, and no more was heard of it afterwards. On each succeeding day two liquid motions were passed, the urinous admixture being always perceptible in them; the anus, however, remained free from the slightest appearance of excoriation.

"And now was experienced the benefit of the sphincter ani. The control exercised by the fibres of that muscle kept him clean and dry. It was only when the desire to empty the bowel came on with too much suddenness and urgency to allow time for the bed-pan to be placed under him, that his bed-clothes or body-linen were at all stained by the excretions.

"On the tenth day from that of the operation, to my surprise and delight, I found he had passed his urine, to the amount of four ounces, in a full stream through the penis; it was voided without pain, or straining, or spasm. After this no urine was ever detected in the evacuations.

"The calculus, twenty-four hours after extraction, and spontaneously dried, weighed just sixty grains. A few particles had been separated in the operation. It consisted, externally, of ammoniaco-magnesian-phosphate. The internal part, when seen, consisted of phosphate of lime."

There still remain some interesting papers in the volume well worthy of being noticed, and to which we must revert when we have a little more room —perhaps next week.

MEDICAL GAZETTE.

Saturday, June 28, 1834.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

EDUCATION IN THE PROVINCES.

ALTHOUGH the Memorial of the Bristol Surgeons may seem to have been completely answered by the several protests of their brethren of Birmingham, Winchester, and Bath, we yet may be permitted to add a few remarks to those already offered by ourselves; and this we rather feel called upon to do, in consequence of a letter in our present number from a correspondent who belongs to the Bristol party. It certainly argues in him and his colleagues a degree of fairness which is not a little honourable to them, especially as we have had occasion to state so decidedly our views, in a manner any thing but coincident with those emanating from Bristol.

The chief point concerned in this short discussion is, as our correspondent observes, now considerably narrowed. It no longer relates to the comparative excellence of the education which the medical student may obtain at the provincial schools; but whether something considerably advantageous may not be added to that education, by the rule that the provincial student, before he shall be held qualified for entering on practice, is obliged to visit the metropolis, and become an attendant at some of the London hospitals for at least six months; or, on the other hand, whether this "obnoxious" rule ought not to be dispensed with by the Royal College of Surgeons. Why the rule in question should be termed "obnoxious," by a certain number of provincial teachers in a particular locality, who are evidently disposed to look upon their own establishment as all in all, it is not very difficult to conceive; but it is exceedingly gratifying that such limited views by no means pervade all those eminent teachers in the provinces who have an equally good right to be animated by an *esprit de corps*, as their Bristol brethren. Nay, some of them have come forward, as our readers are aware, and have stated freely and firmly their dissent from the provincial perfectibility-doctrine; while others, we happen to know, fully participate in the same feeling,—approving highly the sentiments so well expressed by the surgeons of Winchester, Birmingham, and Bath, yet deeming it not necessary to come before the public medico-politically, in defence of what they consider to be almost a self-evident proposition. If the Bristol surgeons do not know this already, we take the present opportunity of informing them of it; and we strongly recommend them not to reckon on the acqui-

sition of support from many of those to whom they sent their circular, nor to imagine that silence gives consent where the parties merely do not think it necessary to reply.

We have ourselves, indeed, some hesitation in seriously resuming the subject, for much the same reason; for when we come to consider what is precisely the matter in dispute, we cannot be persuaded that it requires many words. It would be very easy, no doubt, to adopt a complex or wilfully-confused consideration of the subject, and to run on interminably upon it: we might trace the history of County Infirmaries, and the schools so recently got up in connexion with them; we might pay abundance of compliments to the projectors of those establishments; while we should not forget the obligations of most of these gentlemen to the London schools; and we might further shew the present high degree of importance of the schools in the provinces, and how much their ultimate perfection depends on their union with those in London. By such a circuitous route, we might contrive to be very voluminous and complimentary; but we are no lovers of verbiage; and we are, moreover, much inclined to think that on the present occasion the less we indulge in digressions the better. The question simply is, whether, with all the advantages that provincial education can afford, much might not be gained by the regulation that the student should resort to the metropolis for his final discipline?—whether, in fact, it is not desirable that the young medical man, before he settles down to practise, should be obliged to visit London, and see how professional matters are conducted here?

Now, the very reasons which are advanced by the localists against this arrangement, tell, as it seems to us, potently in its favour; for they savour strongly (without offence be it spoken) of the influence of provincial and exclusive habits. Who but those who think there is perfectibility, if not absolute perfection, in their own necessarily limited sphere, would endeavour to maintain that material benefit would not arise, in many respects, from entering as largely as possible into the world, and from using the metropolis as the most desirable, as it is decidedly the most practicable, resource for such a purpose? The gentleman who writes from Bristol in our present number, seems, in some measure, aware of this, but, curiously enough, he advocates foreign capitals in preference to London; thus involving himself, as it would seem, in no small self-contradiction. He admits that there is room for some accession to be made to the stock of information laid in at provincial schools; but he thinks that the student must either go abroad for that accession or not stir from home; if he cannot afford to visit foreign countries, he should be content with what he can acquire in the provinces; but, at all events, to London he ought not to go—because it is needless; for whatever is known in London immediately becomes known in the provinces, through the press and the facilities of personal communication and correspondence! We are most ready to acknowledge that the greatest facilities are afforded for the transmission of information, professional and otherwise, in this country; but who does not allow that information, of a professional or practical nature, obtained in such a way, has but a secondary efficacy—*segnius irritat animos*—while that procured by actual personal intercourse—at a period of life, too, when so much depends on vivid external impressions—is possessed of a much higher and more enduring value? How much is what we have been only in the habit of learning by our ears, or from our books, enhanced by personal ac-

quaintance with the eminent individuals and the scenes described! how many errors are removed!—how many new ideas gained! And, if foreign scenes are inaccessible, as they must always be to the great majority of young medical men, with what show of reason or justice is London proscribed—admitted, as it is, to be the grand centre of the intellectual wealth of the empire? Indeed, Mr. Fripp himself tacitly admits it, when he maintains that all the new improvements are now nearly simultaneous here and in the country parts, owing to the rapidity and facility of our means of transmitting intelligence. Yet though he thus concedes to us priority of information, and an earlier acquaintance with improvements in practice, he is opposed to the idea of the students coming closer to the fountain head!

Had the College in London prescribed a foreign course—as Mr. Fripp hints that they might more prudently have done—it is easy to conceive what an outcry would have been raised against them: but, in our judgment, they adopted a wiser course, in enjoining to the provincial student a residence of a certain duration in the metropolis, as an alternative: and surely, in limiting the period of that residence to six months, as reasonable an arrangement was effected as could with moderation be desired. Some standard was necessary—some injunction. It could not be left to the discretion of the youthful aspirants themselves, nor, perhaps, to the matured wisdom of their provincial instructors; so they took what we fancy the generality of impartial persons will agree with us in considering the most prudent course. We observe that the opponents of the College, on this head, are fond of laying stress on the term *walking* the hospitals, and the "farce" of London hospital attendance, as Mr. Fripp calls it; but they gain nothing by this sort of misrepresentation. No medical man requires to be informed that there is no such regulation enjoined by the College of Surgeons as that of "walking" the hospitals, in its degraded sense. The College requires hospital attendance, and presumes that the spirit of its regulation shall be honestly complied with; and objectors only debase themselves by appealing to the abuse of a wholesome measure as an argument against its utility.

The objections of Mr. Fripp and his colleagues to the arrangement in question, on the score of its *expense*, are so happily dealt with by the surgeons of the Bath United Hospital, that we do not conceive it necessary to touch again on that point, the more especially as we have already given our opinion upon it. But we must not omit to notice another part of our correspondent's letter. From our remarks on the desirableness of a *variety* of medical instruction, Mr. Fripp has contrived to draw rather an ingenious inference. He says, that, consistently with our position, the London pupil ought to be sent to glean information at the provincial hospitals. If Mr. F. is serious, we can only say, that he has oddly misunderstood us, in supposing that we could for a moment recommend other than a variety of *equally good*, if not *superior*, instruction; and nobody, we will add, but a thorough provincial, would pretend that London has not advantages of various kinds, which the provinces can never afford. This, however, is not the only fault we have to find with Mr. Fripp's mode of sustaining his argument: he talks of the crowds and jostling in our large hospitals—of the inferior appointments in our smaller ones; and, in short, it is difficult to say what he approves of in our metropolitan arrangements. This may seem to some people a sturdy mode of maintaining a position; but it is not quite graceful, we think, if it borders on want of information, or want of candour.

The "jostling" argument, for example, of Mr. Fripp, seems to involve no small mistake; for those who are acquainted with our hospitals know, that the number and large size of those establishments, and the distribution of the pupils over so wide a field, prevents any thing like that "jostling," which Mr. F. so fancifully figures to himself. Nor does he seem to be better informed with regard to the grievous irregularities and misconduct which he attributes to our metropolitan hospital pupils. It is no doubt, to a great extent, a question of testimony; but notwithstanding our correspondent's serious insinuations, we have very good reason to believe that the medical pupils of London are as well conducted as any others in the world—those even of Bristol not excepted.

With the observations of our correspondent touching the propriety of securing a good solid primary education, we entirely agree, as we have already expressed ourselves favourable to the arrangements of the Winchester surgeons, who propose that the extent and value of the student's provincial acquirements should be ascertained by an examination at the outset of his career in the metropolis. But this subject need not at present delay us. We hasten to conclude, by observing that while we allow that numerous improvements have yet to be made in our system of medical education, and that, among other parts of it, the mode of hospital attendance is susceptible of amelioration in perhaps not a few respects, we cannot but in the strongest manner set our faces against the *stagnation of exclusively local education*, advocated by particular parties; nor can we forbear from urging our Bristol friends to reconsider the request they have, in our opinion, so immaturely made in their Memorial: for, as it seems to us,—and we doubt not to all who impartially consider the subject,—in their anxiety to "repeal the union," they shew a most strange and unnatural willingness to throw away the fairest pearl in their crown—to abandon one of the best sources of their professional fame. We cannot, we assure them, contemplate without pity their eagerness thus to cut off all communication between their pupils and those of London; forgetting totally that in achieving this mock independence, they would deprive their young men not only of those positive professional advantages, but the moral ones also, which London, above all other places, so pre-eminently affords;—we allude more particularly to that certain cure for vanity and arrogance, which is generally effected in the conceited student by a short residence here. As to the intellectual riches of the capital, and the several opportunities which are here presented for improving and exalting the faculties of aspirants, it were trite in us, and now tedious, further to dwell upon the point.

DEATH OF SIR GILBERT BLANE.

We regret to announce the decease of this distinguished veteran, which took place at his house in Sackville-Street, on the morning of the 26th. Sir Gilbert was in his 85th year.

ANSWER OF THE SURGEONS OF THE BATH UNITED HOSPITAL TO THE SURGEONS OF THE BRISTOL INFIRMARY.

We, the surgeons of the Bath United Hospital, received, some time ago, from the surgeons of the Bristol Infirmary, a copy of their Letter to the Chairman and Parliamentary Committee on Medical Education, recommending an alteration in the law of the Royal College of Surgeons in London, requiring students to attend a metropolitan hospital before they be qualified to practise.

We did not, at any time, entertain the same opinions on this subject as the surgeons of the Bristol Infirmary, but as there was no urgent necessity for replying to their statement, we were unwilling to express our dissent from gentlemen with whom we are personally acquainted, and for whom we feel great respect. It appears, however, that the letter of the Bristol surgeons has been communicated to the surgeons of other provincial hospitals, and that the answers of some of them have been published; we cannot, therefore, longer refrain from making known our sentiments on the propriety of permitting students to become practitioners, without any attendance of a London hospital, lest our silence should be construed into an approval of the system advocated by the surgeons of the Bristol Infirmary.

We have received from the surgeons of the Birmingham Hospital a copy of their answer to the surgeons of the Bristol Infirmary. This document so ably expresses the opinions which we have formed on this subject, that we need only state that we fully concur with the surgeons of the Birmingham Hospital, that the provincial medical schools have been and are of great value to students, but that they do not embrace that complete system of professional education, and cannot possess those opportunities of variety in practice, which would justify the College of Surgeons in dispensing with a residence in the metropolis.

The objection to an attendance at a London Hospital, on account of its expense, we deem utterly valueless; as we cannot admit that economy ought to be put into competition with the advantage which the public would derive from efficient practitioners; and we do not consider that the respectability of the profession would be promoted by increased facilities of entering it.

We are also of opinion that the College of Surgeons have shewn sufficient liberality to the provincial schools, and that the privileges which these schools have acquired from the Apothecaries' Company are highly injurious to the College of Surgeons, to the profession at large, and to the public.

(Signed) GEORGE NORMAN,
JOHN S. SODEN,
R. W. BROWN,
Surgeons to the Bath United Hospital.

Bath, June 7, 1834.

To RICHARD SMITH, Esq.;
WM. HETLING, Esq.;
R. LOWE, Esq.;
H. DANIEL, Esq.; and
N. SMITH, Esq.
Surgeons to the Bristol Infirmary.

SURGICAL REPORTS
FROM THE
LONDON DISPENSARY.

By R. R. ROBINSON, Esq.
Surgeon to that Institution.

CASE I.—*Diffuse Popliteal Aneurism cured by Ligature—Double Pneumonia.*

WILLIAM WARREN, æt. 36, a carman, admitted November 6th, 1832. Perceived flying pains in the joints three weeks ago, which ultimately settled about the left knee; a bandage was applied, without benefit. The leg swelled, and confined him to bed. The whole left limb, from the trochanter to the toes, is enormously swollen, nearly double the size of the other; integuments generally white, but about the knees, and in the ham especially, they are red; the whole thigh, particularly the lower part, where there is a strong pulsation, very tense and hard; below the knee the swelling is not so tight, though considerable; the ankle œdematous; little or no pain, except on motion; face flushed; lips dry; frequent rigors; some oppression at the chest, and cough, the last few days; mind wanders in the night, when he is generally restless; thirst; pulse at wrist frequent, vibratory, and rather sharp; slight *bruit de soufflet* in cardiac region, especially towards the left side; rhoncus crepitans throughout the anterior part of right lung; respiration slightly mucous, but otherwise healthy, throughout left; urine high coloured—deposits a lateritious sediment; skin perspiring; tongue clean and moist; bowels open. Has been taking colchicum. V. S. ad ℥xx. Slight syncope.

Cal. gr. v.; Opii, gr. i. statim dimid. nocte maneque repetanda; Mist. Cath. Ant. 2 die; Lot. Plumb. tepida.

November 7th.—Crassamentum buffy, but flat; some sleep; breathing rather short; frequent, dry cough; limb free from pain; foot more swelled, and œdematous; circumference of lower part of left thigh twenty-two inches; temperature 95° F.; pulse less vibratory, easily compressed, 84; two motions; skin hot. His poverty compels him to go into the London Hospital.

8th.—Was admitted under Mr. Scott. The limb was placed on an inclined plane, and cal. gr. i., pulv. dov. gr. iij., ordered every hour. Some sleep; feels easier; œdema greatly diminished; tumor much the same; pulsation evident to the touch. A distinct thrill, somewhat bellows-

like, when the ear is applied over the lower part of the rectus femoris.—Perstet.

10th. — Tumor harder, less pulsatile; coagula appear to be forming; more prominence over the internal condyle, measurement the same; pneumonia in lower part of the left lung; crepitation continues throughout the right.

Vesic. Sterno; P. Ipecac. gr. iss. n. et m.; Mist. Salin. c. Tr. Scillæ 3 die.

16th.—Leg in the same state. Was going on remarkably well until two days back, when, in being moved up stairs, he caught cold and some fever, which have abated under salines and antimony; measurement of the limb rather less; breathing free; cough nearly gone; pulse less vibratory; manner hurried; tendency to floetitation.

19th.—Feeling a sudden snap in the ham this afternoon, about three, with great pain, and the tumor becoming suddenly tense, the femoral artery was at four o'clock secured by double ligature by Mr. Scott, at the usual spot, on the inner edge of the sartorius. The operation was well borne; all pulsation in the tumor stopped; the tumor was less tense. He felt relief, and could move the limb better.

28th. — Fever returned, and copious diarrhœa occurred; but these have been checked by salines, antimony, and laudanum. Cough and pulmonary symptoms have subsided; limb, by measurement, is three inches smaller than before; quite warm; pulse less vibratory.

December 1st.—General health much improved; cough gone; appetite good; skin over the tumor more flaccid; tumor less; toe everted; limb immoveable.

6th.—Stronger; ligature came away this morning; more pain in the tumor (which is smaller), since a thunder-storm, which occurred on the 2d instant.

26th.—Tumor greatly diminished; foot still everted. Can raise the limb himself; general health good.

February 13th, 1833.—Stronger; circumference of the limb only seventeen inches. Can stand, but cannot walk.

March 2d.—Swelling still diminished. Is up; walks about the ward with crutches.

June 5th, 1834.—Upon inquiring after this patient to-day, I was informed that he slowly recovered the use of his limb, and resumed his work; but that on the 16th of March he complained of headache and inability to lie down; that on the 23d of March it was with difficulty he could reach his home, from extreme dyspnœa; and that on the following morning he died; his face becoming livid, and discharging from his mouth a little thick yellow fluid.

CASE II.—*Diffuse Femoral Aneurism—Fractured Neck of the Thigh-bone (?)—Hydrocele—Diseased Bladder and Lungs.*

Robert Benson, æt. 80, admitted May 7th; a hydrocele on the left side. For many years has passed gravel, and had difficult micturition; every year latterly the subject of diseased lungs. Was knocked down in the street eleven weeks ago; confined to bed since; the injury is seated on the left side. The neck of the left femur seems to have been fractured, as there is a great roundness in the situation of the trochanter major; he is himself unable to flex the thigh; by others it can only be half bent upon the body. The left thigh cannot be rotated outwards; it is full half an inch shorter than the other, and the toes are turned outwards. There is redness, heat, and tension of the integuments covering the trochanter, which extend over the lower part of the abdomen, and also over the anterior part of the thigh, where there is an irregular, hard, pulsating tumor, extending from the lower part of the abdominal muscles as far as the apex of the triangle formed by the sartorius and triceps. The pulsation in the tumor has only existed, he says, four days; pain in the course of the internal cutaneous nerve of the thigh; breathing exceedingly short and rattling; a troublesome cough; face congested; great prostration of strength. The integuments of the back are beginning to give way, from pressure. Pulse rather hard, *regular;* tongue clean and moist; skin hot.

P. P. ℈j. statim; Mist. Salin. Ant. ter die; Lot. Plumb.

May 9th.—Cough troublesome; breathing rattling, chiefly from inability (from his position and weakness) to expectorate the mucus; respiratory murmur loud throughout the chest; aneurismal tumor much the same; pulsations not quite so strong, 75 in a minute, and *intermittent*, corresponding in number and intermission with those of the radial artery; no numbness or coldness experienced in the foot, which is the same in temperature as the other. On applying the stethoscope to the tumor, there is a noise more like a rough rhonchus mucosus, than either a bellows or a sawing sound.

Rep. Lot. et Mist. Linctus; Pil. Scillæ, n. et m.

12th.—Tumor has extended above Poupart's ligament; is more elevated, soft in the centre, and acuminated; skin covering it very red, thin, and shining; pulsations as before. Slight œdema about both ankles; pain at the inside of the knee very acute at times from pressure on the cutaneous nerves; is relieved by standing up;

breathing and eough in the same state; pulse intermittent, feeble; two motions; tongue dry and brown; very restless.

Tr. Opii, gtt. xl. statim, et rep. gtt. xx. si opus sit.

13th.—Seen by my friend, Mr. Luke. Tumor rather on the inerease. As the only possible ehanee of saving his life, it was proposed to tie the artery, but he would on no aeeount submit; less pain in the knee; pulse 84, intermittent; less cough.

18th.—Tumor decidedly larger, and more pulsatile; a feeling of whizzing under the hand; still the rough mueous sound to the ear; sleeps mueh, though he did not take opiates, as ordered.

20th.—Tumor very tense, larger, and more diffuse; skin eovering its upper part very mueh eongested; limbs eold; eountenanee mueh altered; sleeps mueh.

25th.—Alive, but mueh weaker. His wife gave up the tieket to-day, as she is sure all his debility is owing to the medicine he has taken; that his ease has been mistaken; that he has an abseess which ought to have been poultieed; and that an operation was only proposed by way of experiment. She further stated, she would take eare he was not touehed, dead or alive, for she would keep him six months in quiek lime. Upon my requesting to know the result of the ease, she said she would on no aeeount inform me, fearing, I believe, that I should have him taken up, as to her knowledge (though not to mine) some resurreetion-men resided near her.

Case III.—*Aneurism of the Abdominal Aorta, communicating by two openings with the inferior cava—General Dropsy—Hæmaturia.*

Thomas Wallis, æt. 42, transferred from Dr. A. Frampton, July 7th, reeeived a hurt in the baek two years and a half ago, whieh he felt for a few days, and then got rid of; and though he has oeeasionally felt pain there since, he has done nothing for it. Five weeks ago he got wet through, and sat in wet elothes; felt chilly immediately, and soon went to bed. Not long after his legs began to swell, and have eontinued so ever sinee. Countenanee very pale, almost chlorotie; serotum and thighs enormously swelled; skin perfectly white, very tense, and pits deeply on pressure; abdomen slightly tender, and rather full, as if indistinetly eontaining fluid; veins about abdominal integuments numerous, and varieose; pulse quiek, vibratory, and slightly hæmorrhagie; tongue elean and moist; respiratory murmur audible and healthy throughout the ehest; heart's aetion regular, not very strong: rather better than a fortnight ago his urine was bloody, and has so eontinued. Bowels have been freely purged by calomel and elaterium; his strength has been supported by light nourishment—deeoetion of bark, and aeid. eucurb. eruentæ lumbis ad ℥x. After the seeond eup was applied, he felt (to use his own words) "as if a load was removed from the kidneys."

Calom. gr. iij.; Elateri, gr. j. alt. auroris. Quin. Sulph. gr. i.; Pulv. Digitalis, gr. $\frac{1}{6}$, ter die. Lot. Liq. Calcis e. Spiritu seroto.

July 9th.—Easier; slept better; legs not so tense; several motions, from the pills; has passed a pint and a half of ehocolate-coloured urine, with several clots of blood (whieh, he says, passed in clots), and sinee that has voided ℥ij. of high-eoloured urine, free from blood.

Repetantur omnia.

12th.—Urine more free; no blood sinee last report; stronger; legs less swelled; serotum deeidedly less; pulse 98; skin hot; two motions daily.

30th.—Legs and abdomen more swelled; veins about the latter very large, tortuous, and purple; faee puffy, and slightly œdematous; breathing short; eough; cannot lie on his sides as well as he eould; slight crepitation about the lower part of the lungs; urine copious, elear, straw-coloured, does not eoagulate by heat; skin of legs disposed to inflame at parts.

Perstet.

August 11th.—Integuments of legs rather tighter; they have burst in one or two plaees, and diseharged serum eopiously. Less dyspnœa and eough. Aeupuneturation.

Repetantur medieamenta.

28th.—Stronger; eopious discharge of serum from the punetures; breathing freer; bowels open; urine free. Punetures repeated; bandages to the legs.

September 14th.—Faee rather more œdematous; legs have diminished greatly; a sloughy sore upon eaeh shin; urine very high eoloured—deposits a red sediment on standing.

Oetober 31st.—Continuing mueh the same, and not being able to support himself, he went into the London Hospital. Calling aeeidentally at his residenee today, I learnt that he eontinued to mend—that the swelling diminished—that his breathing was better—that he was mueh more eheerful, and appeared almost eonvaleseent—when he died suddenly, two days ago. I obtained permission to examine the body.

Dissection.—Œdema entirely gone. Lungs mottled, eongested, unadherent. A pint of fluid in the ehest. Heart large; all its cavities, but partieularly the right auriele and left ventriele, loaded with dark blood, partly fluid, but chiefly eoagulated, with some fibrinous *polypi*. Aortic valves

thickened; steatomatous deposit under the inner coat, but no dilatation of the thoracic aorta. Liver of usual size, soft in structure, nutmeg-like in appearance. Stomach, intestines, &c. healthy. Very little fluid in the abdomen. Upon holding up the convolutions of the intestines, a tuberculated, white, circumscribed tumor, was visible, which proved to be the descending aorta considerably dilated, and having the lumbar glands impacted in its substance—some of them enlarged, but none of them hard. The dilatation began a little below the renal arteries, and extended nearly to the bifurcation. The dilatation formed a circumscribed tumor, as large as the fist, which was strongly adherent to the lateral parts of the spine: upon separating them, the posterior part of the aorta was found to have been entirely destroyed, as was also the anterior common ligament of the spine. The bodies of the vertebræ in this situation, scabrous; intervertebral cartilages unaffected. The spine, therefore, was the posterior boundary of the aneurismal sac, which contained several recent clots, none in layers, and some fluid blood. The interior of the sac was rough, from ossific deposit under the inner coat, and had two distinct communications with the inferior cava.

Remarks.—These cases in some respects differ from the usual forms of aneurism.

The first is peculiar, from its being an aneurism of the diffuse kind, although not attributable to a blow or sprain, and from its being accompanied with many of the symptoms of rheumatism. I confess that I was at first inclined to think the disease rheumatic inflammation of the fascia of the thigh, with subfascial abscess; to which, from the tension of the fascia, the pulsations of the femero-popliteal artery had been propagated. I was the more inclined to this opinion, from having had a case of extensive abscess around the shoulder-joint, attended with evident pulsation, which I shall relate hereafter; by the flying pains which ultimately settled in the left knee; by the buffy appearance of the blood; the lateritious sediment in the urine, and other symptoms of fever; by the vibratory pulse, and by the *bruit de soufflet* in the cardiac region.

It is to be regretted that a post-mortem examination was not obtained, as it would have been desirable to have investigated the extent and situation of the rupture in the artery, as well as to have known the condition of the neighbouring vessels. The femoral artery, where it was tied, appeared to be healthy.

From the almost sudden death, which occurred many months after the operation, and the dyspnœa he complained of a few days previously, it is not improbable that an aneurism about the arch of the aorta also existed.

The second case is peculiar, from the complication of maladies under which the patient laboured, and from his age; aneurism, as far as I have seen, not being common at his period of life. It is interesting, from the way in which it occurred. I have no doubt that the femoral artery (and probably many other arteries) was in a diseased and ossified state; therefore the coats gave way from the fall more readily than otherwise they would have done, and aneurism of the diffuse kind was the result.

I much regret that I could not ascertain this and other points more accurately by dissection; but his wife was so insensible to all reasoning, that I could not even learn the result of the case, though he could not have existed long after I saw him for the last time,—much less could I obtain permission to examine him after death.

A more distressing case than the above cannot well offer itself to the surgeon; for, unless age and debility should prevent it, the patient must inevitably be lost by hæmorrhage. Left to itself, the tumor was daily on the increase, and must before long have burst: had the artery been secured higher up, (and I believe most surgeons would have felt compelled to have made such a proposition, when they saw, as we did, the tumor rapidly enlarging), the vessel would in all probability have been found there diseased; its coats would have been cut through by the ligature, before obliteration had been completed, and a hæmorrhage would have followed that would have been very soon fatal.

The third I believe to be a very rare case, as I do not remember any such in Corvisart, Baillie, Burns, Laennec, or Hope; nor have I perceived mention made of a communication between an artery and vein, except by puncture, in the works of Scarpa, Hodgson, Guthrie, or S. Cooper: there is no such preparation in the museum of the College of Surgeons; and the only analogous case that has come to my knowledge is one of aneurism of the arch of the aorta, opening into the superior cava, occurring at St. Bartholomew's Hospital.

It is curious that two openings should have been made in the vein, with only a small bridle between them; they are small and round, and appear either to have occurred in consequence of ulceration, or to have undergone reparation, if originally ruptures.

In looking over the symptoms, I do not see how it was possible to have detected this case during life, except by

auscultation; for the patient never complained of any beating in the abdomen. From the artery being throughout its calibre dilated, and not pouched, and from the communications which existed between the artery and vein, it is reasonable to suppose that neither pulsation, or tendency to form coagula, was considerable.

It is probable that some peculiar sound might have been perceived by the stethoscope; but as this appeared a case of *inflammatory dropsy*, it did not occur to me stethoscopically to examine the abdomen. The hæmaturia is attributable, no doubt, to the extreme congestion which must have existed in the renal vessels; it is curious, because the blood was discharged in the fluid form, and mixed with the urine, till a day after the cupping, when numerous coagula passed; after which it altogether disappeared.

It is also worthy of notice, that in this case, with so much apparent congestion about the kidneys, the urine should not have been albuminous.

The preparation, illustrating the communication between the artery and vein, is in my museum, and open to the inspection of any member of the profession.

LITERARY NOTICE.

In July will be published, a new edition of Mr. Wardrop's "Morbid Anatomy of the Human Eye." 2 vols. royal 8vo.

The price will be considerably reduced from that of the former edition, but the same regard will be paid to the fidelity and beauty of the colouring of the plates, the whole being executed under the superintendence of the author.

NEW MEDICAL WORKS.

A Practical Treatise on Medical Jurisprudence, with so much of Anatomy, Physiology, Pathology, and the Practice of Medicine and Surgery, as are essential to be known by Members of Parliament, Lawyers, Coroners, Magistrates, &c. and all the Laws relative to Medical Practitioners; with Explanatory Plates. By J. Chitty, Esq. Part I. royal 8vo. 21s. bds.

The Surgical and Descriptive Anatomy of the Bones, Ligaments, and Joints. By W. Thomas, Member of the Royal College of Surgeons. 12mo. 6s. bds.

New System of Organic Chemistry; translated from the French of Raspail, with Notes, &c. by Wm. Henderson, M.D. 8vo. 18s. bds.

Madame Boivin on the Diseases of the Uterus; from the French. By G. O. Heming, F.L.S. 14s. cloth. Plates to Ditto, 8vo. 12s. cloth.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *June* 24, 1834.

Age and Debility	30	Inflammation	24
Apoplexy	6	Bowels & Stomach	3
Asthma	6	Brain	2
Cancer	1	Lungs and Pleura	5
Childbirth	1	Insanity	3
Consumption	56	Liver, diseased	2
Convulsions	21	Locked Jaw	1
Croup	1	Measles	5
Dentition or Teething	7	Mortification	1
Diarrhœa	1	Paralysis	4
Dropsy	16	Scrofula	2
Dropsy on the Brain	10	Small-Pox	5
Epilepsy	1	Thrush	1
Fever	4	Tumor	1
Fever, Scarlet	8	Unknown Causes	4
Hæmorrhage	1		
Heart, diseased	2	Stillborn	11
Hooping-Cough	11		

Decrease of Burials, as compared with the preceding week . . . 204

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

June, 1834.	THERMOMETER.		BAROMETER.	
Thursday . 19	from 49	to 78	29·89	to 29·80
Friday . . 20	51	81	29·71	29·69
Saturday . 21	45	85	29·68	29·71
Sunday . . 22	57	75	29·79	29·98
Monday . . 23	49	76	30·23	30·27
Tuesday . . 24	46	75	30·29	Stat.
Wednesday 25	47	71	30·25	30·17

Prevailing wind, S.W.

Except the morning of the 22d and the 25th, generally cloudy: rain in the morning of the former day.

Rain fallen, ·3 of an inch.

CHARLES HENRY ADAMS.

NOTICE.

We cannot listen to the renewed request of M. de Moscati, to give insertion to the letter which we have already refused: yet we will not deny him a corner for the extract from the Lancet of last week, which he has sent us:—

"With regard to the questions which M. Moscati has put to us, we can have no possible motive for withholding from him correct replies. The letter signed "S. B. R." on the introductory lecture of Mr. Green, was received from Dr. Elliotson, at a period when we were unacquainted with M. Moscati. As to what M. Moscati has stated, relative to his attendance at the College of Physicians, we have ground for believing that some of his assertions are correct, but with regard to others we cannot vouch for their accuracy, as all the circumstances did not fall under our observation."

This is all we can or will do in this business.

W. WILSON, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JULY 5, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XXXIX.

PRETERNATURAL LABOUR.

BREECH PRESENTATIONS.

It is by no means an universal rule that the child should lie in any of the positions which have been described; for there is no part of the body or limbs that may not present. Those cases, then, in which any part of the fœtus offers itself except the head, have already been classed, after Dr. Denman, as preternatural labours; and by far the most frequent of these is the presentation of one or both nates.

Many speculative fancies have been indulged in, designed to account for the preponderance of head presentations; and gravitation has had most supporters. It was supposed that the placenta was invariably situated at the fundus uteri; and that, the fœtus being suspended by the funis umbilicalis, its head, which was the heaviest part, naturally inclined downwards; especially as in the younger embryo the umbilicus is comparatively so near to the pubes. This, however, cannot produce the influence ascribed to it; because, during at least the latter half of utero-gestation, the fœtus is not suspended by the funis, which indeed is too long to admit of such a possibility;—because the placenta is not always, nor indeed generally, implanted at the fundus uteri,—being sometimes situated upon the cervix, or over the mouth of the womb itself; in which latter case, at no period of pregnancy would the fœtus be suspended under the upright posture of the body;—and because the funis is sometimes found coiled around one of the fœtal limbs; which accidental position must influence the depending part, even if the embryo were actually suspended. These and other facts are most forcibly adduced by Dubois, in a late paper published in the Memoirs of the Academy of Medicine, to overturn the opinion that gravitation had any influence in producing the presentation of the head; and he has ascribed the general situation to an instinctive impulse implanted in the fœtus, which inclines it to take the most favourable position for its escape,—as the needle points mysteriously to the pole. But such a mode of reasoning and illustration cannot be considered either as argumentative or conclusive; it is, in fact, completely evading the question, after attempting to elucidate it; and the method he has taken can only be regarded as a cloak for human ignorance. It would, in my opinion, be much better not to endeavour to explain the secrets of nature, so deeply hidden, but to content ourselves with referring this also to a general, though not invariable law,—a part of the great system which shows the design, and exemplifies the harmony, that reigns throughout the whole works of Providence. M. Virey, indeed, has stated that in those pregnant animals of the multiparient kind which he has dissected, he always found in the horns of the uterus the snouts pointing to the vulva; that in a gravid viper which he opened, all the young, eight in number, were placed in the direction with their mouths towards the external parts; that, in the egg, the head is always directed to the large end, and that that end is extruded first; and that the same obtains with regard to the ova of fishes. We all know that the larvæ of insects escape with their head first,—that the chrysalis eats through its shell, and the caterpillar through its silky covering; and we see, therefore, one common law regulating the whole of nature's operations.

the whole of his observations, it may be inferred that this was only one of many experiments made in support of a truth or principle that had been, in a great degree, previously matured by dissection and deep reflection; for, in his Anatomical and Physiological Treatises on the Brain and Nerves, he points out the functions of each nerve separately, but previously states that the division or distinction of the nerves, by reason of their various respects, is wont to be manifold; to wit, as they are either soft or hard, singular or numerous, in their beginnings; or that they serve either to the faculty of sense *(quod sensui tantum)*, or motion *(aut motui)*, or both together *(simul utrique facultati.)* He says, the first is for the sense of smelling; the second, for the sense of seeing; the third and fourth, for motion; the fifth, for both sense and motion; the soft portion of the seventh, for the sense of hearing; the hard portion, rather for motion than sense, and also for pathetic motion; the eighth, for the involuntary functions of the parts to which it is distributed; the spinal accessory, for the involuntary functions of the arms, &c.; the ninth, for the motions of the tongue, requisite for the articulation of sounds, as the nerves of the fifth, spread into this member, serve for the distinguishing of the tastes, for the exercise of either faculty, viz. both motive and sensitive; the texture of the tongue being notedly fibrous, hath the virtue of a double organ; therefore, by the two nerves, besides the supplements of spirits, are carried on both the instincts of motion and the sensible impressions.

"But, in truth, this may be rather said—that all the shoots of the nervous and lesser branches remain distinct and singular amongst themselves, from the parts to which they are inserted, even to their beginning; so that a peculiar tract of the spirits, or way of passage, lies open from the brain and its medullary appendix, to every muscle and nervous part; for, in truth, although the nerves, according to their beginnings, may seem to arise from the greater trunks, yet it will easily appear, if you shall open the trunk and these branches, that in them many little nerves, only like hairs, for the sake of a better conduct, are collected together in the same bundle; yea, the coverings being separated, you may follow oftentimes the little nervulets, and these single, to the respective parts and membranes to which they are destinated."

For the history of the more particular functions of every nerve, the reader is referred to the work itself.

> "Oro miserere laborum
> Tantorum, miserere animæ non digna ferentis."

If the writings of physicians of the past century be examined, it will be found that the preceding doctrines were received as fundamental; that they then became neglected, and have since been revived and enlarged upon by the labours of our own distinguished countrymen, as well as by no less distinguished foreigners.

Van Swieten, in his commentary on Boerhaave's 1057th aphorism, says—"It is known from physiology that some nerves serve for the sense of feeling, and others for motion, which, though they are very different in their origin within the brain, yet being collected into the greater trunks of the nerves, are sent together to the different parts of the body; wherefore the functions of the motory nerves may be hindered, while the nerves for feeling remain quite unhurt, or at least not so much, and the contrary."

Other passages bearing on this doctrine may be seen in the commentaries on several subsequent aphorisms.

6, Tavistock-Square,
July 14, 1834.

PHYSIOLOGY OF THE VOICE.

MODE OF PRODUCING CERTAIN MUSICAL TONES.

To the Editor of the Medical Gazette.

Sir,

In the last number of the Gazette, Mr. Bishop has advanced some ingenious reasoning to disprove the correctness of Mr. Wheatstone's explanation of Mr. Richmond's musical performance, an account of which I gave in a former number of your journal.

Mr. Bishop admits that the bass sound is laryngeal, but supposes that the production of the higher notes in the performance "approximates most nearly to the common mode of whistling." It is evident, however, that if this were

the case, the higher notes might be produced, as they are in whistling, without an accompanying bass or fundamental sound. Now the contrary is the fact. Mr. Richmond *cannot* produce the higher notes but in conjunction with the drone.

Mr. Bishop further states, that "the higher notes [in the performance] are not necessarily harmonies [it should be harmonics] resulting from the fundamental notes; their relative positions in the scale being under the control of Mr. Richmond's taste." This remark, if true, would certainly subvert Mr. Wheatstone's hypothesis; but there is reason to doubt its accuracy. As far as it could be determined, on the occasion on which Mr. Wheatstone and myself witnessed Mr. Richmond's performance, the series of higher notes invariably bore a definite relation to the fundamental sound; each higher sound being in the relation of a multiple of the bass; and not otherwise under the control of Mr. Richmond's taste, than are the higher notes, in Mr. Eulenstein's performance on the Jew's harp, under *his* control.—I am, sir,

Your obedient servant,

HERBERT MAYO.

19, George-Street, Hanover-Square,
July 16, 1834.

OXIDE OF BISMUTH IN PULMONARY COMPLAINTS.

To the Editor of the Medical Gazette.

SIR,

I HAVE for years been much struck with the intimate connexion which subsists between the stomach and the lungs, and with the quick interchange of disordered function which takes place between those two important organs. I need not point out the strong analogy of the affections of the mesenteric glands with the disturbed actions of the alimentary organs, of which they form so essential a part. From long experience of the good effects of the oxide of bismuth, under certain conditions of the stomach, and from the above opinion, that that organ is much concerned in most derangements of the lungs, I have been induced to order oxide of bismuth as a remedy for pulmonary complaints; and the success in arresting the progress, if not of ultimately relieving many symptoms, of those affections, I think sufficiently justify me in calling the attention of my medical brethren, who have more frequent opportunities than I now have, to a subject of this, usually considered, intractable nature, and warrant my taking the liberty of giving you the trouble of this communication.

I have the honour to be, sir,

Your obedient servant,

GEORGE SMITH GIBBES.

Weymouth, July 10, 1834.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Illustrations of the Natural History of Worcestershire, with Information on the Statistics, Zoology, and Geology, of the County. By CHARLES HASTINGS, M.D.

THIS is, we believe, the first publication that has issued from the press under the sanction of the Worcestershire Natural History Society. The society itself is of a very recent date, being established since the suggestion for the forming of such scientific co-operative bodies was thrown out at the first meeting of the Provincial Medical Association. We have already taken occasion to express our decided approval of the principle of such unions; and we are glad to find the author of the work before us dwelling with earnestness on the advantages to be derived from them by the medical profession; while he does not forget to assume that credit for his professional brethren which they so richly deserve as cultivators of the natural sciences. "With that profession," says Dr. Hastings, "the inquiries proposed to be instituted have a very close connexion, since without a correct knowledge of the natural history of the locality in which the physician practises, he must be ignorant of many things which influence its mortality, and cannot discern what Hippocrates esteemed the greatest excellence in the art of physic—the constitution of the seasons, and the diseases which attend them." And again, in his

or both feet may present, or a foot and the breech together, or both feet and the breech, or a knee and a foot, or both feet. Thus you see what a great variety we may observe, even in this apparently simple order of preternatural cases.

It is evident that there will not be more difficulty when the knees present, than in a breech presentation—perhaps not so much; because the parts are expanded or opened more gradually, the body of the child forming more of a cone. But although it is, perhaps, not so painful a labour as when the legs are doubled up towards the abdomen, still it is more dangerous to the child, since there must be more pressure on the funis umbilicalis when the upper part of the body, or the head, is passing, in consequence of the parts not having been so completely opened as if the breech had previously passed double.

If the breech and a foot should present, which is by no means unusual, more space would be occupied, and more time would be generally taken up, than when the breech presented singly; but still the same action would go on, and the same effect be produced, provided the pelvis were sufficiently large. The foot would come external previously to the breech, the same turns would be effected, and the labour would most likely be completed naturally, without much assistance.

Conduct under breech presentation.—Let us now advert to the best mode of managing a breech presentation. In this case, a great deal more is required of the obstetrician than under a natural labour, both so far as the protection of the woman's parts is concerned, and also particularly with regard to the preservation of the child's life; for the infant is always placed in greater or less jeopardy from the pressure which must take place on the funis umbilicalis during the passage of the head. More care is requisite to prevent injury to the woman's structures; because, in natural labour, when the head is born, since that possesses the largest circumference of any portion of the fœtal body, the passages are generally sufficiently distended by it to permit the easy transit of the shoulders and breech. But when the breech comes first, being smaller in diameter than the shoulders, it only causes a partial dilatation; the shoulders pressing upon the parts subsequently, distend them still more, and at last the head, which is the largest body, has still farther to open them; so that we must continue our support to the perineum until the infant is entirely in the world. In natural labour, however, it is only necessary to protect these structures while the head, and perhaps the shoulders also, are making their escape.

The first duty, then, we have to perform, is to ascertain the presentation; and it is a matter of the greatest possible consequence that we should detect a breech case early in labour, lest we should confound it with the head, or particularly the shoulder; for there are many points of resemblance between the breech and both these parts; and we have already learned that a breech case requires comparatively little assistance; but that, under a shoulder presentation, the performance of an operation both difficult and dangerous becomes necessary to accomplish delivery.

No indication authorizing a supposition that the breech presents, can be learned from the mode in which the membranes protrude into the vagina, which is usually, as when the head offers, in the form of an egg. But the breech may be discriminated from the head and other parts, as soon as it can be felt by some marks both positive and negative; and we shall first give our attention to the negative points with reference to the head, reserving those connected with the shoulder for future consideration. The breech is not so round, nor so hard, nor so strongly ossified, as the head;—it is not divided into compartments by sutures and fontanelles; on the contrary, it possesses, as it were, a double circumference; it is more fleshy, softer to the finger, not so resistant, and more *cushiony;* and we may most probably detect the chink between the thighs, the organs of generation, and the anus. If we have fully ascertained the existence of these negative and positive marks, and especially if we have detected the organs of generation, male or female, and the anus, it is impossible that we can mistake the breech for the head.

These points I require to be determined previously to the rupture of the membranes; and our examination must be made with the greatest care, and in the interval of uterine contraction, lest we should break the membranes; for it is even of greater consequence, in the case we are considering, that we should preserve the watery cyst entire, than if it be a head presentation. When we first make an examination, if the nates be the most depending part, and we ascertain that there is not that characteristic feel which the head supplies, we may be in doubt as to whether the shoulder or breech be at the brim. If so, we should pass two fingers of the left hand, during the absence of pain, into the pelvis, up to the brim, within the os uteri; and it is seldom that we cannot, in this way, gain the information we require. Having, then, positively detected the

breech, there is no necessity for alarm; we are not to suppose that the woman will be endangered; we must not manifest, in our manner, either agitation or anxiety; and we must be particularly cautious not to let the patient hear "a cross-birth" whispered in her chamber, because she will certainly be more or less excited; and such a shock might be so suddenly impressed as to suspend labour, and retard it for a number of hours. We may, then, endeavour to evade her anxious question, *whether every thing is right*, by assuring her of her perfect safety.

At the same time, it is highly desirable that her friends should be informed that the case is one of the simplest kind of *cross-births;* that most probably no operation will be required, but that there is a great chance,—especially if it be a first labour,—that the child will not be born alive. If in this first examination we are quite satisfied that the breech presents, but not able to detect whether the abdomen of the child is situated backwards or forwards, it is neither necessary nor proper that we should be making constant examinations for the purpose of ascertaining this point. It is right that we should be a little more assiduous in our attention to the patient than if the head presents, but not so officious as to alarm her; and it is quite requisite that we should not absent ourselves from the house. We may occasionally make an examination in the absence of pain, watch the progress of the dilatation of the os uteri, and the descent of the membranes, but we must be most careful not to break them, although they should appear externally; waiting even then, within proper limits, for their spontaneous rupture. I would rather by far, under a breech presentation, that the parts were distended completely by the membranous bag, than that it broke before protruding externally. We have nothing to do, then, but to watch the progress of the labour, to keep up the patient's spirits, and to prevent the rupture of the membranes, if possible, until they have appeared somewhat without the labia.

The membranes having ruptured, and the breech fully occupying the pelvic cavity, we apply our hand, guarded by a napkin,—in the same way as when the head presents,—over the perineum, and support it until the breech and legs are in the world; and as soon as the funis has appeared externally, we must bring down a loop, in order to prevent its vessels from being stretched. You are acquainted with the arrangement of the umbilical arteries; that they run in a twisted direction around the vein, and consequently that a compression of their cavities, and a diminution in their calibre, would take place quite as easily from a distention of the cord, as from actual pressure being applied to it. We cannot prevent the compression which the funis must suffer between the child's head and the pelvic bones, but we can prevent tension, by bringing down a loop of the cord; and it is very possible, if we neglected this precaution, that, as the child's body is being expelled, such tension would take place as to impede the circulation, and destroy its life.

When the shoulders are about to pass, it is our duty to take care that they are offering themselves in that position most favourable for their exit; and if they be not, to turn one under the arch of the pubes, and the other into the hollow of the sacrum; and this is doubly requisite, not only for the easy transit of the arms themselves, but for the purpose of placing the head in the most favourable position for its passage through the brim. The arms being expelled, we are not to haul at the child, as if we were operating on a machine, but merely to direct the head into the most easy situation for its exit, taking advantage of the expulsive action of the uterus to aid our gentle endeavours. When the head is pressing on the outlet of the pelvis, the chin being within one ilium, and the occiput within the other, we may with great advantage facilitate the turn of the face into the hollow of the sacrum, by placing the right hand on the back of the child, the left on the abdomen; the two fingers of the right hand forming a crutch around the neck. We may then support the perineum with the left hand; as the head is passing, we may turn the nape of the neck up under the symphysis pubis, as on a pivot; bring the back towards the mons veneris; and thus assist the birth, not by drawing the child's head forcibly out, but merely receiving it as expelled by the action of the uterine and vaginal fibres.

The child being born, our duties are merely those appertaining to common labour; we must wipe its face, take care that it does not inhale any of the mucus about the parts, separate it as before described, dispose of it to the nurse or some other party, and then make an examination of the uterine tumor. In all cases of breech presentation, it is right that a warm bath, and other resuscitatory means should be in readiness, and close at hand, in order that the best chance should be afforded of restoring the child, provided animation be suspended.

It used to be the custom, and it is still practised by some, to make traction as soon as the breech is in the pelvis, and before the nates appear externally, by hooking the finger first in one groin, and then in the other, and to bring down the legs, so that the feet might pass externally as early

as possible. Dr. William Hunter at one time recommended this practice, and he was by no means a meddlesome obstetrician. The object of this practice was to save the patient pain. It was argued, why should we suffer the woman's structures to be so much distended by the doubled breech, when we have it in our power easily to relieve them of the tension, by bringing down the feet, and allowing them to be expanded more gradually; and it certainly was both a very plausible argument, as well as natural conclusion. But the result of this practice is to place the child's life in imminent hazard: as long as the legs are turned up towards the belly, so long the funis umbilicalis lies in a triangular groove, formed between the two thighs and the abdomen, entirely, or nearly, out of the way of pressure from the sides of the pelvis or the uterine parietes; so that its vessels can only be subjected to compression as the shoulders and head are passing. Again, when the breech has been expelled doubled, it has prepared the way for the exit of the shoulders and head much more completely than when it has passed with the feet foremost. The woman's structures must be subjected to a definite degree of distention during the passage of the head; and the extent to which that distention is carried is not influenced at all by the mode in which the breech has passed the pelvic apertures. Is it not better, then, that she should suffer the pain which cannot be prevented, at first, with the chance of saving the child's life, than experience it afterwards, when there is a much greater probability of its being born dead? Dr. Wm. Hunter, indeed, soon saw the danger of interfering in the manner he first adopted; and he tells us, that when he used to extract the legs before the breech, he lost almost every child; but when he changed his mode of practice, and let the breech pass double, and did not allow the legs to escape till after the knees were born, he was much more fortunate in saving the children; and the same facts have been established by subsequent observers. When the body is expelled, indeed, and the arms still remain within the pelvis, our active assistance becomes not only useful, but almost necessary: we may then endeavour to relieve the parts from distension, by bringing the arms cautiously down, for their presence in the vagina can be of no service; they cannot preserve the funis umbilicalis from pressure;—nay, they are actually doing harm, for they take up room, prevent the easy descent of the head, and may perhaps themselves press upon the funis. But in attempting to bring down the arms, our efforts must be most gentle; and we must be very careful to direct the limbs forwards, so that the hands should sweep over the child's face. If we were to turn them backwards, we must necessarily break or dislocate the humerus—an accident that might most easily happen, from the imperfectly ossified state of the bones. That arm placed behind the symphysis pubis must be brought down first, and this is generally not difficult to be accomplished, by sliding one or two fingers perfectly over the shoulder, carrying them a little way along the humerus, and gently directing the fore-arm anteriorly; this being effected, we may bring the child's body forward, so that the side of the neck should be applied closely under the symphysis pubis, introduce two fingers of the left hand back towards the sacrum, and in the same tender manner extract the other arm.

The parts being now relieved from tension, the head having come well down into the pelvis, we may favour the inclination of the face into the hollow of the sacrum by the means just recommended; and if there be any difficulty in so doing, we may pass a finger into the mouth of the child, depress the chin, and give the head the requisite turn. Let us not, however, forget the delicate structures on which we are operating: let us remember that the bones are not strong and solid, but are easily broken; that there is a symphysis in the centre of the jaw; that we may either dislocate the articulation, separate the symphysis, or break the bone itself. You know how tenderly conducted should be the examination of a child's body after birth; and quite as tender should be our attempts to relieve it during labour.

Under a breech presentation, after the liquor amni has been evacuated, the meconium is frequently, but by no means invariably, squeezed out of the rectum, by the mechanical pressure sustained. This circumstance has therefore been noted as a symptom of the kind of case we are considering. On this occurrence I should strongly advise you not to rely for an indication; both because it is not universal in breech presentations, and also because it may take place under other positions of the fœtus. Besides, it cannot appear until after the rupture of the membranous cyst; and it is far better and safer to trust to the knowledge gained by a minute examination of all those points of the fœtal body which can be embraced by the finger, than to any accidental sign.

KNEE PRESENTATION.

We will next consider the case of a knee presentation. We will suppose that the child is at the brim of the pelvis; that the labour in the first instance is going on pretty well—much the same as if the head

or breech presented. The os uteri opens, the membranous bag somewhat protrudes —perhaps in the form of the finger of a glove, instead of assuming the character of an egg; but this is not always the case. Upon making our examination at the commencement of labour, we detect a small round substance, with a flattened surface, possessing the characteristics neither of the head nor the breech. We are then quite sure neither of these parts present; but we may not be so certain whether it be a leg or arm that meets our finger.

If one or both knees offer, the case will usually be terminated by the natural efforts; but if it be an elbow presentation, under which the child lies transversely, we must change its position before delivery can be effected. It therefore becomes a matter of the greatest consequence, that we should discriminate between the patella and olecranon; and I shall mention the distinctive marks, when speaking of transverse presentations.

The knees will descend into the pelvis, the legs will drop out of the vagina, earlier than if the breech had passed double; the body of the child will be expelled; the funis umbilicalis will have lost its protection; and the infant will be in great danger of strangulation.

FOOTLING PRESENTATION.

Again, one or both feet may present, the breech being easily distinguishable by the finger, or lying perfectly out of reach of a common examination.

When we feel the digital extremity of the limb,—since there is no part of the child's body but the hand with which it can possibly be confounded,—it becomes our duty to discriminate between the two, for reasons previously more than once inculcated; and this we can generally do before the membranes rupture. The foot is known by the rounded instep, by the prominent heel, by the toes being all in one line, and by one of the digits not being an opponent to the other. When the hand is at the pelvic brim,—as I shall hereafter tell you, —we feel the flattened wrist and palm, the thumb an opponent to the fingers, the fingers of different lengths, and the absence of the marks just described.

Under presentations of the feet, the labour is usually rather lingering, and dilatation of the parts goes on but slowly; nevertheless, this forms no excuse for hurry or interference: we must wait a moderate time for the descent of the child, and allow nature to accomplish her intention unaided; unless, indeed, there be danger, or some urgent reason for accelerating the labour. Should delay induce us to interfere, or should symptoms of danger supervene, we must take one foot, or both, between two fingers of the left hand, and, by a little traction, bring down the legs, and then we have made the case one of the most simple of preternatural labours.

Circumstances requiring assistance under a breech presentation.—You must already have anticipated that many accidents may happen during the progress of labour under a breech presentation, independently of exhaustion from a long continuance of painful efforts, which will require that delivery should be accelerated; and some of these originate in the mother's system, others in the child's. Thus hæmorrhage, convulsions, or syncope, may induce us to terminate the labour; and delivery is generally more easily accomplished than when the head presents; for we are then compelled either to introduce the hand into the uterus, and change the position of the fœtus, or to apply the forceps, or use the perforator; but when the breech or feet present, we have merely to make extraction by the leg, provided it be easily brought down, or by hooking a finger or blunt instrument over the groin,—as will be more particularly described at our next meeting.

Danger to the child's life would also induce us to expedite the termination of the labour.

We are not likely to ascertain that the child is in jeopardy until after the breech is expelled; but, when the body is half born, our indication may be taken partly from the state of pulsation in the cord, and partly from a futile attempt at respiration being made while the head still remains either in the uterus or vagina. I have already directed that as soon as the umbilicus has appeared externally, a loop of the funis should be brought down, to prevent tension on its vessels; and, at the same time, an observation may be made on the rapidity and strength of the circulation. If the arteries are beating freely, firmly, and equably, about 100 strokes in a minute, the child is in no present danger, and we need not accelerate the labour for its sake; for by so acting we might leave the uterus uncontracted, and occasion an attack of hæmorrhage. But if, on the contrary, the circulation be languid; or if the beats be very rapid, small, feeble, tremulous, or intermittent, some impediment exists to the transmission of the blood, and the child's life is in imminent hazard.

The other indication implying danger to the child, is an abortive attempt at breathing before the head is in the world, known by a sudden spasm of the diaphragm and abdominal muscles, repeated at uncertain intervals. It would appear almost incredible that the infant should endeavour to respire while the face is closely

embraced by the maternal structures, and when it can inhale nothing but the uterine discharges: such, however, I have witnessed on numerous occasions; and my attention was first called to the fact by my father. I have before adverted to this circumstance, and adduced it as one argument to prove that the first respiratory effort was produced by an impression made on the cuticular nerves of a part of the body, and was not necessarily dependent on the nostrils or mouth coming into contact with the atmospheric air. This convulsive effort is never observed so long as the circulation along the funis is carried on with vigour; because, while the child's wants can be supplied through the medium of the placenta, there is no necessity for calling forth the hitherto dormant function of the lungs; but when that source is cut off, a fresh action is requisite for the continuance of life. This gasp, then, is indicative of danger, and, together with a declining state in the power of the circulation through the umbilical vessels, would induce us to expedite the delivery, lest the fœtus should perish *in transitu*. In using our extractive means, however, we must ever remember the sensibility of the mother's organs, and the delicacy of the fœtal body. Violence or force must do irreparable injury; and exertion even, in the case under consideration, is inadmissible.

At our next meeting, we shall discuss the difficulties sometimes met with under breech presentation, and the best means of overcoming them.

A CASE OF FATAL EFFUSION OF BLOOD INTO THE PERICARDIUM.

With Dissection.

By James Carson, M.D. Liverpool.

The following case of a disease which is of rare occurrence, which is obscurely marked by the accompanying symptoms, and which can scarcely fail to produce unusual excitement by the unexpected fatal termination, seems highly deserving of being recorded.

Mr. W., a gentleman about 52 years of age, of a tall and robust form, clear complexion, subject occasionally to dyspeptic affections, though of very regular and temperate habits; of an active disposition, though his occupation was sedentary and confining; had been for twelve months affected with considerable anxiety of mind, in consequence of the doubtful issue of some building speculations. Towards the end of Lent, which he had rigidly observed according to the injunctions of the Catholic Church, on the 11th of March, a day exempted from the prohibitions respecting diet, he had eaten freely of beef-steaks with onion-sauce. He was, at that meal, sparing as usual in the use of wine. On the evening of the following day, he was engaged (in a fatiguing and rather anxious way) with the business of a club, of which he was treasurer. On his return from the club, about eleven o'clock at night, in company with two of his friends, when he had nearly reached his own house, he was seized with faintness and debility to such a degree, that without the assistance of the friends who accompanied him he would not have been able to have kept his feet. Soon after his arrival at his house, he was visited by Mr. Bromilow, his medical attendant. He described himself as faint and exhausted; complained of an obtuse heavy pain at the precordia, and was affected with flatulent eructations. His respiration was free; his pulse 70, and regular, though weak. He had no affection of the head, nor pain any where, excepting as described in the chest. His bowels had been opened that day. Mr. Bromilow ordered an antispasmodic draught, and left him with directions to take something warm, and go to bed. He took the draught, and a weak glass of brandy and water. At three o'clock he sent for Mr. B. again; and, as the pain in the chest was not abated, he expressed a wish to be bled, which Mr. B. agreed to, more with the hope of satisfying his mind than from any great necessity for that measure being indicated by the symptoms. He lost a pint of blood. An opiate was then administered. At this visit, Mr. B. examined the chest more minutely. He applied his ear to the different regions of the naked chest, but perceiving no unusual sound or vibrations, concluded that the heart, lungs, and large vessels, were in a sound state. At five o'clock A.M., I visited him. He felt cold, perspired gently, and chiefly complained of a pain in the chest, which he described as wearisome and oppressive. It was not increased by taking a full inspiration. He had vomited a little in the course of the night,

and had discharged some of the onion sauce he had taken the day preceding the attack. He was much troubled with flatulency, and belched frequently, but was not relieved by it, so far as regarded the pain in the chest. His pulse was regular; the heat of the body natural; and respiration good. He had had no sleep.

From the information given by Mr. Bromilow, connected with my own observation, I considered that nothing could be indicated by the symptoms beyond an affection of the stomach, which is known to exhibit itself in such anomalous forms. He took four grains of calomel, and two of opium. We visited him again at half after eleven o'clock. He had had little sleep. The symptoms remained the same. He was ordered an aperient mixture, and we proposed to visit him again at seven o'clock. At this visit I replied to the anxious inquiries of the family—that we did not see any cause for alarm; that the complaint seemed to arise from indigestion; and that I had no doubt he would recover. At three o'clock in the afternoon he sent for Mr. Bromilow, as the pain still continued unabated, and wished to know if he might have any thing to rub the part with. The bowels had not been opened, and he had had little or no sleep. A short time before seven o'clock, the hour at which we had proposed to visit him, and at which I was prevented from attendance by an urgent call to a distant part of the country, Mr. W. was seized with what the family conceived to be a fit; and, a short time after the arrival of Mr. Bromilow, expired. In consequence of my unavoidable absence, other physicians were called in, and two arrived, but not until after the death of the patient. I applied for permission to open the body, which was granted. The body was examined twenty-four hours after death, by Mr. Bromilow, in my presence, and in that of my son, Dr. Carson, jun. The following were the appearances on dissection:—

Upon opening the chest, the lungs on both sides were perfectly sound and collapsed; but, notwithstanding the collapse, the chest was filled more than it usually is, when the lungs are sound. This indicated the existence of some foreign substance, or morbid enlargement of some of the organs. The pericardium was found accordingly to be immensely distended by some fluid, which, when this bag was opened, was found to be blood, partly liquid and partly coagulated: the quantity was not less than three pints. It was purely blood, without the admixture of any fluid indicating inflammatory action. The external surface of the heart, and internal surface of the pericardium, were examined carefully, but no ruptured vessels, from which the blood might have flowed, were discoverable on either of these surfaces. The heart itself was perfectly sound, the valves were in good condition, and no disease existed in any of the large vessels. The lungs were free from adhesions, and were every where sound. The other viscera were in a sound state. A great deal of care and time was expended in trying to discover the source from which the blood had flowed into the pericardium, but in vain: a slight ecchymosis was observed about the root of the pulmonary artery. Dr. Baillie, in his Morbid Anatomy, says, "Cases have occurred, though very rarely, in which a large quantity of blood has been accumulated in the cavity of the pericardium, but where no rupture could be discovered after the most diligent search, either in the heart itself, or in any of its vessels. This appears very wonderful, and not at all what any person would expect *à priori*. Two conjectures have occurred to me, to explain this phenomenon: 1st, that the blood-vessels on the surface of the heart have lost their compactness of tissue, so that the blood may have escaped by transudation. The other is, that the blood may have been poured out by the extremities of the small vessels, opening on the surface of that part chiefly of the pericardium forming the immediate cover of the heart, from their orifices having been to a very uncommon degree relaxed."

There is a case related by Dr. Alston, in the sixth volume of the Edinburgh Medical Essays, in which the disease of the chest was of long standing. Three pints of blood, which was partly coagulated and partly mixed with lymph, were found in the pericardium. No ruptured vessel was discovered either on the outer surface of the heart, or the inner surface of the pericardium. Upon pressing the heart, a bloody serum oozed out of a great many orifices on its surface, and principally near its base. No disease was discovered in the interior of

the heart or large vessels. Dr. Baillie refers to two cases of extravasation of blood into the cavity of the pericardium, in which the source of the hæmorrhage could not, after the most careful examination, be discovered. In both these, functional disease of the heart had been observed for some time previous to the death of the patient.—*Vide* Medical Observer, vol. x. p. 330; Memoirs of Medical Society, vol. i. p. 238.

Various opinions have been advanced respecting the sources from which, in the above cases, the blood was derived. One of the suppositions made by Dr. Baillie appears to me to approach the nearest to the truth, which is, that the blood had oozed out of the small vessels on the internal surface of the pericardium immediately covering the heart. It is probable, I think, that the oozing, particularly in the case now narrated, arose from the condition of the blood, and the relaxed state of the fibres. It would appear that the disease was general, and that the shivering, faintness, and depression of spirits, were not the effects of the flow of blood into the pericardium, but that this last was, like the affections stated, the effect or symptom of the general disease,—that, in fact, there existed a morbid state of the whole system, similar to that which takes place in purpura, in some kinds of epistaxis, hæmatemesis, and in bleeding from the bowels in typhus fever. The pain in the chest was, in the first place, occasioned by the admission of blood into a cavity not accustomed to the stimulus of that fluid. There is no reason to suppose that the action of the heart would be mechanically affected until the quantity of the blood was pretty considerable; for the blood would readily follow the dilatation of the pericardium, occasioned by the elasticity of the lungs, when the chambers of the heart had finished their contractions. No sound was perceived, on carefully examining the chest: indeed, no sound could be excited, as no fluid was poured from one vessel into another; for, as the auricles expand as the ventricles contract, the change of place in the constituents of the fluid in the pericardium would be inconsiderable, and made with quietness.

There does not appear to be any symptom in this case that would have warranted the medical attendants in giving an unfavourable prognosis. As a matter of prudence, a less favourable one might have been made; but the same prudence would not permit the expression of a favourable prognosis in any case whatever*.

FUNGOID TUMOR OF THE BRAIN.

To the Editor of the Medical Gazette.

Sir,

If you think the annexed case of fungoid tumor of the brain of sufficient importance to entitle it to a place in your well-conducted journal, you will oblige me by inserting it at your earliest convenience.

Your obedient servant,
Wm. Starkey,
Member of the Royal College of Surgeons.

Westgate, Wakefield,
June 20, 1834.

Mrs. S., a married lady, æt. 46, of a full, plethoric habit, but who never had any family, enjoyed almost uninterrupted good health till about five years ago, when she became subject to frequent attacks of headache, accompanied by sickness, which generally terminated in vomiting, the matter ejected having a bitter, sour taste. The pain generally came on in bed, about midnight, making her very restless and uneasy, and would continue most of the following day, and sometimes longer, then leaving her as well as before the attack. In this way she went on, suffering severely at longer or shorter intervals, deceiving herself with the idea that it was a bilious or rheumatic affection, and thus she neglected calling in medical aid, as in the interval of attacks her appetite was good, and she appeared plump and healthy. But even at this time her friends felt uneasy about her, in consequence of noticing that her memory became very defective, and that at times she was hurried and confused in her manner, and at a loss for words to express herself; complained of seeing indistinctly, and as if objects were moving and dancing before her eyes,—symptoms which were much increased about the menstrual period, and relieved after a free and copious discharge. This state of things continued from 1829 till the time I was called in, which hap-

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* Liverpool Medical Journal.

pened on the 26th February, 1833, at 12 o'clock at night. On my arrival, I was informed by those about her that she had been suffering all the previous day from her usual headache; had had several fainting fits; and, as they expressed it, "could hardly keep life in her." She complained of great pain in the head; face hot and flushed; extremities cold; pulse hard, and about 90; tongue slightly furred. I immediately abstracted from the arm 16 oz. of blood; ordered the feet and legs to be fomented with flannels wrung out of hot water, and then dipped in spt. terebinth.; and prescribed for her ext. colocynth. c. c. cal., to be followed up with inf. sennæ, magnes. sulph. and tinct. jalap. On my visit the following morning, at 10 o'clock, I found the cathartic medicine had operated powerfully on the bowels; she had had no return of fainting fits; the extremities were warm; pulse softer, and less frequent; head much relieved; but as she still complained of pain over the eyes, I recommended the application of twelve leeches to the forehead, to keep in bed, and to take a dose of effervescing medicine, with fifteen drops of vin. antim. tart. every four or five hours; the cathartic medicine to be repeated as occasion required. Under this mode of treatment, she was in a few days restored to her usual state of health; and as she had a great disinclination to take any more medicine, and considered herself well, I took my leave, advising her to pay strict attention to the bowels, to abstain from wine and malt liquor, to eat sparingly of animal food, and that only every other day. In about a week, on passing the door, I looked in, and found her lying on the sofa, complaining of her usual pain in the head, great lassitude and depression of spirits; appetite not amiss, and her nights tolerably good; the bowels rather inactive, to obviate which I recommended her to continue the aperient, the head to be shaved and kept wetted with spirituous lotion, and the occasional application of leeches, as the pain changed its seat, being sometimes in one part of the head, and then in another, but most generally about the occiput, shooting upwards and forwards to the temporal regions, and over the forehead. This mode of treatment, with a course of blisters at the back part of the head, and behind the ears, with alterative doses of mercury, and the use of the shower-bath, were continued for about six weeks; at the end of which time she appeared much better, the pain still recurring, but at longer intervals, and with less severity. As the weather was now becoming mild, and the disease was evidently of subacute character, and had hitherto proved very intractable, I began to suspect it might be connected with a strumous diathesis, and recommended change of air, which was immediately carried into effect. She was absent about a month, and returned home without having received much benefit. After a short interval she went to Harrowgate, and then to Scarborough. At the latter place she remained but a short time, as she felt herself getting much worse, and became anxious to be at home. The journey was performed with great difficulty, and was very tedious and distressing, from the great languor and disposition to frequent fainting on the road. At York she was stiff, cold, and insensible, for about two hours.

As soon as she arrived at home I was sent for, and found all the powers of life exceedingly depressed; the skin almost as cold as marble; sensibility greatly blunted; pulse about 50, and very weak. Ordered hot fomentations, with spt. terebinth. to the feet and legs, and mustard cataplasms to the epigastric region; to have hot brandy and water, and take the following medicine:—

℞ Ammon. Carbon. grs. x.; Pulv. Gum. Camph. grs. v., tere c. Spt. Vini, q. s. add.; Sacch. Alb. ʒss.; Aq. Distill. ℥iss. Ft. haust. 2da quâque horâ sumend.

After taking about six doses of the above prescription, and persevering in the hot fomentations, &c., re-action was produced, but not to such an extent as to permit depletory means, the vascular action being still much below the natural standard, the pulse slow and soft, beating about 60, and the extremities could not be kept warm without the constant application of a stone bottle filled with hot water. Local bleeding was again had recourse to, with a succession of blisters and other counter-irritants, which, after a time, afforded considerable relief; and she was again so far recovered as to be able to go out and take daily exercise, by walking in the garden, and riding in a carriage. The appetite was now good, and her sleep calm and refreshing, with only occasional pain in the head; but

not so distressing as to produce depression of spirits and prostration of strength. I now recommended her to try the country again, taking with her some pills of hydr. submur. comp.; to take 5 grs. each night on going to bed, and to use the shower-bath. She was absent about three weeks, took the pills as directed, which operated gently on the bowels, but did not produce the least mercurial effect; and, until within two or three days of her return home, she continued better, complaining only of her sight becoming weaker and weaker, although no change was apparent in the eyes, and lamented that she could not see to work or read. The day after her return from the country I was again requested to see her, and found her labouring under all her former symptoms. The same treatment was again pursued, but without any good effect.

Matters went on from bad to worse; pain in the head constant, tongue furred, pupil dilated, vision more obscure, in the nights frequent sickness and vomiting, with hiccup and complete loss of appetite.

I now felt convinced that I had to contend with some deep-seated and obstinate malady, which had hitherto resisted (except for a time) every thing that I had done for my suffering patient; and, taking into account the uncertainty that prevailed in regard to the true nature of the complaint, I requested a consultation, when Dr. Crowther, the family physician, was called in on the 26th January. Up to this period she had lost little or no flesh; her appearance was plump, and a stranger seeing her would have thought her quite well. The constitutional disturbance at no time corresponded with the local distress in the head; the pulse seldom exceeded 80, and oftener not more than 70; the urine invariably abundant, and straw-coloured. As the digestive organs were now more deranged than at any former period, Dr. Crowther prescribed such remedies as had the effect of restoring them to a better and healthier action; the appetite improved, her nights were better, and pain in the head moderated. This state of things was of short duration; the disease soon assumed a more active character; head-ache without intermission; pulse accelerated, varying from 100 to 110; tongue loaded; appetite gone; bowels inactive; frequent sickness; vomiting and troublesome hiccup; the memory more and more defective; recollection almost gone; the amaurosis complete. Dr. Crowther now prescribed cupping on the temples, and a caustic-issue to be made high up in the neck, sufficiently large, after the slough had separated, to admit half-a-dozen French beans, which discharged very freely, and for a time appeared to afford relief; but this was only temporary. At this period of the disease, March 20th, Dr. Crowther and I usually met twice a day, and continued it for nearly a month afterwards. It was now observed that she was wilder and more incoherent in her remarks, would say the strangest things imaginable, and in a short time after be quite rational: her nights became restless, and she could not be kept in bed, unless held down by two or three other persons. These symptoms were followed by confirmed derangement, accompanied with a high state of maniacal excitement; refusing all medicine and food. Becoming, at last, totally unmanageable, the strait-waiscoat was applied. After this state of things had continued a few days, she would be comparatively calm, with great prostration; the pulse quick and small; tongue of a dark-brown colour, dry and parched, with sordes on the teeth, gums, and about the lips; urine scanty and high-coloured; the strength requiring support by the use of sago, wine, and such other nourishing food as could be forced down. The former state of things would occasionally recur with high delirium, and thus the symptoms alternated for several weeks, losing flesh rapidly, each paroxysm leaving her weaker and weaker. On our visit on the 31st, Dr. Crowther inquired of the nurse, "had she made water?" We both felt surprised to hear that none had been passed since the day but one before; more particularly as previous to this the secretion had been copious, and the discharge regular. Upon examination, the bladder was found to be considerably distended, giving much pain to the patient when pressed upon. The catheter was immediately introduced, and nearly a quart of high-coloured urine drawn off. The next day it was passed twice; the quantity discharged by the catheter, at both times, being about a pint and a half. Its daily use was required for some time afterwards, when nature resumed her own work; the quantity small, resembling a strong infusion of coffee. The bowels were obstinately confined,

and evacuations could only be procured by forcing down cathartic medicine, and using injections. About a fortnight previous to her death, she had cold rigors, with convulsive tremors, each occurring twice in twenty-four hours. Deeply comatose for thirty-six hours before she expired.

Appearances on Dissection, twenty-seven hours after death.—The cranium was thin, the dura mater natural, the arachnoid coat was opaque, and had an effusion of serum beneath it; the pia mater was much loaded with blood, and adhered generally to the substance of the brain. The medullary substance was softer than usual, and the cut surfaces presented many bloody points. About three ounces of serum were found in the lateral ventricles. Several round transparent vesicles, containing serum, were attached to the plexus choroidei.

The appearance of the optic nerves was natural, but the fornix and thalami of these nerves were softer than usual. Externally, the posterior lobe of the left hemisphere presented no difference from the rest of the brain, except that the blood-vessels of the pia mater were more congested, and that the veins in many parts were varicose; but on cutting into it, a fungoid mass was found, somewhat resembling the substance of the placenta in colour, but in substance it was less dense, of an irregular form, occupying about one-third part of the lobe, but no where penetrating through the pia mater.

The cerebral substance in immediate contact with the fungoid mass, was softer than natural. The cerebellum, medulla oblongata, and the nerves proceeding from the brain, were in a natural state.

ON PERFORATION

OF

STRICTURES OF THE URETHRA.

With Cases.

By R. A. Stafford,

Surgeon to the Mary-le-bone Infirmary, and formerly House Surgeon to St. Bartholomew's Hospital.

[Concluded from p. 386.]

Case XVII.—*Case of Stricture, which had produced Impotence, &c., perforated.*

May 18, 1833.—Mr. R., æt. 49. This gentleman, about eighteen years ago, contracted a gonorrhœa, which, in consequence of his surgeon having died soon after he had consulted him, was neglected, and improperly treated. From this cause hernia humeralis was brought on, then strictures, and, two or three years afterwards, retention of urine. During the first part of the complaint he led a very irregular life, drinking and sporting; so that the gonorrhœa continued on him for two or three years: at length, however, he found a difficulty in voiding his urine. This increased until it only flowed by drops, when he became seized with severe rigors and retention of urine. No instruments could be passed into the bladder; and therefore, when he was recovered from this attack, caustic was employed four or five times, with the view of destroying the stricture. Its application produced a very alarming hæmorrhage, so much so that the surgeons who attended him were afraid to continue its employment. By using it, however, they were enabled to pass a bougie through one stricture four inches from the orifice. Another was then found an inch further on. This also was overcome, when a third, much worse than either of the others, was discovered at the bulb: it was quite impermeable to any urethral instrument, and appeared to be of a very indurated structure. As no other remedy but the caustic (excepting the division of the stricture from the perineum) offered the slightest chance of relief, its application was again proposed by other surgeons. The patient, however, having suffered so much before, and having nearly lost his life from hæmorrhage, declined its use. Since this period he has suffered most severely, and informs me, so distressing and urgent have his symptoms been, that he really believes he has sometimes got up thirty or forty times during the night to endeavour to make water; and in the day he has had incontinence. The straining and pain, also, have been so great during the attempt to void the urine as to cause total blindness for half a minute at a time, and frequently to force an evacuation of the fæces. The genital organs likewise have become wasted and impotent, and the lower limbs extremely feeble.

Finding that he had derived no benefit from treatment in this country, he went to Paris, to consult the French surgeons. He did not, however, reap any advantage from the remedies they employed; consequently, having heard of the method of perforating strictures, he was recommended by his son, who was then studying medicine in that metropolis, to return to London, and place himself under my care.

On examining him, I found him to be much in the state as above described, and that he had an impermeable stricture si-

tuated at the bulb. After having prepared him for the operation, I introduced the urethral perforator, and cut through half an inch of stricture. A small gum-elastic catheter was then introduced into the bladder. On the alternate days, the size of the catheter was increased by the introduction of another. A No. 13 could be passed in less than a fortnight. Steel dilators were now passed twice in the week; and in six weeks from the operation this patient left my care, being able to make water as well as ever he had done in his life.

Remarks.—The urgent desire to void the urine very frequently, is common in the worst stage of stricture; and it is one of those symptoms, occasioning fatiguing and restless nights, which tends to wear out the patient. The most remarkable symptom in this case, was the temporary blindness at the time of micturition. This most probably arose from the straining, producing an obstructed circulation of blood through the lungs, whereby the return of blood to the heart was prevented, thus causing a determination of blood to the head; by which, probably, the optic nerve and the retina were affected. I have often seen the same occur, in a minor degree, in passing a bougie; owing, perhaps, to another cause—the nervous excitement, as in blushing, producing a determination of blood to the head. In one instance, the nose has always bled; in another (Case IV. in this series), the conjunctiva has been suffused with blood; and in many, the face has become very red and flushed. Impotence is by no means uncommon in stricture. The erectile and ejaculatory power is frequently lost, or the semen is kept back by the closure of the stricture, and thus impregnation is prevented.

Case XVIII.—*Stricture, producing Impotence, perforated.*

July 1833, Mr. C., æt. 43, a gentleman of a highly nervous temperament, applied to me, under the following circumstances. He had been afflicted with stricture ever since he had been a boy; which latterly had so distressed and fretted him, from the constant pain and irritation he felt in the urethra, that he could hardly speak of his complaint without weeping. He had an anxious and worn-out countenance; he had become emaciated and feeble; he seldom got any sleep, and had entirely lost his appetite. The urethra was so irritable that he could not at first bear the introduction of a bougie without fainting, and feeling the sensation as if a red-hot iron were passed; and he voided his urine only by drops. He had lost the erectile power of the penis, and was quite impotent.

After some trouble and difficulty, I at length got him into such a state as to bear the passage of a bougie, and found a stricture situated seven inches from the orifice. As he suffered much pain in passing the instrument, and as the urethra at the strictured part was extremely sensitive and tender, I only made one incision at a time with the urethral perforator. After four applications of the instrument, I succeeded in perforating the obstruction. A small gum elastic catheter was passed, and left in the bladder. This was withdrawn, and a larger size introduced on the second day. Four days from the operation the catheters were left off, and he was treated by steel dilators. In a month a No. 10 could be passed, that size being the natural calibre of his urethra.

Remark.—In this case, as in the last, impotence was produced by the stricture, and disease was attended by the worst symptoms.

Case XIX.—*Stricture, producing Retention of Urine, perforated.*

In September 1833, I was requested, in consultation with Mr. Walker, of Piccadilly, to see a gentleman who had laboured under retention of urine for more than two days, and who had had permanent stricture for some years. The bladder was extremely distended, reaching as high as the umbilicus; and he was suffering the most excruciating agony, not being able to void even a drop of urine. Mr. Walker had most judiciously employed every remedy in his power, but without effect, and the smallest catheter could not be passed farther than five inches down the canal. Under these circumstances, and as the bladder must otherwise have been punctured, I proposed the perforation of the stricture. Accordingly I introduced the urethral perforator, and gradually cut through about an inch of stricture, which enabled me to pass a catheter fully seven inches. There was, however, so much spasm, that it was impossible to get it into the bladder. We therefore placed him in a warm bath, and bled him to four-and-twenty ounces. On taking him out of the bath, he was ordered a dose of liq. opii sed. (mxxv.), and we agreed to meet, (thinking that, being under the influence of opium, he might be more tranquil,) early in the morning, to make another attempt to pass the catheter, or, if we failed, to puncture the bladder. We met at eight o'clock: a catheter was then

passed with the greatest ease, and at least three pints of high-coloured acrid urine was drawn off. The catheter was left in the bladder, and changed for one of a larger size every other day; and in a fortnight a No. 12 could be passed with the greatest facility. He has continued well ever since.

Remark.—It will be seen that this patient was reduced to the very last extremity before the stricture was perforated. There is no doubt, had not this operation been performed, the bladder must have been punctured.

Case XX.—*Stricture of 35 years' duration perforated.*

April 2d, 1834.—Mr. H., æt. 57, first felt symptoms of stricture in the urethra at the age of 22. Six years afterwards it had so much increased that he was obliged to apply for surgical advice. Sixteen years after this period the passage was so much contracted, that he got an attack of retention of urine, which was so violent that his life was considered in danger. For thirteen years afterwards bougies were passed by different surgeons, but none could ever be got into the bladder. In March 1834 he applied to me, and I found he had an impermeable stricture of the bulb. I passed the urethral perforator, and divided the stricture at one operation. From this period steel dilators were passed, until a No. 13 could be introduced with ease. During the whole time, this patient went about, and followed his usual occupation.

Remark.—In this case, as in many others which I have lately published, no other treatment (excepting the division of the stricture from the perineum) could have been adopted but perforation.

Case XXI.—*Two Strictures divided by the Lancetted Stilette.*

Mr. R., æt. 47, has been afflicted with strictures for nineteen years, which arose from a badly-treated gonorrhœa. He has been in the continual habit of passing bougies ever since; and although he sometimes has obtained relief during their employment, yet they have always returned immediately afterwards. Seventeen years ago he had retention of urine, which ended in extravasation into the perineum, the region of the pubes, and the parts adjacent. An opening was made into the urinary abscess by the surgeons who attended him at the time, and a considerable quantity of urine and pus let out. For about two months after the operation the urine passed through the wound in the perineum. The wound at length healed up, and the natural passage was restored. Since this period he has passed bougies for himself, but of late years the passage has nearly closed up again; and, in consequence of the pain and irritation of the urethra and bladder, his health has greatly suffered; and his rest at night, from the constant inclination to make water, has been broken. On examining him, I found two strictures, one four inches from the orifice, and the other six: through these I could only pass the smallest-sized bougie. On the 18th of this month (June, 1834) I divided the first stricture, and on the 20th the second. Steel dilators have since been passed daily; and at this present time I can introduce into the bladder a No. 12 with the greatest facility. This patient has never been confined, and has gone about as usual. He has lost all the pain and irritation of the urethra, and can void his urine in a full-sized stream.

Remarks.—It may be observed, from the cases which I have now, as well as from those which I have before published, that strictures of the urethra may be perforated or divided, without the least danger, in any part of the canal, even from the orifice to the third or middle lobe of the prostate gland. When I first adopted this treatment, I felt it necessary, as a matter of precaution, to place the patient on a very strict discipline, keeping him on low diet, purging him, and abstracting blood by leeches, &c. It gratifies me now to state, that I have not found of late, unless under peculiar circumstances, such severe treatment necessary: in most cases I have allowed the patient to live moderately well, and to go about as usual, without confining him. After the operation, also, I have omitted leaving a catheter in the bladder, merely passing steel dilators every day for some time afterwards. This plan of treatment has usually answered equally as well as when the catheter has been introduced and left in the canal of the urethra.

In conclusion to these remarks, I beg again to repeat, that I have never known any mischief arise from the operation; the pain has been but trifling,—not more, nor even in some cases so much, as the puncture made in bleeding; there has never been hæmorrhage from the cutting; no false passages made; no extravasation of urine, nor abscesses formed in

consequence; nor, indeed, has there ever been in any instance a single unfavourable symptom occur. The treatment has been always attended with success, and the patients, from having been in the most deplorable and painful state of disease, being emaciated and worn out by their long and continued sufferings, have in a short period been so relieved, that they have gained flesh, and have become healthy and strong.

BIRMINGHAM COUNTER-MEMORIAL.

To the Editor of the Medical Gazette.

Sir,

The following Memorial, transmitted to the Committee on Medical Education, appointed by the House of Commons, is respectfully offered to the Editor of the London Medical Gazette, for insertion or notice in his liberal, candid, and able publication, by his very obedient servant,

John K. Booth.

The Physicians of the Birmingham General Hospital, believing that the opportunities afforded for medical and surgical education by that institution are equal to those of the London hospitals, submit the following reasons in support of their opinion:—

1. The Birmingham Hospital, containing 180 beds, is as large as most of the London hospitals.

2. The manufactories of the town, and the mining operations of its neighbourhood, afford a large number of *accidents*, almost all of which are brought to the Birmingham Hospital.

3. This Hospital, being open to patients coming from any distance, and not appropriated exclusively to the use of the inhabitants of the town, contains cases of *all the diseases* prevailing in the Midland Counties, of which Birmingham may be considered as the centre.

4. Clinical lectures and instructions are given by the physicians, who *regularly* attend three times a-week, and oftener when the cases are urgent.

5. In order to enforce due attendance of the pupils, they are required to enter, at each time, their names in a book provided for that purpose; and *certificates* are not granted to them unless this regulation shall have been strictly observed.

6. Medical cases are accurately recorded for the use of the students; and the bodies of all patients, medical or surgical, who die in the house (if permitted), are examined in the presence of at least one of the officers.

7. All accidents, or urgent medical cases, are admitted at any hour of the day or night, without any ticket of recommendation being required.

8. The Physicians of the Birmingham General Hospital are of opinion that provincial medical and surgical schools, in connexion with the opportunities afforded by such institutions as the Birmingham General Hospital, give to the student a complete knowledge of the *principles* and *practice* of his profession, which is further guaranteed to the public by the examinations in London.

(Signed) Geo. Edw. Hale,
John K. Booth,
James Johnstone,
John Eccles.

General Hospital, Birmingham,
June 28, 1834.

ON PROVINCIAL EDUCATION.

THE BRISTOL MEMORIAL CONSIDERED.

To the Editor of the Medical Gazette.

Sir,

As a provincial practitioner, I request at your hands the favour of being allowed to make a few remarks on the subject of confining surgical education to the provinces—a subject which appears to have laid strong held on the attention of the memoralizing surgeons of Bristol, and has been treated by them in a very self-satisfactory, but not particularly modest way: and the same may be as truly said of the teachers of the "Bristol Medical School," the glories of which I, for one, must confess myself to have been quite ignorant of, until enlightened by the dashing communication with which they have lately favoured the medical world.

In the business of teaching, something more than an ample stock of materials is required,—something relating to the teacher, whose full and intimate

acquaintance with the subject, whose leisure to instruct, and whose capability to arrange and impart his ideas, is of chief importance. It by no means follows that the possession of a mine is attended with the ability profitably to work and explore it, or that a surgeon must be a capable teacher because he is attached to a large hospital; and yet, if the Bristol memorial assert any thing, it asserts this; than which a more illogical and monstrous proposition was never advanced. The qualifications of the teacher, I say, are of greater importance than abundance of materials of instruction; and we ought principally to regard those qualifications, in comparing schools, or admitting to privileges. The Bristol surgeons have, indeed, shown no lack of *spirit* in throwing down the gauntlet on this point, as a reference to the fifth and last head of reasoning will show*; and as no metropolitan surgeon has accepted the challenge, they probably hug themselves in the belief that they have come off victorious—that, albeit unknown beyond the precincts of their city, they are yet equal in ability and desert to the great lights of English surgery, whom we all revere—and that, surgeon-apothecary-accoucheurs as they are, they have still contrived to achieve, in surgery, triumphs, and an extent of knowledge, as great as those who have pursued the subject with not less earnestness, not fewer opportunities, but without the collateral aid of pharmaceutical and obstetric practice.

The medical art is so complex, and embraces so wide a field, that no individual can hope to excel, or reach the highest possible point of perfection in every department of it; there is a necessity, if full justice is to be done to the subject, for a *division of labour*. This principle is recognised and acted upon in every pursuit and profession affording scope for the exercise of the intellectual powers; but in England, as regards the medical art, it obtains only, with a few isolated exceptions, in London; and this it is which seems to me to constitute the foundation of the superiority of the London surgeons. They are strictly and fully surgeons, and their time and mental labours are not, as in the country, largely engrossed either by the superintendence of pharmaceutical manipulations, or the more enchaining occupations of midwifery. Will it be affirmed that a surgeon practising midwifery is as free to devote large portions of his time to his hospital patients, to the public instruction of pupils, and to the preparation and delivery of courses of lectures, as one who forswears that alluring and absorbing branch of practice? Supposing the capabilities and opportunities to be equal, which is likely to become the better surgeon, and prove the better teacher? For my part, I have not the slightest doubt on the matter. The result is as might be expected; and I believe the whole profession, save the interested parties, are pretty much of the same opinion. I really do think Sir Astley Cooper is a greater surgeon than Mr. H——; and that the system of division of labour, rather than any original difference in their mental constitution or education, has enabled the former to become so. But, besides this division of employment, which brings with it leisure, and a stimulus to think, (both too often wanting when a practitioner is daily overwhelmed by a run of all sorts of cases in all departments of an extensive and difficult art), the metropolitan practitioner is exposed to the influence of other circumstances, which must tend to raise his capabilities higher than those of his provincial brethren in general. These are, collision with the highest orders of intellect and attainment, not only in his own profession, but in every walk of life: and will any one say that this constant association with all that the age produces of great, and admirable, and refined, must not impress a mind most favourably, and dispose it to the fullest and severest cultivation of its powers? The other influences are such as are contained in, 1, the practising among better judges of professional merit, which I consider the combined profession, and much of the population of a metropolis, to be; and, 2, the possession of ample libraries and museums, which, though not necessary to every practitioner, are certainly of the greatest use to the teacher, and through him to the student. From all these sources, on which I wish my space would allow me to dilate, I cannot help considering that the London surgeons draw an amount of "capabilities and attainments," which practitioners in the provinces, under the existing system, cannot hope to equal. On the ground,

* Page 191.

therefore, of more information being procurable in London, I think surgical students should be compelled to resort thither. But at the same time, I am not prepared to assert that the law should exist, if the surgeons of many of the provincial hospitals were debarred from indulging themselves in accoucheurs' and apothecaries' pursuits; either of which, however useful and honourable their exercise may be, appears to me incompatible both with conveying the cultivation of surgery to the highest pitch, and with due doctrinal or practical teaching.

As to the inducement the Bristol lecturers hold out to persons, of being able to qualify them at a less expense and in less time than could happen in London, I would make them a present of so creditable and worthy a piece of argumentation, only remarking, that it is to be hoped they will on this text further favour their brethren with their notions of what the claims which medicine has to be considered a liberal pursuit, and its followers to rank as gentlemen, consist in —whether in hastily shuffling through a cheap and imperfect education in some corner of some unknown province, or the reverse?—I am, sir,

Your obedient servant,

A SPECTATOR.

Liverpool, June 25, 1834.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

A Practical Treatise on Medical Jurisprudence, with so much of Anatomy, Physiology, Pathology, and the Practice of Medicine and Surgery, as are essential to be known by Members of Parliament, Lawyers, &c. &c. By J. CHITTY, Esq., Barrister at Law. Part I.

IF any medical man takes up this book, expecting to find in it what its title properly implies, and what the medical profession would presume that a work offered them by Mr. Chitty should contain—namely, law, or legal science, as it is applicable to medicine, he will be grievously disappointed. This large and expensive volume is a mere compilation of anatomy and physiology, drawn from those elementary treatises with which every member of the profession is familiar. The author, indeed, partly admits this in his preface, where he says he does not anticipate being regarded as an authority by medical practitioners, but insinuates that students may find him serviceable as a condenser. To judges, barristers, magistrates, and private gentlemen, as well as to officers of the army and navy, Mr. Chitty conceives that his book will be eminently useful. The volume now published, we are given to understand, is but a portion of the whole work; which is to embrace, "*First*, anatomy and physiology; *secondly*, pathology, and the science and practice of medicine and surgery, &c.; *thirdly*, medical jurisprudence and police, and the laws relating to public health and injuries to the person, and the medical and surgical evidence; and *fourthly*, the laws relating to all members of the medical profession." If Mr. Chitty had confined himself to the last three heads, he would have prudently remained within his own element, and been more likely to produce a work of real value. As it is, we have here only a common-place book or compendium of anatomy and physiology—his *first* head; and how he is to contrive to dispatch the other three or four important topics of which he means to treat, as he promises, in another volume like this, we are rather at a loss to conceive. But a word or two touching the materials before us.

Mr. Chitty allows, though somewhat reluctantly, that counsel in general are shallow men of science: but he obviouly proceeds on that admission throughout his work, for we find him every where defining the simplest medical terms, and overloading his pages with puerile explanations. Even "anatomy," he informs us, is derived "from the Greek *ana*, 'apart,' and *temno*, 'to cut,' or 'to cut apart,' or dissection!" We copy this clumsy attempt at etymology *literatim*. "Physiology," "pathology," "surgery," &c. are disposed of in like manner; but we may observe, that where derivations would be more wanted—as where certain terms from Mason Good's nomenclature are most needlessly introduced —none are given. Thus we find the words *Empressma Laryngitis*, and *E. Bronchleminitis*, perhaps, to puzzle the young lawyers, offered without ceremony; at the same time, the reader is informed

that the Saxon word Throat is " from the Greek θροεω, to speak."

There are several errors, not merely typographical, scattered through the volume, which would require careful correction. Thus we have in several places " the cutis ver*um*" as one of the " three skins" by which the body is covered. We observe, also, " Pneuma Thorax" mentioned as a disease of the chest, and " cerum" given as the Latin name for ear-wax. The plates which accompany the text are wretchedly done: some of those representing the viscera are very vile and execrable.

Hortus Medicus; or Figures and Descriptions of the more important Plants used in Medicine, &c. &c. By George Graves, and John Davie Morries, M.D. Edinburgh, 1834.

The author states, that he has been induced to undertake the present work from a consideration of the expensive manner in which other volumes on this subject have been got up: nevertheless, the price of the one before us is two guineas coloured, and thirty shillings plain. The plates, however, are very far superior to those in any work on medical botany published in this country; and some of them are excellent specimens of the art. They have one great defect—that of being in many instances badly selected; that is to say, we have beautiful representations of plants familiar to all, while those with which we are least acquainted are for the most part omitted. This ought to be remedied in another edition, or in another volume.

The medical and pharmaceutical part is a mere compilation; nevertheless, the compilation is a good one.

MEDICAL GAZETTE.

Saturday, July 5, 1834.

" Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

Cicero.

BIOGRAPHICAL SKETCH OF SIR GILBERT BLANE.

We had only time last week to announce the death of this distinguished man, in the 85th year of his age. We propose at present to lay before our readers a short sketch of his life.

Sir Gilbert Blane was born in 1749; being the son of Mr. Gilbert Blane, of Blanefield, a small estate in Ayrshire. He received the early education which is usual in Scotland among those intended for the church—for this was Sir Gilbert's original destination. Circumstances, however, having occurred to change his views, he embraced the profession of physic with ardour, and distinguished himself among his fellow-students at the University of Edinburgh, where he spent five years. In the year 1775, at which time he was one of the Presidents of the Medical Society, the building wherein the meetings are still held was erected, on which occasion he delivered an oration highly creditable to his talents. Already, while still at the University, Blane had distinguished himself among his fellow-students by his talents and industry. He attracted the notice of Dr. Robertson, the author of the Scottish History; of Dr. Blair, the author of the popular sermons; and of Dr. Cullen. By them he was recommended to Dr. William Hunter, who was then at the zenith of his reputation in London. About this time the last Lord Holdernesse was declining in years and health, and required a domestic medical attendant; and Blane was selected by Dr. Hunter as the most fit person for this office. The above nobleman was reckoned the most accomplished, and one of the most highly-educated men of his time, and was much in the confidence of His Majesty George the Third. Through him Dr. Blane was at once introduced to that circle of fashionable life in which he was destined at a future period to move. To the kindness of Lord Holdernesse likewise, joined to the professional recommendation of Dr. Hunter, he owed his introduction to Lord Rodney, with

whom he sailed on his brilliant naval expedition, in the capacity of private physician, without any public situaion. During the first engagement, every other efficient person on the quarter-deck being killed, wounded, or absent on other duties, Sir Gilbert was employed by the Admiral in conveying orders to the officers at the guns: on one of which hazardous missions he was slightly wounded. So favourable an impression was made by Sir Gilbert on his friend and patron by his conduct on this occasion, that the victory was followed by his promotion to the high office of physician to the fleet. In this situation he continued to attract the favourable notice of his commanding officer, with whom he was present at not fewer than six of the most celebrated engagements which took place between 1779 and 1783; and of the battle of the 12th April he gave a vivid and authentic account, in a letter to Lord Stair, which was afterwards published. During this time he had the chief medical charge of the fleet in the West Indies, and collected much important information relative to the diseases of seamen, and their treatment, and introduced numerous improvements in the department under his control.

The nature of Sir Gilbert's appointment precluded him from enjoying half-pay; but in consequence of the unanimous application to the Admiralty, of all the principal officers who had been on the station, he was rewarded by the grant of a pension from the Crown; which pension, at a later period, was doubled, by the recommendation of the Lords of the Admiralty.

After he returned from the West Indies, Sir Gilbert settled in London as a physician, and in 1785, on the application of his present Majesty, he was appointed Physician Extraordinary to George IV., then Prince of Wales. About the same time, or a little before, he was elected physician to St. Thomas's Hospital; for which he was indebted much to the friendship of Sir Walter Farquhar and Dr. Saunders, but perhaps still more to the zealous co-operation of his former patron, Lord Rodney, who seems to have canvassed for him with all the hearty good-will of a British sailor. In a letter addressed to one of the Governors, his Lordship says—

"I heartily wish that Dr. Gilbert Blane may succeed as one of the physicians of St. Thomas's Hospital. Britain owes this proof to Dr. Blane: for to his knowledge and attention it was owing that the English fleet was, notwithstanding their excessive fatigue and constant service, in a condition always to attack and defeat the public enemy. In my own ship, the Formidable, out of nine hundred men, not one was buried in six months. May I beg you will make, with my best respects to the Governors, my most earnest request in behalf of Dr. Blane."

The government, during a series of years, had recourse to the experience and sagacity of Sir Gilbert on many important occasions. When the Admiralty was under Lord Spencer, he was appointed one of the commissioners of the sick and wounded; a branch of the public service which he greatly improved. His arrangements for the provisioning of fleets on foreign stations were of the most superior description, particularly in supplying them with abundance of lemon-juice, so that it became a regular ingredient of the sailors' diet; and the scurvy in consequence was speedily banished from our fleets. He was consulted on the subject of quarantine, on the arrangement of the hulks, and of some of the prisons on shore, and also with respect to the transportation of convicts; in all which departments he enforced an attention to cleanliness and ventilation to an extent which had not been previously practised, and the results

of which were conspicuously beneficial. The most remarkable occasion, however, on which his opinion was sought, was during the great and alarming mortality which took place among the troops sent on the unfortunate expedition to Walcheren. The report which he then made was concurred in by the medical officers of the army, and led to the abandonment of the island.

Soon after this period he was created a baronet by the Prince Regent, having now been for a considerable number of years resident in the neighbourhood of the palace, and in extensive private practice.

Sir Gilbert Blane, after his labours at sea were terminated, gave to the world, during the first twenty years of the present century, various works, and numerous papers in the transactions of "a Society for the Improvement of Medical and Surgical Knowledge"—in the "Medical and Chirurgical Transactions"—as well as in some of the periodicals of the day. Neither were his labours altogether confined to professional subjects; for during the scarcity of 1799 and 1800 he published a treatise, in which he shewed not only the most efficient means of obviating the danger of famine, but of guarding against the evils of discontent and combination among the lower orders. At this time the great scarcity had led to riots and disturbances in the country. Several of the judges, and particularly Lord Kenyon, in his charges to juries on the trials of the rioters, had denounced the practice of forestalling and dealing in corn, as the cause of the famine. Blane's pamphlet was reckoned a very successful and useful one in destroying these prejudices. He also published some observations on naval tactics, particularly on the plan of breaking the line, as practised by Lord Rodney, and also wrote a pamphlet to shew that corporal punishment could not safely be dispensed with in the army or navy.

One of his most important works is his Medical Logic, which was published in 1819; and which has gone through several editions. He also edited his medical works in the form of a miscellaneous collection; and it is remarkable that he was engaged in correcting the last proof-sheet of those volumes, concluding with the words "valete, vixi!" when he was seized with that illness from which his mind, at least, never recovered. Nor is it perhaps less striking, that, when he had exhausted the last remains of his understanding in warning his country against the dangers of cholera, his own wife should have been selected as one of the first victims of its invasion.

In the year 1826 Sir Gilbert was elected, by a great majority of votes, a member of the French Institute, although some of the most eminent men in Europe had been put in nomination for the honour; and on the death of Sir Humphry Davy he was elected an honorary member of the London College of Surgeons,—the bye-laws, we believe, allowing of but one or two eminent individuals enjoying that honour. He was also a Fellow of the Royal Societies of London and Edinburgh, and a member of many other learned bodies.

In the winter of 1821 he suffered dreadfully from prurigo senilis, the irritation being of the most harassing and exhausting kind. To this disease he remained more or less subject during the rest of his life, and he obtained no respite from his sufferings except when under the protection of large doses of opium; but he never succeeded in completely conquering this enemy of old age, and remained ever afterwards a slave to the remedy, as well as a martyr to the disease. His sufferings

from it were, at times, very painful to witness, leaving him little rest by night or by day. Under this violent infliction, he was obliged to increase the dose of opium to a degree seldom witnessed; having sometimes taken as much as a solid drachm in the twenty-four hours: while, during his last illness, two, and even three, hundred drops of Battley's sedative liquor were given for a dose; so that the quantity, in twenty-four hours, sometimes equalled 1000 minims. His medical attendants often deplored the necessity of this remedy, and attempts were made, more than once, to reduce the quantity and to leave it off; but no one would venture to push the experiment far; and those about him were convinced, by observation, that the stimulus could not have been withdrawn without his sinking under the deprivation. The vital flame seems latterly to have been manifestly kept burning by this artificial means.

In January last he had a very severe attack of diarrhœa, through which he struggled, contrary to the expectation of his medical attendants, but he never recovered his strength. This disorder was quickly followed by anasarca and ulceration of the legs, the great drain from which gradually exhausted him. From the 12th of last month he was constantly confined to bed, and died on the morning of the 26th, having been attended by his relation, Dr. Gairdner, and by Dr. Maton, together with an old and intimate naval friend, Mr. A. Copland Hutchison.

Sir Gilbert Blane was an accomplished and learned physician, endowed with great natural sagacity, and extraordinary zeal and perseverance in the pursuit of knowledge. He was deeply anxious for the advancement of his profession, and never forgot the interests of that department in which he began his career;—as an illustration of which, we may mention his establishment of a prize medal for naval surgeons, so often referred to in the pages of this journal. So sensible were the medical officers of the navy of his exertions in their behalf, that they presented him, in 1821, with a handsome piece of plate, bearing an appropriate inscription.

In his appearance, Sir Gilbert had much of the Scotchman, and he retained something of his northern accent to the last. His manners, though he had been much in the most polished society, were not remarkable for strict attention to minute points of etiquette; and the station he attained may fairly be attributed rather to his talents and industry, than to the possession of external graces or artificial attractions.

THE ORIGINAL MEMORIAL FROM BIRMINGHAM.

As we have given the respective documents from the Bristol, Bath, and Winchester surgeons, on the subject of provincial education, as well as the counter-memorial from the physicians of the Birmingham Hospital (see p. 480), we think it right to present our readers with a copy of the original memorial from Birmingham, although we have already stated its leading sentiments at some length in the course of our remarks on this subject.

A rough draft of an intended memorial from the surgeons of the Bristol Hospital, praying the Legislature to make such alterations in the charter of the Royal College of Surgeons in London, as will in future dispense with residence in the metropolis, having been presented to the surgeons of the Birmingham Hospital for their approval,

They beg to state, that having carefully and maturely weighed all the objections stated therein, they respectfully decline taking any part in such petition.

They do not consider the reasons of such moment or weight as to justify the

taking away from the Court of Examiners of the Royal College of Surgeons such a power as complained of—viz. the compelling the six months' attendance at a London Hospital.

They consider the Provincial Medical Schools of great value to the students of the medical profession, as elementary ones, and as laying the ground-work of sound anatomical knowledge in the early career of the student; and they think the Court of Examiners of the Royal College of Surgeons have, with sufficient liberality, met the views of such preparatory schools, in remitting attendance on the metropolitan hospitals to the above-named period.

In reply to the objections—viz., *in the first place*, "the opportunities being fully as adequate in most of the provincial hospitals for every purpose of education as those in the metropolis"—we beg to observe, that we see great advantage resulting, and always likely to result, from students being obliged to attend for six months at the metropolitan hospitals. The comparisons they will necessarily draw from the diversity of practice in operations, treatment, &c. will be of infinite use to them when they themselves become hereafter active and responsible practitioners.

The second objection—"That the morals of students are more likely to become corrupted in London than in the larger provincial towns,"—*is not at all borne out by facts.*

The third objection—"That owing to the present existing restriction, students very commonly neglect to avail themselves of present advantages, in the hope that they shall have full opportunity of redeeming them when in town"—is, in our opinion, *not* valid. The idle and dissolute are to be found in every walk of life; and with the existing privileges, we do not believe that students in general are so negligent of their own future interests as the objection would seem to imply.

The fourth objection—"That it implies an invidious distinction between the teachers in the metropolis and those in the provinces," is, in our opinion, wholly groundless.

The fifth and last objection—"That it occasions a very considerable increase in the pecuniary expenditure, without any advantages in any degree correspondent to the extent of the outlay," must be considered *a mere matter of opinion*, and subject to various considerations.

In conclusion, we consider the privileges already granted to provincial schools, so far as the Apothecaries' Company is concerned, very injurious, and likely to be still more so to the profession in general, owing to numbers of students who merely pass their examination at Apothecaries' Hall, and which they can do without residence in London, or attending any surgical practice in the provincial hospitals, and who immediately begin as general practitioners in medicine and surgery, in violation of the interests of the members of the Royal College of Surgeons.

(Signed) Richard Wood,
Bowyer Vaux,
Joseph Hodgson,
Alfred Jukes,
Surgeons to the Birmingham Hospital.

May 6, 1834.

COLLEGE OF PHYSICIANS.

Monday, July 1, 1834.

Sir H. Halford, Bart. in the Chair.

This was the last meeting of the season. We observed a considerable number of eminent provincial physicians and surgeons present; more particularly some of the most distinguished among our Edinburgh brethren. Two papers were read—for the first only of which we can at present make room. The second, which was from the pen of Dr. Gregory, we shall lay before our readers next week.

On Poisoning. By Dr. Wilson,

Physician to the Middlesex Hospital.

A Case of Poisoning by Carbonate of Barytes.

Before stating the case, it may not be improper to observe that Orfila had never seen a case of poisoning from the compounds of barytes; and the only one he records is extracted from the Journal of Sciences and Arts, and told in about three lines. It was a case in which a girl took an ounce of hydrochlorate of baryta, in mistake for Glau-

ber's salts; which caused a sense of burning, vomiting, convulsions, cephalalgia, and deafness, followed by death in an hour's time.

Orfila, from various experiments on animals, concludes that pure barytes, or the carbonate, produces death by acting on the nervous system, and corrodes the parts with which it comes in contact, and that the best antidote is sulphate of magnesia or soda, because barytes has such a strong affinity for sulphuric acid, that sulphate of barytes is immediately formed, an insoluble and inactive compound.

Mr. Brodie, also, from his experiments with hydro-chlorate of barytes, concludes that death is caused by its action on the brain and heart.

The recorded symptoms produced on brutes by barytes, and the effects of it on the human subject, are strikingly similar,—as the following case well illustrates.

A young woman half filled a tea-cup with carbonate of barytes, (which was kept in the house for rats, and marked "Poison;") then filled up the cup with water: she said the powder fell to the bottom like white sand; she stirred it up, and swallowed the whole contents; she found no particular taste; had fasted twenty-four hours previously. Soon afterwards medicine was given her, which caused her to vomit.

On her way to the Middlesex Hospital in the evening, two hours after the event, she found, for the first time, dimness of vision, succeeded by double vision, ringing in the ears, pain in the head, and throbbing in the temples, a sensation of distention, and weight at the epigastrium: she said she felt as if blown up with wind, and complained of palpitations.

When in bed, she first complained of pain in the legs and knees, and cramps in the calves. She vomited twice a fluid like chalk and water, which formed a deposit. Her skin was hot and dry; her face flushed; pulse 80, full and hard. Repeated doses of sulphate of magnesia were given to her.

During the night she had fifteen evacuations; had no sleep, from pain in the head and epigastrium, and ringing in the ears.

The next day she had a hot skin, with profuse perspiration, and slight pain about the pharynx. Her tongue was covered with a white fur, and moist.

A day or two after, the cramps became more severe in all the extremities, with a sense of weight, and soreness when touched.

These symptoms, slightly modified, lasted a long time: those which persisted the longest, and which still exist, are severe pains in the head, pain in the left side and epigastrium, great and long-continued palpitations. There has been much difficulty in persuading her to take any sustenance.

The effect on the circulation, in this case, is well marked by the severe headaches and throbbing of the temples, and the frequent and long-continued palpitations.

The effects on the nervous system are marked by the abnormal vision, the ringing in the ears, the cramp, pain, and sense of weight, with numbness of the extremities.

The slowness of her recovery may in a great measure be attributed to the moral affection under which she has laboured. She left the hospital last week.

A Case of Poisoning by Nitric Acid.

A boy, employed at a coach-maker's, took up a quart bottle of nitric acid, thinking it to be small beer, and drank some of it, which immediately caused vomiting of mucus, with several clots of blood. He was brought to the Middlesex Hospital in half an hour after, when the tongue was swollen, and of a citrine appearance, the uvula and tonsils enlarged, as if œdematous; there was great pain about the larynx and pharynx, but none elsewhere; pulse small and feeble; constant vomiting, which was encouraged by frequent draughts of calcined magnesia. Two hours after, he was unable to swallow liquids; had a mixture of gum and honey to moisten the mouth; the sickness ceased, and he began to doze.

In the evening, the pulse more feeble and intermitting; the voice very feeble; face blanched; eyes heavy, and half closed. The bowels had been twice opened, and leeches applied to the throat.

Twelve hours after the accident, the boy was evidently dying. It was judged advisable to make an opening into the trachea, which was done by Mr. Arnott; after which the muscles about the neck ceased to act laboriously; the chest was only raised as in a deep sleep;

he appeared to be decidedly relieved from the most urgent symptoms, and again fell into a calm sleep.

Next morning he continued to breathe freely through the opening, but could not swallow; had a beef-tea enema; tongue and lips covered with a sloughy coat, peeling off, leaving a red surface, with elevated papillæ. He struggled through the day, and died in the evening.

Examination of the body, sixteen hours after death.—The tongue was covered with a layer, having all the appearance of the epidermis, as it separates from the skin when in a state of decomposition; it was perfectly detached at the base of the tongue, leaving a smooth surface; but the layer remained over the whole pharynx, and for an inch and a half down the œsophagus, of a more dirty citrine colour than on the tongue, which, being elevated, the part beneath was vascular, shining, and feebly adherent; about the middle of the œsophagus, it seemed as if the same layer had been detached, leaving the appearance observed on raising the lining membrane in the pharynx—that is, there were patches of vascular membrane running down, and having edges where the inflammation had ceased; at the cardiac orifice of the stomach, the membrane was thickened, but not destroyed; the surface of the stomach was healthy, except that it exhibited a brownish appearance at the glands; the epiglottis was shrivelled, corrugated, and brownish; the aperture much diminished; the cavity of the larynx was shining after being washed, so that some flakes, similar to those afterwards seen over the cricoid cartilage and below the larynx, to the extent of an inch down the trachea, may have been washed away from the body of the larynx. The vocal chords were natural; on separating them, the membrane seemed rather shining and vascular. Below the point where these flakes were noticed, the trachea became gradually more vascular, and at the bifurcation of the bronchi the membrane was intensely red, and also through the smaller branches; but no secretion of pus was seen.

The lungs were gorged with sero-sanguinolent fluid.

The preparation is on the table, exhibiting the appearances which have been described.

A Case of Poisoning by Sulphuric Acid.

A young woman, having bought two pennyworth of oil of vitriol, while in the shop swallowed part of it; which being observed by a little girl who was with her, she struck the woman's elbow, and the cup fell down.

The patient was brought directly to the Middlesex Hospital. When admitted, she had a constant desire to vomit, and what she brought up was of a dark-brown colour. The state of the mouth could not be ascertained distinctly, from the whiteness caused by the magnesia taken before her admission.

Two hours after, her lips began to swell; the pain in the throat and stomach increased, with a violent burning sensation. She had a restless night, with frequent vomiting, and a sensation as if she were going to be choaked. Her tongue was covered with a white dense fur, with traces of dark veins, like those of the leaf of a tree; but in vomiting, something like skin was said to have been brought up; since which the tongue has been red. There were scabs forming about the lips, hands, and arms, where the acid had touched them.

During the first four days she was bled to upwards of thirty ounces, and had about 100 leeches applied to the throat; and she took magnesia and linseed-tea, &c.; after which, the abdominal pains became much less, the vomiting less frequent, and she could swallow with less difficulty. Her voice was pretty distinct, with a soft and regular pulse, and a moist and cool skin; but she had two or three shivering fits, followed by what she called a cold pain felt about the navel.

Two days after, the cough became more troublesome, with constant irritation in the throat, which continued for some time; when, during a violent fit of vomiting and coughing, she brought up a large piece of sloughy membrane, which was found to consist of the inner coats of the œsophagus, much thickened, and very firm in texture; its length was eight or nine inches, and its width that of the œsophagus, being pervious throughout its whole extent. The inside was quite smooth, the anterior ragged; so that the entire mucous membrane, with perhaps some of the circular fibres of the muscular coat, may both be found in the ejected tube which has

been preserved, and is now placed upon the table. She experienced great pain in the night after.

The further history of this case, for the last six months, must in this place be omitted: suffice it to say, her varied symptoms seem to be much influenced by atmospheric changes. During the days on which she suffers the most, she is unable to take any sustenance, and her oppression causes her to bring up, by retching, a pint or two of mucous fluid during the twenty-four hours. Of late she has rarely been able to swallow any thing but fluids; the only medicine she has had, excepting in the few first days, has been castor-oil occasionally. She is much wasted, but in good spirits; and now, at the expiration of six months from her admission, is much better than she was two months ago.

ROYAL INSTITUTION.

Friday, June 13, 1834.

Dr. Faraday on Caoutchouc.

It was observed by the lecturer that the substance of which he meant to treat this evening—caoutchouc, or Indian rubber—was hardly known about a century ago, though now become an article of such high importance, not only for its remarkable properties, but in a commercial point of view. It is not a little singular that when the people of Old Spain first settled in South America, about 300 years ago, the tree which yields the inspissated juice of caoutchouc was cut down as useless. Dr. Faraday called the attention of his auditors to two fine specimens of the plant, which he brought before them. But other parts of the world besides South America produce this tree: large quantities of caoutchouc are now commonly imported from the East Indies and other hot climates. The colour of the juice originally is a yellowish white; but by exposure to the atmosphere it grows dark. The natives also, who gather it, by their adulterations render it of a darker hue than natural. The mode of shaping it into bottles is well known; the juice being collected on clay balls, which, as soon as the caoutchouc is dry, are broken up.

Three kinds of oil are prepared from this substance, of different degrees of value, and sold at from four shillings to twenty shillings per gallon. Cocoa-nut oil, which has been much in use of late, has been found objectionable, from the circumstance of its solidifying in lamps: this objection, however, is removed by adding a little of the caoutchoucine to it, by means of which it is kept fluid, and yields a brilliant white light; and thus cocoa-nut oil is rendered much more economical and serviceable than sperm oil, for domestic purposes.

Besides this application of caoutchouc to the purposes of fuel, it is also available for the manufacture of various textures which are all water-proof—such as shoes and boots, thread of the finest kinds, elastic balls for juvenile sports, bandages used in surgery, girths, braces, &c. Dr. F. described the process of manufacturing water-proof cloth. A thin fabric is covered with a layer of fluid caoutchouc, another piece of the fabric is laid above this, and the whole is then pressed together and allowed to dry. It is a curious fact that caoutchouc, after having once been dried and made use of, can never become fluid again—a circumstance which the learned lecturer professed himself unable to explain. The varnish which caoutchouc yields, when used as a solvent for copal, is of the highest value in the arts: pictures are perfectly protected by it from the effects of the atmosphere. Indian rubber was first brought to the notice of English artists by Dr. Priestley, 70 or 80 years ago: he recommended it, in a work of his on Perspective, as a substance well adapted for removing blacklead lines. It was sold, he said, by a person at the 'Change, for the very reasonable price of 3s. for about a cubic inch; but it was very durable—and would never wear away! Chemists would be glad to hear, Dr. Faraday said, that a new principle had been discovered in caoutchouc; he had had a small portion of the substance (which he exhibited) sent him by the discoverer, at Dresden. Another remarkable phenomenon connected with the oil of caoutchouc is, that it is attended by a dense vapour on its surface, which may be decanted off, and carried in an open vessel.

Such were the principal facts relating to caoutchouc brought before his audience by the learned lecturer. In conclusion, he took the opportunity of briefly alluding to the satisfactory manner in which the evening meetings have been conducted during the season. This was the last of them; and in the name of the Committee, Dr. F. returned thanks for the support which they had received.

CLINICAL LECTURES ON MEDICINE,

Delivered at the Hôtel Dieu, Paris,

By M. Chomel.

[From the "Leçons" published under his immediate inspection.]

ANATOMICAL LESIONS IN TYPHUS FEVER.

The morbid changes of structure met with in those who die of typhus fever are no less varied than the symptoms which we have described in a former lecture. Some of these changes are constant, or nearly so, while others occur in a certain number of cases only; hence the distinction into lesions which are essential, and lesions which are accidental—a very important division, and one which we shall therefore adopt.

LESIONS WHICH ARE CONSTANT.

The lesions which are constantly, or nearly always, met with in typhoid fever, occupy the follicles of the intestines and the ganglia of the mesentery. The alteration of the follicles, whether the glands of Brunner or of Peyer, is far from being always the same. If the form of fever changes during the progress of the malady, corresponding changes take place in the anatomical lesions, dependent upon the epochs at which such variation in the type may have occurred. I shall describe these changes, following them through the different phases of the disease.

Tumefaction of the intestinal follicles and mesenteric ganglia.—It would be difficult to say at what epoch of the fever this alteration commences, because I have not had occasion to open the bodies of typhus patients who have died before the seventh day; and the most recent works contain no information on this point. Of fifty-five cases examined by M. Louis, the most recent had not proved fatal till the eighth day. M. Brettonneau appears to have examined the body of one patient who died on the fifth day. The case has not been published, but if we may judge from the report of one of his pupils, it would not appear that the lesion found at this time differed from those which presented themselves to me at a later period—viz. the seventh day. At this time the intestines are usually found distended with gas, which augments their transparency, and in many cases enables us to distinguish spots externally, of greater or less size, and more or less opaque. In examining the bowel externally, we discover salient patches, corresponding to these opaque spots, which are raised one, two, or three lines, and have projecting borders. The colour of these patches varies with that of the surrounding tissue, but is always darker. Their size and form are not less various: the larger are elliptical—the smaller are more round, but appear, like the former, to have their seat in Peyer's glands. Besides these, still smaller ones are sometimes met with, occasionally resembling a large pustule; these are Brunner's glands. Such patches are found in the ileum, and sometimes at the end of the jejunum; but in the greater number of cases they occupy the end of the small intestine. The largest spots are always opposite the insertion of the mesentery, and are placed at variable distances from each other. Sometimes we also meet with this lesion in the large intestines, which only exhibit spots of small dimension; for in the natural condition, aggregated follicles are never met with. The mucous membrane which covers these, appeared, in the only case which I had an opportunity of observing, at the seventh day, and in the few wherein death took place near this period, not to have suffered any appreciable change. When an incision is carried through the thickness of one of these spots, and the edges of the section are examined, the mucous membrane is first distinguished, and then beneath it a layer, the thickness of which varies from one to two, or even three lines, formed of a white or slightly yellow, firm, smooth, and brilliant substance. This structure has some analogy with that of tubercle not yet softened, but differs in respect to its brilliancy, which contrasts strongly with the dull aspect of tubercle. Beneath this layer, which occupies the whole extent of the patch, is usually found the cellular tunic, then the muscular, and lastly the peritoneum. This whitish matter, however, in the isolated follicles, presents itself in the shape of little cones. It is rare that we can distinguish an orifice at the summit of those follicles which occupy the extremity of the small intestine, while it is often distinguishable in those follieles of the cœcum, or colon, when they are changed to the same extent.

At the same time that we perceive these changes within the intestine, we also find the lymphatic glands in the neighbourhood of the patches evidently diseased; they are considerably enlarged, and of a deeper colour than natural. Sometimes, also, they are softened; although there are instances, in which, on the contrary, they become preternaturally hard.

The alteration of the follicles and of the lymphatic glands, above described, generally follows a regular course. It scarcely ever happens that all the follicles of the intestine are affected from the beginning; it is first in the part nearest the ileo-

cœcal valve that the lesion is most distinct, the appearance of change diminishing in proportion as we recede from this point; and the same circumstance is observed with regard to the order in which the ulterior changes take place.

Ulceration of the Follicles.—If the patient, instead of dying a few days after the commencement of the attack, sinks at a later period, during the second stage, the lesion of the follicles presents a different appearance, varying according to the date at which the disease proves fatal, and other less appreciable circumstances.

The first modification which is perceived on the surface of the patches, is in the mucous membrane which covers them. This membrane commences, about the eighth, ninth, or tenth day, or even later, to lose its smooth appearance; it becomes rugous, cracks, and even entirely disappears; giving place to an excavation which penetrates more or less deeply into the layer beneath it, the nature of which has already been explained. The exact period of the development cannot be fixed with precision. Of nine patients who died in the clinical wards during the second period, four presented patches and follicles without ulceration; five shewed ulceration in different degrees; and from them it would appear that the ulceration usually begins between the ninth and twelfth days. In some recent works, cases are related in which the patches were untouched by ulceration so late as the twenty-eighth and thirtieth day of the disease. However, in admitting that the date of the original attack has, in these cases, been marked with sufficient accuracy, they can only be regarded as exceptions to a general rule, nothing analogous having been observed either by M. Louis or myself; and our cases, taken together, amount to not fewer than ninety-two, in which the bodies were examined after death. I may therefore state, in a general way, that it is from the eighth to the twelfth, or at most to the fifteenth day, that the ulceration of the mucous membrane commences. Two distinct varieties are met with in these ulcerations. In a certain number of cases, the ulceration which succeeds to the engorgement of the follicles begins in the mucous membrane which covers them. It presents, at first, a small ulcer, which extends in breadth and depth, and precedes the destruction of the layer beneath. In other cases, the ulceration commences with the softening of the layer which is beneath the mucous membrane. Suppuration takes place, as in cases of gangrene, and the mucous membrane, as well as the layer beneath it, constitutes a kind of debris, which has been sometimes looked upon as an eschar. These two forms of ulceration, although very different in their mode of development, are sometimes met with in the same individual, and both equally lead to the same result; namely, the destruction of the mucous membrane, and the adventitious layer which forms in patches beneath it.

The ulceration of the isolated follicles differs little from that already described.

One of the most remarkable phenomena during the period of which we speak, is the colour which the patches sometimes assume when the ulceration commences. Occasionally the debris of the patches preserve, even after careful washing, a yellow or green hue, evidently dependent on the action of the bile. As to the rest, however, these cases present nothing remarkable, except merely this yellowness of the eschars.

Intestinal ulcers.—If the patient sinks at a period still more advanced than we have supposed in the cases above mentioned, the alteration of the follicles presents new modifications. In some patients, not only do we find no patches remaining, but there is actually no trace of them left, nor anything but ulcers, which in most cases indicate the situation of the large glands of Peyer, but which, in other cases, occupy different situations, and present forms so various as to lead to reasonable doubts concerning their origin. Sometimes these ulcers are bounded by the cellular texture, the mucous membrane alone having been removed; at other times only the peritoneum remains. The number of these ulcers is also very various; it is rare, however, for it to equal that which is seen when death takes place during the first or second period. The appearance which these ulcers present, although it thus offers numerous varieties, may however be reduced to two principal forms; one which may be called the simple intestinal ulcer, and the other which may be called the intestinal ulcer with hypertrophy of the muscular and cellular coats. In the first, the mucous membrane is white and extremely thin, nor is there any appearance of inflammation, or any other degeneration; the bottom alone presents some very small granulations. The second form is recognized by the prominence of the edges and thickening of the textures, which also exhibit marks of inflammation, and sometimes are so firm as to resemble scirrhus.

Perforation of the intestine.—Sometimes the peritoneum itself becomes involved in the ulceration, after the other textures have been destroyed. In the cases where this accident occurs, the patient is usually seized suddenly, and without obvious cause, with violent pain, followed by symptoms of acute peritonitis, which al-

most invariably leads to a rapid and fatal termination. On examination after death, effusion into the peritoneum is generally found, and the ulcer detected externally; and on examining it from within, the aperture is found to correspond with an ulceration larger than the opening, which seldom surpasses three lines, and is generally only from one to one and a half. These perforations almost always form in the last foot of the length of the ilium: sometimes there are two, and even three such apertures. As to time, their occurrence varies much—perhaps, on the average, from the 15th to the 30th day. The accident is sometimes the result of ulceration, sometimes of a true eschar, and sometimes it is produced by the distention of the intestine, causing the rupture of textures weakened by disease.

Cicatrization of intestinal ulcers.—Hitherto we have followed the changes which the follicles undergo from their first enlargement to their entire destruction. We shall now consider them in a different point of view, and trace their restoration to health. When the patient sinks at a period more advanced than we have yet supposed, such as six weeks or two months, the edges of the ulcers, which we have described as simple, have become so flat, that it is difficult to say where the mucous membrane ends, and the ulcer begins. At the centre of this last, some small inequalities may be seen, particularly when the part is examined under water. When the patients die at a period still more advanced, sometimes no trace of ulceration is found, but occasionally there is only a slight depression, with a perceptible redness at the points usually occupied by such ulcerations.

Resolution of the patches.—In those patients who die at a period more advanced than that at which the patches begin to ulcerate, these last, when they are not ulcerated, usually present a less considerable degree of prominence than in those who have died during the first few days. At a period more advanced, this prominence is still less marked; and we may trace it gradually diminishing, till the patches return by degrees to their primitive state. The period at which this resolution commences varies much, and would require a greater number of cases than I have witnessed to determine it. I have seen the resolution far advanced by the twentieth day, and I have seen it apparently but just commencing after the thirtieth.

Lesion confined to one set of follicles.—The changes which we have been considering, occupy, at the same time, the isolated follicles, and the glands of Peyer; but the same alterations may be limited either to the one or the other of these. Cases of this kind are rare; and I do not believe that any authentic instance of this change having been confined exclusively to the isolated follicles, is to be found in any modern author. It is true, indeed, that if we refer to the plates which are to be found in some of the editions of the work of Rœderer and Wagner, we might believe that the disease which they have described, and the symptoms of which presented in the severer cases a perfect resemblance to those of typhus fever, always gave rise to that variety of ulceration, in which the isolated follicles alone are diseased; but on examining the plates with some care, and comparing them with the text, I am led to think that the drawings had not been made from nature, and that the authors had themselves mistaken the true anatomical character of the disease the history of which they have recorded.

Lesion of the mesenteric ganglia.—The changes in the mesenteric ganglia present numerous varieties. Sometimes they are only increased in bulk, without other appreciable change; sometimes they are also softened; in some I have seen traces of pus, and in others have found the glands indurated, without being enlarged. These changes are (like those in the follicles) dependent upon the period at which the patient sinks. Thus, where the disease is recent, the glands are found to be much enlarged, and very red, or of a rose colour; sometimes, even from an early period, they contain an admixture of pus, giving them a greyish colour. In proportion as they are examined at a more advanced period, this grey tint becomes gradually more pronounced, and passes into a slate or dirty yellow, the matter becoming at the same time more and more attenuated. I have never, however, found it entirely fluid, as in an abscess.

At a still more advanced period, corresponding to that at which the follicles, when they have not been ulcerated, return to their natural state, or else to that at which the ulcers begin to cicatrize, the mesenteric ganglia are less tumefied, often less softened; and in following them to a period far removed from the onset of the disease,—as that when the follicles are again quite restored,—the glands are found to be considerably diminished, and nearly restored to their natural size, but red, or violet coloured, or even black, both externally and internally, without softening; indeed, on the contrary, they are usually indurated. The following table shews the comparative frequency of these different states of the mesenteric ganglia in typhoid fever:—

The ganglia large, with commencing softening and suppuration, from the 7th to the 25th day	14
Idem—very soft, from the 10th to 36th day	12
Idem—filled with a fluid like mucus, on the 21st day	1
Idem—red, large, and hard, after the 19th day	10
Idem—but little enlarged,—blue, violet, or black,—hard, after the 17th day	3
	40

We observe, then, in the gradual alteration of the mesenteric glands, the same series of phenomena as in the follicles—namely, considerable increase of size and softening, which goes on augmenting; then, if the disease takes a favourable turn, a gradual diminution, till their healthy condition be restored. The only difference which we perceive between the lesions of these organs is, that the mesenteric glands never ulcerate, at least I have never seen such a case. In general, the glands which are nearest the cœcum are those in which the change is most marked; the reason of which is, that the follicles are there most diseased, and the two sets of changes are always in proportion to each other. It would appear that the suppuration of the mesenteric glands is not immediately dependent on that of the follicles; that it may exist before the follicles themselves are developed; and that the pus which they contain has not in every case been absorbed, as has been said, from the surface of ulcerations in intestine.

Lesion of the follicles in other diseases than typhus.—One of the most important characteristics of the alterations of the follicles which we have described is, that they are only found as a consequence of typhus fever. It is true that disease of these follicles is met with in some other affections, as cholera, scarlatina, and phthisis; though not so constantly as in fever.

Lesions in Cholera. — In the greater number of those who died in cholera in 1832, the intestinal follicles presented an obvious prominence, sometimes confined to the glands of Brunner alone, at others occupying both those glands and the aggragated follicles. These follicles, the prominence of which never exceeds the fifth or fourth part of a line, are usually of the same colour as the intestine, but of a somewhat deeper tint, because of their thickness; sometimes they are of a dull white, like a thin layer of albumen; sometimes they are uniformly red; at others, of a deep brown. The characters which distinguish this morbid alteration from that which occurs in typhus, are—1st, the difference of the prominence, which is never so considerable in cholera as to lead to the patches which are found being confounded with those which occur in typhus; 2dly, the uniformity of the lesion at all periods of the disease. I have always found the same lesion without any change in cholera, at whatever period the patient may have died—from twenty-four hours to thirty, and even thirty-six days after the commencement. In no case have I found eschars, or ulcerations; nor, consequently, the lesions which succeed to these. This circumstance is of great interest, because it shews how unimportant a part the lesion of the follicles enacts in the different forms which cholera presents at its different periods.

Lesions in Phthisis.—There is more resemblance between the lesion of the follicles in typhus fever and phthisis. In many of those who die at an advanced period of phthisis, we remark sometimes, throughout the whole length of the small intestine, isolated follicles, nearly as large as those which are met with in typhus fever, and, like them, filled with a firm whitish matter; the others ulcerated, with salient edges, from a quarter of a line to a line in thickness. Now thus far there is little difference; but it is rare, in phthisis, to see the isolated follicles alone affected; most frequently the glands of Peyer are so likewise, and present a condition so different from that met with in fever, as to leave no doubt as to the nature of the alteration, at whatever period it is examined. We never see the tuberculous matter spread on the surface of Peyer's glands in such manner as to resemble the patches of typhus; on the contrary, these glands present a small number of tumors, absolutely resembling isolated tuberculous follicles, becoming developed at the distance of one or two lines from each other, generally from the centre towards the circumference; and so slowly that those of the circumference contain only tubercles, when those towards the centre are already completely ulcerated: when, after the ulceration of all the follicles, there remains no trace but one vast ulcer, it might be difficult to distinguish the two conditions under comparison; however, the hardness and thickness of all the tissues which surround a tuberculous ulcer will generally suffice to distinguish it. Besides, the mesenteric glands, at the period we suppose, are in phthisis usually like large white tubercles more or less softened, and cannot be confounded with those of typhus, which at a period equally advanced, are small in size, and red, violet, or black.

Lesion of the follicles in scarlatina.—This differs but little from what has been described above as occurring in cholera. There is usually a slight hypertrophy, with redness of the follicles and glands of Peyer, but which never attains the thickness which we see in typhus, and which never ends in ulceration.

Connexion between the symptoms and the above morbid changes.—After having exposed the symptoms which typhus displays during life, and the lesions which are constantly, or almost constantly, presented after death, we are naturally led to inquire, as far as it is possible to do so, if there exists a constant relation between the principal symptoms of the disease and the different varieties of lesion in the follicles during life. We shall take successively the chief symptoms, and subject them to this examination. The headache, which was only wanting in one out of 42 patients who died, and which was often the first symptom of the disease, can scarcely be looked upon as connected with lesions which it has preceded, neither was any appreciable connexion discovered between its subsidence, and the particular condition of the follicles. The stupor, which is generally developed during the first, or at the commencement of the second period, is a symptom of great importance: it is always united to much prostration of strength, and only disappears when the disease has taken a decidedly favourable turn. Two opinions have been advanced with respect to this symptom, in relation to the intestinal lesions; one, that it did not occur till the patient was already worn out by the long continuance of the disease; the other, that the adynamic state does not come on, except in proportion as the gastro-intestinal affection terminates in ulceration, suppuration, or gangrene. As these opinions have been maintained with talent, and the latter, particularly, apparently even supported by certain facts advanced in its favour, I shall relate the results of my own observation.

Twenty-nine patients out of the forty-two who died shewed a great degree of stupor; of these, twenty-one were in this state when admitted, and eight became affected with it during their sojourn in the hospital. Of twenty-one who were admitted in a state of marked stupor, two were received the fourth day of the disease, three the fifth day, two the seventh day, two the ninth day, two the tenth day, two the twelfth day, four from the twelfth to the sixteenth day, three from the sixteenth to the twenty-second day, one the thirty-sixth day. Again, of those in whom the stupor supervened after their admission, in one the symptoms occurred on the sixth day, in one on the tenth, in two on the eleventh, one on the twelfth, one on the twentieth, one on the twenty-third, one on the twenty ninth. Thus, of twenty-nine patients who had stupor, in four, at least, this phenomenon came on before the period at which the intestinal patches became ulcerated. The doctrine which attributes the stupor, and other adynamic symptoms, to the absorption of pus and debris from the intestines, is not better founded. In four of the above twenty-nine cases, suppuration could not have yet occurred when the stupor came on; and again, in numerous instances where death took place without ulceration and suppuration of the intestinal follicles, stupor had been present. The acute delirium does not appear to be connected with the state of the follicles, any more than the headache and stupor. Of the forty-two patients who died, twenty only had delirium, and the symptom did not appear to be peculiar to any one stage of the disease more than another.

Diarrhœa was present in thirty-four out of forty two, at the time of their admission, and in six only did it supervene during their stay in the hospital.

We may perhaps conclude from these facts, that the varieties of lesion in the follicles and mesenteric glands do not announce themselves by any particular phenomenon; and that the symptoms (except perhaps the purging, pain about the abdomen and borborygmi) are the expressions of the morbid influence upon the entire economy—the disorder which it carries into the principal functions, and that they rather belong to the disease itself, than constitute the effects of the diseased state of the follicles.

SUPERNUMERARY NIPPLES.

The human female presents irregularities of the breast not less frequently than other mammalia. In addition to the instances already on record, I myself have observed the following:—In the year 1824, Maria R., born in Hagenfeld, near Pforzheim, was admitted into our Lying-in Hospital. Her left breast was furnished with two nipples, each of which was enclosed within a separate areola. The supernumerary nipple was situated about three fingers' breadth beneath the other. This breast, on the whole, was not larger than that of the right side. She had a child, and milk came in as great abundance from the supernumerary as from the other nipples.

In the year 1825, the body of a girl was

brought into our dissecting-room: she was about twenty years old, her breasts were beautifully formed, and well developed, but each furnished with two fully-formed nipples, surrounded by a common areola. In 1829, the wife of a Dutch soldier, named Walter, called for the purpose of consulting me; and during the examination of her chest, I observed that the left breast was provided with two nipples; each of which gave milk, *and were surrounded with a common areola.* She assured me that her mother had exactly she same conformation.—*Tiedemann, in Zeitschrift für Physiologie.*

PROVINCIAL MEDICAL AND SURGICAL ASSOCIATION.

The next anniversary meeting is to be held at Birmingham, on the 18th instant. Business will commence at half-past 12 on that day, in the Theatre of the Philosophical Institution. Dr. Conolly is to deliver the annual oration or address,—a duty which we believe was originally allotted to the late Dr. Darwall.

ST. GEORGE'S HOSPITAL.—MR. LANE.

To the Editor of the Medical Gazette.

Sir,

In the last number of the Lancet, the Editor, in commenting on the new appointment of a second assistant-surgeon to St. George's Hospital, states that the other two candidates (Mr. Palmer and Mr. Lane) are "shams."

May I be allowed the opportunity, through the medium of your journal, to set him right on this point, by asserting most positively that I am no "sham" in this affair—that I am opposed to the principle of the appointment—that I voted against it on Friday, the 27th ult. and that I am the only candidate who did so; further, that I am in earnest in the contest, and determined to persevere to the last—I trust with fair grounds of success.

I am sure, sir, you will see the injurious tendency of these remarks to my character and interests at the present moment, if uncontradicted; and therefore feel assured that you will not refuse me the kindness to insert this letter at your earliest opportunity.—I am, sir,

Your most obedient humble servant,

Samuel Lane.

No. 1, Grosvenor-Place,
July 1, 1834.

WEEKLY ACCOUNT of BURIALS,

From Bills of Mortality, *July* 1, 1834.

Abscess	1	Fever, Typhus	1
Age and Debility	33	Gout	2
Apoplexy	4	Heart, diseased	1
Asthma	19	Hernia	1
Cancer	5	Hooping-Cough	12
Childbirth	1	Inflammation	21
Consumption	45	Bowels & Stomach	4
Constipation of the Bowels	1	Brain	5
Convulsions	31	Lungs and Pleura	3
Croup	5	Influenza	1
Dentition or Teething	3	Jaundice	2
Diabetes	2	Liver, diseased	8
Dropsy	13	Measles	3
Dropsy on the Brain	12	Mortification	4
Dropsy on the Chest	1	Rheumatism	1
Epilepsy	3	Small-Pox	2
Erysipelas	1	Spasms	2
Fever	3	Unknown Causes	40
Fever, Scarlet	4	Stillborn	7

Increase of Burials, as compared with the preceding week . . . 48

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

June, 1834.	Thermometer.		Barometer.	
Thursday . 26	from 50	to 75	30·14	to 30·12
Friday . . 27	49	74	30·10	30·06
Saturday . 28	52	79	30·04	30·11
Sunday . . 29	54	80	30·17	30·22
Monday . . 30	51	81	30·26	30·29
July.				
Tuesday . . 1	47	69	30·23	30 11
Wednesday 2	49	74	30·04	30·00

Wind variable, N.E. prevailing.

Rain at times during the morning of the 27th; otherwise generally clear.

Rain fallen, ·1 of an inch.

Charles Henry Adams.

NOTICE.

Mr. Symes.—We have already distinctly declared our intention of having nothing further to do with M. Moscati, or his opponents. Mr. Symes's request, after what we said last week, is idle and absurd.

The printed circular from the Medical and Surgical Society of Newcastle, embodying their address to Mr. Warburton and the Parliamentary Committee, has reached us. It contains several excellent suggestions on the subject of medical reform, with a full account of the Newcastle School of Medicine, and the state of the profession in that town: but we cannot find room for the document.

Errata.—In Mr. C. Hutchison's letter, p. 445, last paragraph, *for* "statement," *read* "subject;" *after* "Mr. Lawrence's," *read* "error;" and in the note, col. 2, *for* "Mr. Crampton," *read* "Dr. P. Crampton."

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SATURDAY, JULY 12, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XL.

PRETERNATURAL LABOUR.

DIFFICULT BREECH PRESENTATIONS.

Since we have learned the mechanism of head and breech presentations, and the difficulties that are sometimes met with under natural labour, we may readily suppose that delays will also occur, and that impediments will exist to the easy passage of the fœtus, when the nates, or any part of the lower extremities, offer at the pelvic brim.

CAUSES, AND CORRESPONDENT TREATMENT.

We may readily imagine, that all the causes referable to the mother, which have been before described as producing delay under natural labour, may equally occasion difficulty when the breech presents; and these may all be referred to one of the two general heads—either inefficiency of the propelling powers, or a diminution of space in the passages.

INEFFICIENT UTERINE ACTION.

It is not unfrequently remarked, that under breech presentations, after the rupture of the membranes, the uterus for some time acts more feebly than in natural labour; and this is perhaps owing to the breech producing less pressure or irritation upon the os uteri than the harder head: but the contractions presently become sufficiently strong; and when the pelvic cavity is pretty well occupied by the fœtal body, and the perineum somewhat on the stretch, the pains are fully as powerful as when the head is passing. Such cases, then, require no artificial assistance; nor is it necessary or desirable to stimulate the uterus to increased action. But should the patient have been debilitated by previous disease, worn down by excessive discharges, be of relaxed fibre, or have borne a great many children, we may anticipate a necessity for some extractive aid. Should, then, this sluggishness on the part of the propelling powers continue, while at the same time the pelvis possesses the ordinary capacity, and the soft structures have acquired a due degree of softness and distensibility, it is right that we should endeavour, by the means already mentioned, to increase the tone of the uterus, and supersede the necessity for manual interference;—such are, warm diluents, taken internally; gentle friction, with slight pressure, over the abdomen; change of posture; and—should the arterial system also be acting with diminished energy—we may have recourse to stimuli. The ergot of rye, as a general rule, is perhaps inadmissible; but if it be exhibited, the cautions I before gave must not be lost sight of.

Presuming, then, that we are in attendance upon a case in which such circumstances obtain as I just hinted at, and the means recommended have not the desired effect, it next becomes a question whether we are warranted in resorting to artificial delivery. To answer this question, many circumstances must be taken into consideration, which have been before sufficiently dwelt on; but since delivery under a breech presentation is for the most part easier than when the head offers, I should be inclined to use the means we are in possession of rather earlier than if the presentation were natural; provided, indeed, no risk would be incurred of injuring the maternal structures. The method to be adopted will

depend much on the situation of the breech: if it be somewhat low in the pelvis, our finger will in most instances be sufficient for our purpose; by hooking it over the groin, and the application of a little traction, we may probably bring the breech to press upon the perineum; we shall then most likely find that uterine action is increased in proportion as the perineum becomes distended; and that no further artificial aid is required. But should the breech be so high that we are unable to insinuate our finger round the groin, we may, by another very simple means, produce a most valuable and efficient purchase: we may possibly be able to work the extremity of a silk or cambric handkerchief over the groin, without any great difficulty: by drawing the end down, a loop is formed round the fœtal limb, and a most powerful hold is obtained. If the handkerchief be used carefully and tenderly, it is preferable to an iron hook; but should the application of the handkerchief be difficult or impracticable, we have another instrument,—more efficient, perhaps, but more dangerous,—in the blunt hook, to be used only as a last resource. If the breech have entered in any degree into the cavity of the pelvis, we can generally succeed in encompassing one or other limb by a small-sized blunt hook. Having, then, warmed and greased the instrument, we carry its handle up towards the abdomen, in the manner I now show you, and introduce the point, directed by our finger, previously inserted, over the bend of one thigh; gentle traction must then be made in the direction towards the coccyx, and we shall most probably find the fœtal body descend. I need not, I am persuaded, again remind you of the delicacy of the child's structures, and the imperfectly-ossified state of the bones; and I need not warn you of the grievous injuries you may inflict on the infant's person.

In the cases now under consideration, we have been recommended by some practitioners (and by Dr. Hamilton, of Edinburgh, among the number) to adapt the forceps over each ilium of the child—if the os uteri be dilated—and to extract by a movement similar to that used when they are applied upon the head. Baudelocque, indeed, tells us that this mode of delivery was first accomplished by a practitioner who mistook the breech for the head, adapted the forceps, and afterwards boasted of his novel success. You cannot, however, fail being immediately impressed with the numerous objections to this mode of proceeding.

The instrument not being made for the breech, but for a more globular body, does not fit that part, and is liable to slip, to the great hazard of the mother's structures. The only way, indeed, by which we can cause the blades to keep their hold at all, is by squeezing the handles firmly together, so that the points may take a deep nip upon the fœtal body; and, as anatomists, I would ask you whether there were no parts likely to suffer from this rude pressure? We may possibly break the fœtal pelvis; and that would be an accident of no trifling importance. We may also do irreparable injury to some of the soft parts. You are aware of the large size of the liver in the fœtus—you know how closely the edge approaches to the pelvic brim—you are acquainted with the dangerous consequences of a bruise or rupture of that organ in the adult,—and you would naturally conclude that the result of such an accident would be equally, if not more serious, in a young child; because the structures are so much more vascular and tender. Independently of these organs, the intestines may be dangerously wounded, as you cannot fail to observe when the instrument is applied to this leather phantom. For such reasons, then, I would strongly advise your not attempting to use the forceps in breech presentations; but there is another, not to be forgotten—that we have better instruments in those I have already mentioned. Hamilton himself, indeed, in his lectures, states, that injury can scarcely be prevented if the forceps in more common use are employed, but that his are not liable to the same objections; that he never had recourse to the blunt hook but twice, and that in both instances the child's groin was seriously hurt. Notwithstanding the professor's eulogium on his own instrument, I am still inclined to the opinion that any variety of forceps is perfectly inadmissible: and that the means recommended, particularly the blunt hook, are equally applicable in cases of impaction of the breech,—which we sometimes meet with,—as when it is detained in the pelvis simply from want of sufficient energy in the expulsive agents.

DISTORTION OF THE PELVIS.

We will presume, however, that the cause of delay is a want of room in the bony pelvis, and that the diminution of space is at the brim, in the conjugate diameter; where, indeed, we usually observe it to exist. It is evident that such a case must be difficult in proportion as the pelvis is contracted; and we shall sometimes, as in head presentations, find that difficulty almost insurmountable.

Our first duty, under such a state of things, is to detect the cause of delay; and we can have little difficulty in determining this point, for the uterus will most likely be acting sufficiently strongly to pro-

pel the breech through the brim, if there were space enough to admit it; and we can positively measure the dimensions of the pelvis as easily as though the head presented, and by the same means. The only question is as to the space necessary for the transmission of the breech. Now this part of the fœtal body does not possess a circumference so large as the head, and, being softer, it is more compressible; so that it may be squeezed through a smaller aperture than the cranium, while whole, would require: at the same time, however, since it posseses no cavity which can be opened, and no contents which can be evacuated, it is impossible to draw it through so small a pelvis as the mutilated and collapsed skull. If, indeed, the pelvis measure but two inches and a half from pubes to sacrum, I am persuaded the double breech may, by management, be made to pass, and that it could be extracted through a considerably less space, provided the legs were first brought externally.

But, even if we succeed with the breech, a larger space is required for the shoulders; and if they pass, still there is more room necessary for the passage of the head, so that we have difficulty following difficulty, and each of them greater than the one preceding. This, indeed, is just the reverse of what happens under a head presentation; for, generally speaking, when the head is born, the body can be extracted with comparative ease.

Being, then, fully satisfied that the breech presents; having learned that the pelvis is malformed or small, the woman having been some hours in strong labour; perceiving that there is a chance of her sinking under the continued struggles, unless she is assisted,—we are fully warranted in offering relief by the means I have already stated. It is not, indeed, necessary to wait until the os uteri is entirely dilated, because the breech may be extracted through the pelvic brim before full dilatation has taken place, provided the organ be soft and distensible. It is our duty, in all instances, to endeavour to extract the child without injury to its person; but if the diminution of space be great, we can scarcely expect that it will pass alive, because we know that if it be at the full time, and well ossified, we shall be obliged to evacuate its brain before the head can be born; nevertheless, since it may be smaller or less ossified than usual, we must be most careful to prevent injury to its limbs.

A gentle swaying motion from side to side will facilitate the escape of the body, which being born, one shoulder must be turned into the hollow of the sacrum, and the other brought underneath the pubes. The arms must be extracted in the manner recommended at our last meeting, and the head must be brought to the pelvic brim, in the situation most favourable for its passage; namely, with the face to one ilium, and the occiput to the other. We must then pass the finger of one hand into the mouth, depress the chin, and make traction by the two first fingers of the other, fitted like a crutch across the shoulders; taking care not to dislocate the neck or injure the jaw. By this means we shall probably enable the head to pass the brim; and when it has entered the pelvic cavity, we may turn the face into the hollow of the sacrum, and we shall probably complete the delivery with little difficulty, because the principal impediment is overcome.

When we are endeavouring to bring the child's head through the brim of the pelvis, whether the organ be contracted in its dimensions or of a natural formation, it is of the utmost importance that we should recollect its relative diameters; for if, even where they are of the usual size, we place the head with its long diameter in the situation of the short diameter of the brim, the cranium will not enter the cavity, but will remain above the brim, and the child's life will eventually be destroyed by pressure on the funis umbilicalis. I have been called to many cases under the belief that there was a diminution in capacity, where the person in charge, — not sufficiently attending to the different diameters of the pelvis, and the relative form of the fœtal skull, — had officiously interfered with the child's progress, and had brought the chin forcibly against the promontory of the sacrum, or behind the symphysis pubis. Thus the very means which had been employed to effect delivery were exactly those that retarded the birth; because, if nature had been allowed to expel the head unaided, she would have directed the face to one side, until the brim was cleared, and afterwards back into the hollow of the sacrum. By far the greater number of such difficulties, indeed, I have seen occur under the hands of females, who, not sufficiently informed of the anatomy of the pelvis, and its relative proportions, considered that the child was only to be extracted by main force; and unable to appreciate the value of the different positions requisite for its easy transit, used strong extractive efforts continuously in one direction, and thus produced the mischief so cautiously to be avoided. In the generality of these cases, by simply inclining the face to one side, the difficulty has vanished, the head has descended, and almost immediately been expelled. The turning of the head into the most favourable situation for its passage through the brim is one of the most important points in the

management of a breech case, but it is of all others, perhaps, the most frequently neglected.

Smellie, in one of his plates, as you may observe, delineates the forceps applied over each side of the child's head, when it has been brought to the brim in this improper position, and recommends that the necessary turn should be effected by their agency. I cannot think the application of the instrument, under such circumstances, ever necessary,—nor indeed could it be easily accomplished;—for by introducing the finger into the child's mouth, depressing the chin upon the chest, and turning the face to one side, we shall most probably accomplish our object; unless, indeed, the head be very strongly compressed by the contracted uterus—very firmly jammed by previous attempts at extraction—or the pelvis be of a diminished capacity.

If, then, we have placed the head in this most favourable situation, and made use of as much exertion as we think ourselves warranted in doing, for the space of fifteen or twenty minutes, without the expected success, we shall be compelled to diminish its bulk for the purpose of accomplishing extraction; and the operation is not much more difficult than if the head had originally presented.

The same deadly instruments are required for perfecting this intention. The cranium must be perforated, and the brain partially evacuated; but we do not feel so much compunction in having recourse to this measure as when the head presents naturally, because the child must be dead before the operation can be required. No person would think of perforating the skull before some considerable efforts had been made to extract it entire; and under those efforts, the great chance is that the pressure on the funis would have been such as to destroy the fœtal life. On some occasions I have witnessed the gradual death of the infant from this cause, while I was unable to prevent it, or advance succour; and in others I have delayed applying the destructive means until the vital spark had flown: shrinking from being myself the instrument of death, but choosing rather,—however sad the alternative,—to wait quietly until I was assured the heart's last pulse had throbbed.

Mode of performing the operation.—The woman lying on her left side, an assistant must bring the child's head down as low as possible, and turn the neck upwards, under the symphysis pubis, so that one acromion is towards the *mons veneris*, and the other towards the *fourchette*. An unoccupied space at the back part of the pelvis is thus procured, into which we can insinuate two or three fingers of the left hand with ease; they must be carried up against the skull, to the projection behind that ear which is next the sacrum. Along these two fingers a perforator must be passed, and making steady pressure against the part, with a semi-rotatory motion we introduce its point within the skull as far as the rests; the two handles of the instrument must then be separated by an assistant, in the same way that I recommended before, and a crucial incision made, if practicable. Having made an aperture sufficiently large to admit the instrument fully within the cranium, we break down the brain as perfectly as possible, and commence extraction. We seldom require to use an extracting instrument, since the means of traction is afforded us by the body of the fœtus itself; but if it should be requisite, we can fix the crotchet on the inner surface of the bone, and a very firm purchase is afforded us, because of the strength of the cranium at this part. It may possibly slip, or break away from its hold, when another point of resistance must be sought for; and in our efforts we must be most assiduous in guarding the extremity of the instrument by our finger, to prevent laceration of the os uteri or vagina. If possible, let us avoid an extracting instrument; but if any be required, the crotchet appears to me much the most applicable.

There is certainly more difficulty in perforating the skull behind the ear, than when the vertex presents; and that for three reasons. In the first place, the vagina being partly occupied by the neck, rather impedes our movements. Secondly, the bones at the base of the skull are thicker, and consequently we must use more exertion in perforating them with the point of the instrument. And, thirdly, the point of the instrument is more liable to slip to one side—to run up between the bone and the scalp, and not to enter the skull at all. Such an occurrence is easily known, by the facility with which it passes up to the rests, and the very slight resistance which is remarked; and also by examining the laceration we have made by our finger, after the withdrawal of the instrument. If we find no jagged edge of bone, it is merely the scalp that is punctured, and we must make another attempt, by turning the extremity of the instrument a little more in the direction of the centre of the cavity of the skull. Sometimes, indeed, the perforator will slip in the same manner between the skull and the scalp, when applied to the vertex under a head presentation; and I ought to have mentioned this, when speaking before of the operation of craniotomy, as one of the embarrassments we occasionally meet with; but this mischance is not so likely to occur

when the head presents, as in the case now under consideration, because we have then a better opportunity of directing the point against the spot most dependent, and the bones at the upper part of the cranium not being so resistant, yield more readily.

If you cast only a cursory glance at some of the pelves on the table, you will immediately suppose that it would be perfectly impossible for the breech to pass through them, even when diminished to the utmost extent it is capable of,—by the legs having been first extracted,—and compressed into as small a space as the semi-ossified structures would allow. Under such an aggravated state of disproportion, one alternative alone is offered us,—that of performing the Cæsarean section; and the only question to be considered is, the size of the pelvis under which we are warranted in having recourse to this terrible expedient. Somewhat more room, then, would be required for the transmission of the body and shoulders, under a breech presentation, than when the head offers at the brim; and, provided the conjugate diameter measures less than one inch and three quarters, I should think myself justified in proposing the abdominal incision. It certainly never occurred to myself to meet with a case in which the breech would not pass by the use of the means before recommended: such instances, however, are on record, and might happen to any of us. Even should we succeed, after much exertion, in extracting the body and shoulders of the child through a pelvis less than the dimensions I have just noticed, still it appears to me that the head, in this position, would require considerably more space, after perforation was effected, than when the vertex presented; and on this account also I should be inclined not to attempt delivery *per vias naturales*, unless there existed nearly one inch and three quarters.

PELVIC TUMORS.

Other causes than distortion of the pelvis may occasion a want of the necessary space for the passage of the breech: thus, tumors may have formed in the pelvis, such as I have before mentioned—exostosis, diseased ovaries, scirrhous and suppurating glands, polypi, and some others; and there are no specific rules which we can apply to breech presentations, under these deviations and difficulties, that are not applicable also to cases in which the head presents. Our indications are exactly the same: we save the child if we can, but not at the expense either of the mother's life or of extensive injuries to her person.

If the tumor possess distinct fluctuation, whether it be a suppurating gland or enlarged ovary, let us puncture it. If there be a polypus in the pelvis, impeding the passage of the child's breech, body, or head, let us remove it, provided that can be done without danger to the mother; but if the tumor be hard and immoveable, so that we cannot lessen its bulk, and fear to dissect it away from its attachments, we must act upon the common principles which I have so frequently laid down—wait for some time, in the hope that nature may overcome the impediment; and if she fail, traction must be made with the finger or blunt hook surrounding the groin, and the case managed as just directed.

RIGIDITY

of the os uteri, vagina, and perineum, singly or combined, may occasion difficulty, as we have noticed when speaking of lingering labour.

Under this complication, the os uteri may probably be relaxed by bleeding, by enemata, by the injection of warm oil or mucilaginous fluids into the vagina, and we may possibly deem it necessary to exhibit opium: and the vagina and perineum may be softened perhaps by artificial lubrication and external fomentations. Failing in these means, delivery must be resorted to, by measures already, I trust, sufficiently explained.

HEAD LEFT IN UTERO.

In the ages of rude surgery it has not unfrequently occurred that the head has been separated at the neck by violent and ill-directed efforts, and left in utero after the extraction of the rest of the body; but to meet with such a case is now rare. The only instance in which this accident came under my own treatment happened in the practice of a midwife attached to a charity of which I have the charge. The child was putrid, and she had been attempting to extract it without reference to the propriety of its position. When I arrived, I found the chin hitched upon the sacral promontory, the vertebræ entirely separated, and the cranium attached to the body by a very small portion of integument. There was a tumor in the pelvis, which possessed the characteristics of an enlarged ovary. Not desirous of encountering these difficulties alone, I requested my father's assistance, who promptly attended. Having introduced his hand into the uterus, he changed the position of the head, so that the crown came to the pelvic brim, and perforated it at the sagittal suture, while I steadied the uterine tumor externally. We had then little trouble in extraction; and this method appears to me the most likely to succeed of any which has been practised: but under such anomalous cases no rule can be laid down for universal, and scarcely for general guidance. I once

heard a highly-revered and much-lamented physician, lately deceased, state in a public medical meeting, that in the early part of his practice it had come within his knowledge, that a patient had been sent from Dublin to London, for the purpose of having the head of a child extracted from the uterus, which—together with the placenta—had been left in that cavity after the body had been drawn away; and he instanced the fact as a proof of the violence which the womb will sometimes bear with impunity. Our brethren in Dublin in the present day are too well acquainted both with the mechanism of parturition under its infinite varieties, and with the appropriate application of artificial assistance, to commit such a blunder as leave a head in utero; and a great deal too much good sense to send the patient to our metropolis for relief, if such a case should come under their observation.

PHYSIOLOGICAL AND PATHOLOGICAL OBSERVATIONS ON THE BLOOD AND URINE.

Being the Gulstonian Lectures for 1834, delivered at the Royal College of Physicians.

By Geo. Burrows, M.D.
Fellow of Caius College, Cambridge.

Lecture I.—*Sketch of the rise and fall of the humoral pathology in England—Injections into the veins by Sir Christopher Wren—Robert Boyle's experiments on fresh-drawn blood with acids and salts—Boerhaave's opinions on the use of alkaline and non-purgative salts—Huxham's saline plan of treatment of malignant fevers—Cullen's attack on the humoral pathology strengthened by the contemporaneous improvements in morbid anatomy—Advance of animal chemistry since the time of Cullen—Attention of the medical public called to the pathology of the blood, by the work of Dr. Stevens—Coincidence of the statements of Boyle, Fracassati, Huxham, Haller, Pringle, and Hewson, with the opinions of Dr. Stevens—Action of mineral and vegetable acids, of alkalies, alkaline, and neutral salts, on fresh-drawn venous blood, exhibited by experiments.*

Sir,

Dr. Theodore Gulston, to whom this College is indebted for the endowment of the lectureship I have this year the honour to hold, directed in his will that these lectures should be pathological, and "upon some dead body (if possible by any means such a body could be procured.)"

I have ventured so far to deviate from the intentions of our benefactor, as to confine my observations to a part of the human body, and that part one of the most important, in health and disease, of all the various components of the whole—I mean the blood, which, as John Hunter said, "is more expressive of disease than any other part of the animal economy*."

But here, sir, I must claim some indulgence from yourself and my audience, that these lectures are not illustrated by so many original facts and experiments as I could have desired, on the ground that I have, from unavoidable circumstances, undertaken these lectures at a much shorter notice than is usually given to those appointed to deliver them.

Before I enter upon the practical part of my subject, I wish to take a short survey of the rise and fall of the humoral pathology in England. I shall state the causes which threw the consideration of the state of the blood and other animal fluids, in diseases, into neglect, and the reasons why pathologists of late years have so exclusively considered the solids of the body to be the seats of diseases. I shall endeavour to bring before you some reasons for advocating a more careful consideration of the state of the blood and other animal fluids, in diseases, founded upon the changes produced in the blood and secretions by various internal remedies, and upon some recent observations which lead pathologists to suppose that the blood itself is capable of undergoing great morbid changes, and, under some peculiar circumstances, of being converted into the most malignant structures which infest the human body.

After many ages of darkness, during which almost the whole of ancient literature had perished, learning again revived in the fifteenth century. The physicians of that period almost exclusively turned their attention to the works of Galen. Linacre and Caius, two of the brightest ornaments of our College during the sixteenth century, translated the works of this celebrated physician from their original Greek, into Latin, and thus made them accessible to the great body of English physicians.

It is well known that the explanations Galen attempted to give of the phenomena of health and disease, rested entirely upon the state of the fluids of the body; and during the sixteenth, and first half of the seventeenth century, physicians were almost wholly employed in explaining and confirming the humoral pathology.

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* Hunter on the Blood, p. 77.

The discovery of the circulation of the blood by our immortal countryman and honoured colleague, William Harvey, towards the middle of the seventeenth century (exactly at the time when Dr. Gulston founded this lectureship), directed men's minds more forcibly to the humoral pathology—to the importance of the blood both as a cause of health and disease—and likewise to the facilities the circulation of the blood afforded to the introduction of remedies into the animal body, through the medium of the vessels of the circulation. Thus we find as early as the year 1659, that Christopher Wren, Savilian Professor at Oxford, proposed to the Hon. Robert Boyle to inject substances into the veins of dogs. In 1665 he read a paper to the Royal Society—"An Account of the Rise and Attempts of a Way to convey Liquors immediately into the Mass of the Blood*." In this paper he states that, by means of quills attached to bladders, he injected solutions of opium and crocus metallorum into the veins of dogs; and he adds, "the opium being circulated into the brain, did within a short time stupify, though not kill, the dog; but a large dose of crocus metallorum made another dog vomit up life and all."

These experiments are detailed more at length in Mr. Boyle's "Usefulness of Experimental Philosophy" (Part II. Essay ii. p. 53), and, through the Philosophical Transactions, soon excited the attention of continental physiologists. Fracassati, Professor of Anatomy at Pisa, soon afterwards injected into the crural and jugular veins of dogs different liquids. He states† that, 1st, diluted aquafortis, injected, caused death very quickly, and the blood was found coagulated throughout the large vessels.—2d, Diluted sulphuric acid, when injected, did not kill so quickly; but after death the blood was found coagulated, and dark as soot.—3d, When "oil of tartar‡" was injected, death ensued; but the spectators found, to their surprise, that the blood was not curdled, but, on the contrary, "more thin and florid than ordinary; which seems to hint," he adds, "that a too great fluidity of the blood, as well as its coagulation, may cause death."

Dr. Fabritius, of Dantzig, and many other experimentalists, now commenced the injection of purgative and other medicines into the veins, and observed the usual effects of the remedies upon various organs precisely the same as when taken into the stomach. Subsequent experiments have been made, fully confirming the statements of these writers, and affording one among many other proofs that it is through the blood, as a vehicle, that numerous remedial agents taken into the stomach act upon distant organs.

Emboldened by these successful efforts to introduce remedies directly into the system, Dr. Edmund King ventured to try the effects of the transfusion of blood from animal to animal; and this he accomplished without inconvenience to either animal.

In conjunction with the celebrated anatomist, Dr. Lower*, he proceeded, at Oxford, to transfuse the blood of animals into men, and the most extravagant expectations were formed of this method of infusing health and vigour into the diseased and decrepid. Humoral pathology was now at its meridian height, but was soon to receive a fatal blow from the rashness with which transfusion was practised in Paris. The most frightful and fatal consequences ensued upon the transfusion of large quantities of blood from animals into men, and the government of France forbade the further employment of such dangerous means of treating diseases; and from similar accidents the practice soon fell into disuse in our own country.

In the hands of modern practitioners, with improved apparatus and due caution, the transfusion of blood from person to person has been practised with success; and no doubt in extreme cases it is a powerful and valuable remedy.

But previous to these experiments of the injection of remedies into the veins, and of the transfusion of blood from animal to animal, and from man to man, the Hon. Robert Boyle† had made some experiments upon the human blood, when fresh drawn from the veins, which appear to me to contain the germs of much which has recently been advanced as new, of the effects of various acids and salts on the blood. "I had formerly made," (Dec. 1664), says Boyle, "an odd experiment upon blood yet warm as it came from the animal, viz. that by putting into it a little aquafortis, oil of vitriol, or spirit of salt, the blood would not only instantly lose its pure colour, and become of a dirty one, but in a trice be also coagulated; whereas if some fine urinous spirit, abounding in volatile salt, such as the spirit of sal ammoniac, were mingled with warm blood, it would not only not curdle it, or imbase its colour, but make it look rather more florid than before, and both keep it fluid and preserve it from putrefaction for a long time."

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* Phil. Trans. vol. i. p 128.
† Ibid, 1667, p. 489.
‡ Potassæ subcarbonas.

* Phil. Trans. 1666, p. 353.
† Ibid, 1667, p. 551.

Allowing for the imperfect state of chemical science in the time of Boyle, it appears to me that he has here announced the facts, that acids blacken and coagulate the blood, while saline fluids, on the contrary, render it more florid, and keep it fluid.

The transcendant discoveries of Newton in natural philosophy, at the close of the seventeenth century, turned the attention of all inquirers into the phenomena of nature, from physiology to physics; and we find the physiologists of this period almost entirely engaged in applying the laws of motion established by that great genius to the calculation of the velocity of the blood, of the force of the heart, and other mechanical problems.

Pathologists, however, still explained the phenomena of disease on the principles of the humoral pathology. The physicians from all parts of Europe crowded to hear the lectures of the celebrated Boerhaave, at Leyden; and from him imbibed the humoral doctrines. Haller carried these with him to Göttingen, Linnæus to Upsal; and many of the Scotch professors disseminated the same opinions in Edinburgh, and other universities of Scotland. Boerhaave frequently employed the supposition of an acrimony, or alkalescence, of the animal fluids, to explain the phenomena of disease; and although his doctrines on this subject may be said to have been hypothetical, because chemistry could, in his time, give no correct analysis of animal secretions, still he frequently made the most judicious use* of acids and alkalies, to correct the supposed opposite states of the blood; and in the treatment of ardent and malignant fevers, he expressly directs the use of various neutral salts.

Boerhaave, in the "Institutiones Medicæ," and chapter "Humorum Morbi," writes thus:—

"(730) Et sunt sane hi morbi maximi quidem momenti in scientiâ medicâ, et intellectus eorum basis certe optimæ pathologiæ: intelligi tamen vix queunt, nisi accuratissimâ observatione suorum effectuum quos in corporibus ægris præstant.

"(731) Si autem spectaveris humores totos simul, deprehendes morbos eorum præcipuos esse, vel nimiam fluiditatem, vel nimiam tenacitatem, tum excedentem horum motum per vasa, aut quietem eorundem nimiam," &c.

And in another part of the same chapter he writes, "Spissitudo autem si peccat, exigit attenuationem: hæc procuratur per solida resolventia, ut sunt alcalini fixi, volatiles sales." And again, "Materies in loco nimis tenax mobilis et meabilis redditur, (1) per diluentia, (2) *per salina resolventia* similiter apposita, ut nitrum, sal prunellæ, sal polychrestus*, sal marinus, *sales regetantium ustulatorum*, *sales alcalini*, &c.; so that in malignant fevers, where the blood is found thick like pitch, as it is in the epidemic cholera, and the fevers described by Dr. Stevens as occurring under his observation in the West Indies, Boerhaave would have employed the muriate of soda, nitrate of potash, and other non-purgative salts, which have been so strongly recommended by that physician in his late work on the Blood.

The humoral pathology of Boerhaave appears to have maintained an undisputed sway in the University of Edinburgh, and through the professors of that school, in the minds of the great body of physicians in Great Britain and her colonies, up to the time of Cullen. We find the best practical writers of that date still advocating these doctrines, and among others Huxham. This author, in his treatise "On the Ulcerous Sore Throat of 1752," states that that epidemic† "was undoubtedly a fever of the malignant pestilential kind, in which the blood became highly acrimonious, dissolved, and putrescent;" and that‡ "he found the blood florid truly, but so soft and loose that you might cut it with a feather, giving off little or no serum, but having exactly such an appearance as when spirits of hartshorn is poured to the blood, just as it runs from the vein, and which prevents its natural coagulation." In the beginning of such malignant fevers, Huxham§ "often met with a buffy, sizy appearance of the blood; and yet, blood drawn two or three days after from the very same persons hath been quite loose, dissolved, and sanious as it were." He goes on then to the subject of the treatment, warning his readers "that when there is just reason to fear a contagious malignity in a fever, we should proceed with the utmost caution as to repeated bleeding: it will be found that the pulse and strength sink surprisingly after the second and third bleeding."

In such malignant fevers, with an altered state of the blood, Huxham informs us that he‖ immediately put his patient on a saline plan of treatment. He employed a mixture of the salt of wormwood¶, or volatile salt of hartshorn, and juice of lemon and nitre; and, lastly, his famous tincture of Cinchona bark.

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* Aphorismi de Cognoscendis et Curandis Morbis, Hermanni Boerhaave. Cap. Morbi ex Alkalino Spontaneo. Aphorismi 82, 86, 88.

* Sulphate of potash, and sulphuret of potash.
† Page 42.
‡ Page 35.
§ Huxham on Sore Throat, p. 36.
‖ Ibid, p. 40.
¶ Subcarb. Potassæ; Subcarb. Ammoniæ.

From these quotations, I think it apparent that the theory,—that malignant fevers arise from an altered condition of the blood, and that the appropriate treatment is by the non-purgative neutral salts, is due to Huxham, and that modern writers on this subject have not given him credit for his theory, or else have been ignorant of his writings.

The doctrines of Boerhaave, Huxham, and other humoral pathologists of the last century, met with a most powerful and successful opponent in Cullen. This great man, whose theories so long reigned triumphant in many of the schools of Europe, was himself educated in the doctrines of Boerhaave. In the preface to the Practice of Physic, published by Cullen in 1783, he has put forth his reasons for abandoning the humoral pathology of Boerhaave, and of following up the theories of Hoffman, professor in the University of Halle, upon the "spasmus" and "atonia" of the solids.

Cullen adopted an opinion of Hoffman's, as the ground-work of his Pathology, the very reverse of that maintained in the Aphorisms of Boerhaave. He thus quotes Hoffman's words:—"Hinc universa pathologia longe rectius atque facilius *ex vitio motuum microcosmicorum in solidis*, quam *ex variis affectionibus vitiosorum humorum*, deduci atque explicari possit, adeoque omnis generis ægritudines internæ, ad præternaturales generis nervosi affectiones sint referendæ*."

This sentence contains the key-stone to the whole fabric built up by Cullen, and so ably supported by him in his writings and lectures; but he could not refrain from confessing "that the fluids of the human body suffer various morbid changes, and that upon these diseases *may* primarily depend; yet I must beg leave to maintain," he adds, "that the nature of these changes is seldom understood, and more seldom still is it known when they have taken place: our reasonings concerning them have been purely hypothetical, have therefore contributed nothing to improve, and have often misled the practice of physic: they have withdrawn our attention from the *motions* of the animal system, upon the state of which the phenomena of disease do more certainly and generally depend."

This attack of Cullen on the pathology of the blood and other animal fluids accidentally derived additional force from the great advances made by morbid anatomy during the latter half of the last century. The splendid work of Morgagni, "De Sedibus et Causis;" the "Sepulchretum" of Bonetus; the "Anatomie Médicale" of Portal; the "Morbid Anatomy" of Baillie, were studied and justly admired in every medical school of Europe. That philosophical and most captivating work, the "Anatomie Générale," of Bichat, with its pathological doctrines founded on the properties of the various tissues of the body, decided the opinion of the medical public in favour of the solidists.

During the last fifty years, the causes of diseases have been looked for in the alterations of the structure and other physical properties of the solids. The scalpel, the injecting syringe, and the microscope, have all been employed with astonishing success in unfolding to the eye of the anatomist the structure of morbid parts. Vast collections of morbid anatomy have been made in this and other countries. Perhaps there is scarcely any kind of morbid structure that ever occurred in the human body, of which some specimen may not be found as a preparation in some museum of Europe.

The pathology of the fluids has been almost neglected during this period; and while every form of diseased structure is shown by preparations, models, and drawings, to the student of medicine, the healthy and morbid conditions of the blood and animal fluids are very slightly dwelt upon.

I trust that these observations will not be misconstrued into any thing like a contempt for the services rendered to pathology by the morbid anatomist. I am far from entertaining such an opinion. Morbid anatomy is the right hand of pathology; animal chemistry is the left. All I mean is, that the right hand has been employed to the exclusion of the left, and those parts of the work which are most unfinished must be accomplished by the agency of this left hand.

Cullen, having exposed the errors of the humoral pathologists, it was natural that he and his scholars should pass into the opposite extreme, and overlook altogether the morbid conditions of which the animal fluids, either as the effects or the causes of disease, are undoubtedly sometimes the seat. I must here profess my entire concurrence in the opinion expressed by Dr. Watson, in the Lumleian Lectures of 1832, and by Dr. Roupell, in the Croonian of the same year,—"That it is to a more exact acquaintance with the chemical changes, to which the animal fluids are subject in disease, that we are chiefly to look for the future advancement of physic as a science."

Cullen admits that Boerhaave gave a more correct account of the acid and alkaline states of the animal fluids than any that had been given before; but, after all, that he had done it very imperfectly. "We have, indeed, since Boerhaave's

* Medicina Rationalis Systematica, tom. iii. chap. 4.

time," says Cullen, "acquired more knowledge upon the subject of digestion, and so much as to know that a great deal more is yet necessary to enable us to understand in what manner the animal fluids are formed from the aliments taken in."

It appears, then, that the pathology of the fluids was thrown into neglect by Cullen, on account of the then imperfect state of animal chemistry, and of our knowledge of the digestive function. But if we turn our attention to the powers of analysis, put into our hands by Sir Humphry Davy, in the adaptation of galvanism to the decomposition of so many bodies hitherto regarded as simple; if we peruse the valuable and instructive papers of Berzelius on various parts of animal chemistry; if we regard the labours of Marcet, Wollaston, Prout, and Bostock, we cannot hesitate to admit that animal chemistry has made giant strides since the time of Cullen.

Perhaps we are more deeply indebted to Dr. Prout than to any other of the abovementioned contributors to this branch of science, on account of the practical utility of his researches: he has enabled us to make accurate analyses of the urine in health and disease, and following out and amplifying the views put forth by Sir Gilbert Blane*, has prescribed admirable rules for the treatment of diseases of the urinary organs.

This application of chemistry to the investigation and treatment of some of the most painful diseases, must be regarded as a very signal triumph for that science, and holds out great encouragement to persevere in the same path of inquiry.

Cullen's second objection to humoral pathology, that "a great deal more is yet necessary to enable us to understand the subject of digestion, and in what manner the animal fluids are formed from the aliments taken in," is very much diminished in force by the inquiries and reasoning of the same ingenious philosopher†. His views upon the subject of digestion, made known to the public from this chair, and since then more fully developed by him in his Chapters on the Chemistry of Organization, of the Bridgewater Treatise, seem to promise a deeper insight into this important function than any others that have been offered to physiologists. His theory, at least, offers a probable explanation of the manner in which our food may be reduced to a state in which it may enter the lacteal vessels.

We must not pass over the valuable services rendered to animal chemistry in the series of experiments of Dr. Wilson Philip, on the function of digestion, proving that galvanism possesses a power capable of stimulating the stomach to go through the digestive process, as well as nervous energy itself, and of artificially supplying the one power when experience tells us the other is deficient.

Those who are familiar with the works of Orfila, Christison, and other distinguished toxicologists, must be aware that the pathology of the blood, and other animal fluids, is daily increasing in interest, and that some of our most energetic medicines, as well as some of the most deleterious poisons, are now discovered, by a careful analysis, to have passed from the stomach into the vital current, and are, at one and the same moment, exerting their influence upon every particle of our bodies.

In analysis, chemistry is daily rendering the most valuable assistance to the physiologist and pathologist; but, in synthesis, her efforts have been crowned with very slight success. The only step made upon the road of synthetical animal chemistry is that pointed out by that distinguished and talented individual Dr. Faraday, in the formation of oxamide or oxalamide, from the distillation of oxalate of ammonia in water.

In this process it would appear that the ultimate elements of oxalate of ammonia, (oxygen, hydrogen, carbon, and nitrogen), are set at liberty, and that they combine in new proportions with the elements of water (oxygen and hydrogen), and thus this new compound is formed.

This oxalamide is insoluble in water, and its ultimate component elements are very analogous to those of animal matter: it is, I believe, the first instance of the laboratory of the chemist producing a body at all resembling in its composition the organized substances found in nature.

Here we have, perhaps, one little step on this difficult road of synthetical chemistry of organized matter; but unfortunately, where the path seems most inviting, there have chemists hitherto made no progress.

The composition of lignin, starch, sugar, and gum, is well understood, and the difference in the components of these vegetable substances is very trifling, still we cannot produce, artificially, sugar, nor any other organic compound, by directly combining their elements,—"Because," as Dr. Prout has most justly observed, "we cannot bring together the elements precisely in the requisite states and proportions; still there is no doubt that if the elements could be so brought together, the

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* Sir Gilbert Blane on doses of Carbonate of Potash, Select Dissertations, p. 194; Med. Ch. Trans. 1811.

† Dr. Prout.

compound thence resulting would be the same as the natural compound; for the organic agent (or, I would say, the vital principle) does not change the properties of the elements, but simply combines them in modes which we cannot imitate*."

Thus, then, it appears we are able to convert one vegetable product into another by artificial processes,—for example, lignin into starch, and starch into sugar, and this again into vinegar, and finally this last may be resolved into its ultimate elements, oxygen, hydrogen, and carbon; but we cannot retrace our steps in the present state of chemical science,—we cannot ascend the scale,—we cannot reproduce our sugar, nor the starch, nor the woody fibre.

It is, indeed, at present very surprising that bodies such as lignin, starch, sugar, and gum, which appear to resemble each other so closely in their composition, should nevertheless offer to our eyes such widely-different external characters.

Dr. Prout, who has more profoundly examined this apparent chemical anomaly than any other philosopher, believes that differences in sensible properties are produced or modified by those incidental substances which enter in very minute quantities into the composition of organized matter.

"Incidental substances," says Dr. Prout, "existing in organized bodies, have hitherto been considered as foreign; but we cannot subscribe to that notion. We may now observe, that they seem to us to contribute chiefly towards the production of those striking differences observed among bodies having the same essential composition, and which diversity at first sight appears so mysterious †."

I shall again advert to these incidental bodies, in speaking of the effects of minute doses of certain remedies upon the animal economy.

Are we to despair that chemistry will ever be able to prove synthetically the composition of the products of organization? I think not. Aristotle could not have conjectured that the composition of water would be demonstrated analytically and synthetically with so much reason as we anticipate that the day will come when chemistry shall be able to offer the same proofs of the composition of the different vegetable products, and perhaps of some animal substances also.

Physiologists at one time shrunk back from this and similar inquiries into organized matter, from a general conviction that organic agents (the vital principle) have the power not only of changing the inherent and peculiar properties of bodies, but likewise of causing the results of their combination or agency to be altogether different from those that are produced under precisely similar circumstances by inorganic agency.

The recent work of Dr. Stevens on the Blood recalled the attention of pathologists to the effects of various substances, particularly of acids, alkalies, and neutral salts, upon the blood, when mixed with that fluid out of the body, and when administered as remedies by the mouth. From the manner in which these facts were received by the medical public, and from the reviews of Dr. Stevens's work in the various periodical journals, it would appear that the experiments and opinions of numerous writers, from the time of Boyle up to that of Hewson, had been forgotten.

For the ingenious theory of the production of animal heat at every part of the body, from the generation of carbonic acid gas in the capillaries, physiologists are deeply indebted to Dr. Stevens; and, as far as I know, the credit of this discovery, and of the extraordinary attraction of oxygen for carbonic acid gas, are both his own.

But with respect to the effects of various substances on the blood out of the body, and when used as remedial agents internally, I shall briefly draw your attention to the experiments and opinions of many writers long previous to our own times, who have made known to us the very same facts.

Boyle* appears to have been the first who added liquid acids to fresh-drawn blood, and found that they destroyed its natural colour, turning it to a darkish sooty tint, and causing it to coagulate.

Fracassati, of Pisa, apparently stimulated by these experiments of Boyle's, which had been made known to the scientific world of Europe through the Philosophical Transactions, commenced the injection of the mineral acids, diluted with water, into the veins of dogs. He found that death ensued, and that the blood in the veins and in the right side of the heart was converted into a blackish coagulum. Boyle also mixed a quantity of sal ammoniac with blood, and found it had the very reverse effects of the acids, rendering it more florid and fluid.

Fracassati also injected a solution of subcarbonate of potash into the veins of dogs, which caused their death; and he states, that the blood was not curdled, as by the injection of acids, "but, on the contrary, it was more thin and florid than ordinary."

Huxham, in several parts of his treatise on the ulcerous sore-throat, discusses

* Bridgewater Treatise, Chemistry of Organization, p. 418.

† Ibid, p. 425.

* Philosophical Transactions, 1667.

the effects of alkaline salts mixed with the blood out of the body.

In treating of the use of the volatile alkaline salts, he writes thus *:—"Volatile alkaline salts promote the acrimony and dissolution of the blood in an eminent degree: when mixed with the blood, whilst actually under the power of the circulation and the vis vitæ, they certainly hasten its dissolution and consequent putrefaction; even mixed with the blood out of the body, or rather as it runs from the vein, they quite destroy the texture of the blood globules, nearly in the same manner as the poison of the viper." And he adds, "the blood of the most robust (nay, even the most buffy blood of horses), thus managed, never concretes, as usual, into a solid crassamentum, or throws off the serum, but remains ever after a loose dissolved gore or sanies."

Huxham then goes on to explain, that an excessive internal use of these volatile alkaline salts is capable of inducing a diseased state of the blood, similar to that above described, from the mixture of these salts with the blood out of the body. To these facts I will hereafter advert very shortly.

Sir John Pringle was also aware of these effects of the alkaline salts on the blood, and states in his treatise † on the diseases of the army, "that the alkaline salts prevent the coagulation of the blood; and when taken by way of medicine, perhaps thin and resolve it."

Albert Haller, upon this, as upon every other part of physiology, evinces a most perfect knowledge of all that has been done by previous inquirers into this subject; and he has likewise added some experiments of his own.

Haller thus writes (El. Phys. Lib. 5, s. 2) of the effects of alkalies and alkaline salts mixed with the blood:—(26.) "Spiritus cornu cervi quidem sanguinem nigriorem reddidit, coagula etiam nubeculosa et membranosa, non multis horis duratura, excitavit." He does not, however, appear to have tried the effects of the liquor potassæ mixed with the blood fresh drawn from the body, but proceeded to ascertain the effects of a fixed alkaline salt, potassæ subcarb. "Oleum tartari per deliquium," says Haller, "et sanguini et grumo rubicundum elegantem colorem conciliavit, non tamen supra 24 horas superfuturum."

Haller thus writes in general terms of the effects of the sales medii, or neutral salts, mixed with fresh-drawn blood:—

"Ergo primo ea communis salium mediorum et lixiviosorum natura est, ut fluidum plerumque sanguinem reddant, aut liquidum conservent; colorem vero rubrum aut firment aut omnino augeant."

He instances nitre, as possessing this power in an eminent degree, in the following terms:—"Admisto enim nitro, laminæ equidem cruoris floridissime rubentes nascuntur." "Alii sales medii," he adds, "utcumque similia faciunt. Sal marinus cum sanguine commistus floridum colorem facit, cæterum coagulationem neque valde promovet neque impedit nisi motus accesserit. Sal Epsomensis sanguinem floridiorem reddit fluidumque globulorumque distinctionem conservat."

Haller likewise describes most accurately the changes produced in the blood, both by the admixture of mineral acids with it out of the body, and when injected into the veins.

Weak solutions of these mineral acids, he states *, cause coagulation of the blood, and change it into a dark green colour.

"Meraciores iidem spiritus utique totum cum sero sanguinem cogunt nigrumque reddunt. Spiritus salis sanguinem coagulat, coloremque ad atrum tendentem inducit." He then describes similar effects from the use of nitric and sulphuric acids. Haller likewise repeated the experiments of Fracassati on dogs, and informs us, that, "in venas injectus spiritus salis necat et in iis animalibus quæ enecuit, sanguis per totam venarum seriem ad cor cusque in grumos coactus reperitur. Etiam aqua regia injecta occidit."

He likewise injected nitric acid and sulphuric acid into the veins of animals, and found that they produced death, and that the blood throughout the veins, as far as the heart and the lungs, was full of coagula. I have recently witnessed the experiment of the injection of diluted nitric acid (3ss. in ℥j.) into the veins of a dog, by my friend Dr. Roupell, and the appearances were exactly those described by Haller.

In the year 1770, Hewson published, in the Philosophical Transactions, a valuable paper, relating numerous experiments on the coagulating power of the blood under various circumstances, and upon the nature of the buffy coat which sometimes appears on its surface. It was the opinion of Hewson†, that "the air in the lungs was the immediate cause of the change of colour of the blood there; but how it effects it, is not yet determined. It happens in blood-letting," observes Hewson, "that the blood is first blackish, but afterwards comes out florid: in these cases, the arterial blood passes into the veins without undergoing that change which is natural to it."

* P. 54; Huxham, &c.
† Appendix, p. 347.

* Lib. 5, s. 2 (25.)
† Phil. Trans. 1770, p. 368.

Hewson adds this observation—"Some of the neutral salts have a similar effect on the colour of the blood that air has, particularly nitre. This is far from being the only neutral salt which has this effect on the blood; almost all the neutral salts have the same: they also prevent its coagulation when mixed with the blood just received from a vein."

Dr. Stevens has in his work argued with a great appearance of probability, that it is the oxygen of the air taken into the lungs which deprives the blood of its carbonic acid, and thus restores it to the vermilion tint on the one hand; and that the change of colour in the capillary system from a bright scarlet to a Modena red, is owing to the disengagement of carbonic acid there; and consequently there is an augmentation of temperature and generation of animal heat in every part of the animal economy.

Dr. Stevens has also proved a still greater power of these salts upon the colour of the blood: that the air alone, without their presence, is incapable of giving the blood the bright arterial tint, as may be proved by immersing a clot of blood in distilled water; it becomes quite dark; but upon removing it into a saline solution, the bright scarlet tint is immediately regained *.

I feel that some apology is necessary for having occupied so large a portion of this lecture in quotations from well-known authors; but considering that the paper which first called the attention of the medical public to the effects of acids, alkalies, and neutral salts, upon the blood, was read within the walls of this College, and that neither at that time nor since, as far as I am aware, has any reference been made to the various authors who had previously tried the same experiments, and advanced similar opinions, I have thought this a favourable opportunity of recalling our attention to the subject, and of paying a just tribute to those whose labours had apparently been forgotten.

I will now proceed to support the accuracy of the experiments of Boyle, Huxham, Haller, and Hewson, by putting before you quantities of fresh-drawn venous blood, which, while fluid, were mixed with (1) mineral acids, (2) alkalies, (3) carbonates, (4) muriates and nitrates, (5) sulphates, (6) vegetable acids.

(1.) The mineral acids, diluted and mixed with fresh-drawn venous blood, cause it to coagulate and darken its colour. How are these two effects produced? The coagulation is in consequence of the great power these acids possess of causing the coagulation of all albuminous fluids: the fibrin of the blood coagulates spontaneously, but the albumen does not. This latter is coagulated by heat. By the addition of a mineral acid, the immediate coagulation of both is effected. The darkness of colour caused by these acids arises partly from causes explained by Mr. Brande *, and probably from the decomposition of the carbonates in the blood. Their carbonic acid is set free, and has the power of darkening the colour of the blood.

Thus the whole effect is accomplished; every particle of colouring matter is thus darkened, and remains permanently associated with each coagulated globule of the venous blood.

(2.) The action of the alkalies is to retain the venous blood in a fluid state, and at the same time to darken its colour. The permanent fluidity of the blood, when mixed with alkaline solutions, depends upon the great power the alkalies possess of retaining albumen in solution under the most unfavourable circumstances. They have a similar power over the fibrin. Indeed, fibrin and albumen are only modifications of the same principle. To make use of the language of Dr. Prout, albumen is the weaker of the two. The fluidity of the blood is thus permanently accomplished by the addition of an alkaline solution.

It is not so easy to account for the darkened colour of the blood by the addition of an alkaline fluid. When diluted liquor potassæ is first added to the venous blood, the colour is very little altered; but upon standing exposed to the air, it darkens, and ultimately assumes almost the dark green tint of concentrated bile. Some French chemists were induced, by this resemblance in colour, to attempt to convert the venous blood of the portal system into bile. I need hardly say they failed. Perhaps the gradual darkening of colour may be partly owing to the strong appetite of liquor potassæ for carbonic acid, which it obtains gradually from the atmosphere, and perhaps also from other sources.

A diluted solution of ammonia renders the venous blood permanently fluid, for the same reason as I have given for the action of liquor potassæ; but the colour of the blood is by no means so much darkened.

3 and 4.—Of the salts, when mixed with portions of fresh-drawn venous blood, I may say that those which have the power of rendering albumen permanently soluble in water, also cause the blood to remain more perfectly in a fluid state: thus the

* Paper of Dr. Stevens; Coll. Physicians, May, 1830.

* Philosophical Transactions, 1812.

carbonates render the blood permanently fluid.

The subcarbonate of potash not only renders the albuminous portion of blood permanently soluble, but it also retains the colouring matter in solution; which few of the neutral salts will do. Haller stated that subcarbonate of potash, mixed with venous blood, "elegantem rubicundum colorem conciliavit, non tamen supra 24 horas superfuturum." This statement is accurate; the bright colour is evanescent. The muriates and nitrates in solution, mixed with venous blood, change the colour to a bright vermilion tint, and impede the coagulation of the fibrin; but not so perfectly as the alkalies and carbonates in solution.

5. The sulphate of soda in solution, added to a portion of fresh-drawn venous blood, converts the colour to a bright scarlet, and retards the formation of the coagulum. But my experiments with this salt on venous blood, lead me to believe that it only retards, and does not entirely prevent, its coagulation. In confirmation of this statement, I shew you this quantity (ʒss.) of venous blood, which was poured upon f. ʒj. of solution (ʒj. salt, ʒj. water) of sulph. sodæ, about twenty-four hours since. An elegant little coagulum now floats in the artificial serum of sulphate of soda.

This exception confirms the explanation I have given of the reasons why acids cause coagulation, and alkalies the permanent fluidity, of the blood. If the same solution of sulphate of soda be added to an albuminous fluid as to the highly albuminous urine of dropsy, it will not prevent the coagulation of the albumen upon the application of heat; whereas the alkalies and alkaline salts in solution, when added to albuminous fluids, prevent the ordinary coagulation which takes place on the application of heat. Of this property of the alkaline salts I shall speak more at large.

6. The vegetable acids, and especially the tartaric, when added to a portion of fresh-drawn venous blood, darken its colour, and convert it into a permanently shining elastic solid. The blood so acted upon exactly resembles a mass of jelly. The dark colour, I presume, is partly caused by the nascent carbonic acid, set free from the decomposition of the carbonates by tartaric acid, and remains combined with each coagulated globule. Of the other changes I speak with reserve, and trust that my opinions may be corrected by others more competent.

It is my belief that the action of the vegetable acid (tartaric) upon the animal matter of the blood (fibrin and albumen), is to unite with them in some new proportions, and to convert them into a weaker animal principle—*gelatine*. In speaking of the conversion of one animal substance into another, and of stronger and weaker animal principles, I adopt the language of the great philosopher of modern animal chemistry, Dr. Prout.

The gelatinous mass, produced by the admixture of a saturated solution of tartaric acid with a portion of fresh-drawn venous blood (about ʒj. acid to ʒj. blood), in its external physical characters exactly resembles gelatine. If a portion of it be carefully boiled in distilled water, it is entirely soluble. If the albuminous portion of the blood, before it had been reduced by its union with a vegetable principle, had been subjected to the heat of boiling water, permanent coagulation of the albumen would have taken place. This gelatinous mass also moulders in a manner exactly similar to that which appears on the surface of gelatine.

If a solution of tartaric acid be added to a portion of highly albuminous urine, and this be subjected to heat, no coagulation of albumen takes place; but the urine appears to be converted into a gelatinous fluid—a pellicle forms on the surface, as it does on a weak solution of glue. The tartaric acid, in this instance, seems to exert a similar power over the albumen in the urine that it does over the albuminous principles of the blood. In both cases it unites with the albumen, and converts it into a weaker animal principle—gelatine. Such are the effects of mineral and vegetable acids—of alkalies, alkaline and neutral salts—when mixed with fresh-drawn venous blood.

When acid and alkaline solutions are injected into the veins of animals, it would appear, from the experiments of Fracassati, Haller, and Orfila, that their effects upon the blood are very similar to what takes place out of the body. If these solutions be sufficiently concentrated to disorganize the blood, death immediately ensues. The injection of various neutral salts, of soda and potash in solution, into the veins of man, has of late years been tried to a great extent. Those who saw cases of cholera of the very worst description, treated by saline injections, must have been astonished at the immediate effects *.

Patients who were lying pulseless, almost speechless, deadly cold, with a livid countenance, and a shrivelled flabby skin, would, in the course of a few minutes, present a good pulse, a general warmth, respiration more natural, a full florid cheek, and an open lively eye. There could be no doubt that the saline fluid had changed the dark stagnant blood, for a time, into a florid stimulating fluid. Unfortunately, these good effects were rarely permanent;

* Saline injections in cholera, related in the Medical Gazette, June and July, 1832.

the benefit only lasted as long as the injection more than counterbalanced the loss of fluid by dejections; and sometimes, indeed, the fatal catastrophe was accelerated by sudden effusion of serum into the ventricles of the brain.

These experiments, however, proved the practicability of introducing into the circulation very large quantities of remedial agents, and that in this instance, as in many others, the effects of remedies introduced directly into the blood, were nearly the same as if they had gained admission into the vital current indirectly, through the stomach and absorbent system.

RESULTS OF A

SERIES OF EXPERIMENTS IN REVACCINATION,

Performed in the Royal Army of Wirtemberg.

CONDENSED BY DR. GEORGE GREGORY, FROM A MEMOIR,

BY DR. HEIM, OF LUDWIGSBURG*.

THE repeated instances in which small-pox of an unequivocal character (sometimes severe and even fatal, but oftener mild and singularly modified,) has succeeded to vaccination, have excited in a very remarkable manner the attention of the great military governments of Germany. Two of them, *Prussia* and *Wirtemberg*, have availed themselves of the admirable opportunities which the numbers and discipline of their armies afford, to institute a series of experiments by which the protective power of vaccination might be determined, without risk to the health of the troops, or to the security of the population among whom they were resident. These experiments appear to have first commenced in the year 1829, and the first results were such as to induce both a continuation and an extension of the plan. During the years 1831, 1832, and 1833, the practice of revaccination in great masses of troops has been carried to an immense extent, and the system is still in full efficacy. It has been thought that a brief abstract of the principal results of these very extended trials, in so far as they have yet reached us, might be useful, by directing the attention of the profession in this country to a subject as yet but little cultivated.

In Rust's Magazine (band 39, st. 3,) may be found a statement, entitled "Collection of the reported results of Revaccinating the Prussian Army," together with a circular from the Chief of the Military Medical Staff of that army, addressed to the physicians and surgeons in charge of troops, directing them to collect carefully all facts bearing on the question of *revaccination*, and to report the same annually, at the end of February, to the Royal Military Council. This circular is dated March 15, 1833.

In the London Medical Gazette of Saturday, March 1, 1834, (vol. 13, p. 842,) will be found a detailed statement of the principal results of these experiments in three corps of the Prussian army, viz. the 3rd, 5th, and 8th corps. They are as follows:—

"Out of 6020 men of the 3rd corps, revaccinated at Erfurt in 1831, 2354 exhibited vesicles, among which more than 1-8th proved to be of the true vaccine character. In the following year, 3942 men of the same corps were revaccinated, of whom 1594 exhibited effects, somewhat more than 1-8th being the true vaccine.

"In the 5th corps, out of 3234 revaccinated persons, 2535 were affected, with a like proportion of normal cases.

"In the 8th corps, 2784 men were subjected to revaccination, which took effect on 925. About 1-8th proved to be the true vaccine.

"The accounts from other divisions of the Prussian army had not yet been received. The advantages of the measure of general revaccination are strongly insisted on, and it is observed, that after its adoption at Erfurt not one soldier or recruit became affected, though in close contact with many very violent cases of natural small-pox."

Dr. Heim, of Ludwigsburg, physician to the late Queen of Wirtemberg, has been officially nominated by the government of that country, to superintend the revaccination of the Wirtemberg army. The results of these trials (which appear to have been made with a very perfect knowledge of the subject, and with a closeness of observation worthy of the highest admiration,) are contained in a Memoir now laid upon the table, which is a translation from the German, made with as much knowledge of the English language as Ludwigsburg afforded, in the month of April last. Its length, and the total absence of any arrange-

* Read at the College of Physicians, June 23, 1834.

ment of its parts, preclude the possibility of adapting it, by any change of language, to the purposes of these meetings. Many of the statements, however, are important, (especially when viewed in conjunction with the Prussian experiments already adverted to,) and some of them have the rare merit of novelty.

The following is a condensed summary of the principal statistical and pathological facts scattered through Dr. Heim's long and laborious Memoir:—

Prior to the year 1829, it was the custom of the Wirtemberg service to vaccinate all recruits who had neither undergone small-pox, nor been vaccinated in early life. A variolous epidemic, which shewed itself at Stutgard in 1829, occasioned the issue of a ministerial order, dated March 26, 1829, directing that henceforward all recruits should be subjected to vaccination who could not show satisfactory *cicatrices* either of small-pox or of the vaccine. In the autumn of 1832, small-pox broke out in the garrison town of Ulm; and, again, a third epidemic occurred at Ludwigsburg in 1833. These repeated visitations of small-pox in the Wirtemberg territories occasioned the issue of another order, dated Feb. 7, 1833, directing indiscriminate revaccination of all recruits, without reference to vaccine cicatrices. Directions were further given for revaccinating every individual of the two garrisons of Ulm and Ludwigsburg, of whatever age or standing in the army—those being the towns in which the variolous epidemic had displayed itself in the greatest force.

The general results of these very extended trials are given by Dr. Heim in the form of tables, copies of which are submitted to the meeting.

Table No. I. gives a general survey of the success attending the revaccination of the entire garrison of Ludwigsburg during the summer of 1833. The garrison consisted of a regiment of artillery, a regiment of infantry, and two regiments of cavalry: total, 1683. Of these there were revaccinated, with the best success, 577, (one-third of the whole)—with modified or partial success, 366—without success, 740. Nearly the whole of the subjects of these experiments were, as might be expected, adults, between 20 and 30 years of age, and of course the greater portion of them had been vaccinated in infancy. 144 only were above or under the ages now specified.

Of the 577 men revaccinated, as above stated, with the best success, 293 (more than one-half) had perfect cicatrices. On the other hand, out of the 740 revaccinated *without* effect, 222 had imperfect scars, and 136 retained no mark whatever of their first but effective vaccination.

Table No. II. presents a general summary of the results of the revaccination of the *Recruits* of the Royal Army of Wirtemberg, from 1829 to 1833, of which the following appear to be the most important:—

Total numbers upon whom the operation of revaccination was performed	4802
Of them there were revaccinated—	
With normal or perfect success	1208
With modified or imperfect success	956
With success, but in a degree not accurately specified . .	914
Without success	1724
Total . . .	4802

If we deduct from the total number revaccinated 4802, those in whom the success, though perhaps in the plurality normal, is yet not pointed out with precision (914), the proportion of those revaccinated with good success is 30 per cent—of those with modified or partial success, 24 per cent—of those without result, 46 per cent.

The same table presents us with a series of data from which the value of the cicatrix, as a test of constitutional safety, may be inferred.

Of 4111 persons revaccinated, 2193 had normal cicatrices. In 2390 cases the operation produced a certain degree of effect. One-half passed through the second vaccination in a normal manner; the other half imperfectly, and in a modified form. Of the former, or regular revaccinations (1208 in number), 664 had perfect cicatrices; that is to say, one-half of those upon whom the process of revaccination produced the most *decided* effects, had scars, from which an *opposite* result might have been anticipated.

The converse of the proposition leads to a like conclusion. As thus:—Of 1722 persons on whom the operation of

revaccination produced *no* effect, and in *all* of whom, therefore, good cicatrices might have been expected, 500 had *imperfect* scars, and 259 bore no scars whatever.

Again: of 956 persons in whom, upon being revaccinated, the cow-pock appeared, although in a modified form, 572 bore upon their arms *good* cicatrices, 278 had *imperfect* cicatrices, and 104 had *no* cicatrices whatever.

From these and various other facts Dr. Heim concludes, that the inspection of the scars left by the primary vaccination is devoid of all practical interest: in other words, that no satisfactory conclusion, as to the resusceptibility of the disease or otherwise, can be derived from such a source.

Such are the principal results deducible from the tabular statements transmitted by Dr. Heim. The learned author appends to these certain pathological observations made by himself while superintending, in his official capacity, the revaccination of the garrison of Ludwigsburg, in 1833.

The first and certainly the most interesting circumstance adverted to, is the effect which the extended system of revaccination pursued by the authorities of Wirtemberg had in checking the variolous epidemy. His words are:— "This powerful revaccination of the men comprising the garrisons (of Stutgard, Ulm, and Ludwigsburg,) without reference to the quality of the cicatrices, destroyed the spreading of small-pox among the soldiers." In another part of the MS. he uses these expressions:— "In this manner the revaccination passed in a few weeks through all the regiments of the garrison, and the spreading of the epidemic small-pox among the military was set real bounds to by it."

The next circumstance adverted to by Dr. Heim is entirely new to the practitioners of this country, namely, the difference of result according as the propagating lymph was derived from the arm of a child or that of an adult. It has long been known that children are vaccinated most successfully from others of their own age. Dr. Heim's experiments lead him to believe, that *adults* are vaccinated most effectually from the vesicles *of adults*.

His words are these:—

"In the first regiment of cavalry many fruitless, or at least unsatisfactory, trials were made with fresh and good matter taken from the arms of children. At length I made it prosper upon a soldier, in whom fifteen exquisitely perfect pustules were produced. By revaccinating with this *adult lymph*, I succeeded with those in whom vaccination from the arm of the child had *failed*. Even in individuals of a higher class of society, with all favourable qualities for good success, the trial of vaccination from the child's arm frequently failed, while I soon succeeded in it from the arms of adults."

That this was owing to some peculiarity in the law of *adult vaccination*, and was not attributable to any fault in the vaccine matter of the infant, was proved by its *constant* success when infants alone were the subjects of trial.

Further on Dr. Heim thus expresses himself:—

"Of the superior effect of vaccinating from *adults* to *adults*, I have had experience for the last six years, but the opportunity which has been recently offered of trying it on a large scale, has been rewarded *with the most splendid success*.....My colleagues (he adds) of this place (Ludwigsburg) have also made the same observations."

In another part of the memoir, while adverting to the employment of adult lymph, to which he attributes the superior efficacy of the revaccinations of 1833, Dr. Heim notices the prejudices that so generally exist in Germany as well as in England, against employing the vaccine matter of adults in propagating the disorder. "This false opinion," he states, "is the more striking, as it is neither defended by obvious reasons, nor confirmed by experience. On the contrary, there are cases known to us where the modified vaccine of revaccinated persons produced the *genuine* vaccine in children not previously vaccinated: nay more, in some instances it took effect upon those whom I had vaccinated more than once, but without success, using recent lymph from the arms of children."

Having satisfied himself of the truth of his position, that important practical differences do exist between adult and infantile vaccination, Dr. Heim proceeds to reason upon it.

"*We* might expect," he says, "*à priori*, that the vaccine lymph of adults

TABLE I.

TABULAR VIEW *of the* RESULTS *of the* REVACCINATION *of the* GARRISON *of* LUDWIGSBURG, *in the Summer of* 1833.

Regiment.	Total Numbers submitted to Re-vaccination.	Re-vaccinated with the best Success.								Re-vaccinated with modified Success.								Revaccinated without Results.							
		Total.	Vaccine Cicatrices.			Age.				Total.	Vaccine Cicatrices.			Age.				Total.	Vaccine Cicatrices.			Age.			
			Normal.	Imperfect.	Wanting.	Under 20.	Between 20 & 30.	Between 30 & 40.	Above 40.		Normal.	Imperfect.	Wanting.	Under 20.	Between 20 & 30.	Between 30 & 40.	Above 40.		Normal.	Imperfect.	Wanting.	Under 20.	Between 20 & 30.	Between 30 & 40.	Above 40.
Artillery Regiment	301	128	76	8	44	..	117	10	1	37	14	14	9	..	36	1	..	136	63	57	16	..	133	2	1
Arsenal Company	80	14	6	3	5	1	10	3	..	47	24	19	4	1	44	2	..	19	12	2	5	..	14	5	..
1st Regiment of Cavalry	290	124	62	35	27	2	99	23	..	113	58	46	9	3	103	7	..	53	28	17	8	1	47	5	..
2nd Regiment of Cavalry	283	83	42	21	20	..	76	5	2	120	74	39	7	..	110	10	..	80	50	26	4	..	76	4	..
5th Regiment of Cavalry	729	228	107	49	72	4	103	19	2	49	23	16	10	2	44	3	..	452	229	120	103	5	427	20	..
TOTAL	1683	577	293	116	168	7	505	60	5	366	193	134	39	6	337	23	..	740	382	222	136	6	697	36	1

Table II.

SUMMARY OF THE RESULTS

ATTENDING THE

REVACCINATION OF THE RECRUITS OF THE ROYAL ARMY OF WIRTEMBERG,

BETWEEN THE YEARS 1829 AND 1833 (INCLUSIVE.)

PERFECT SUCCESS.					IMPERFECT SUCCESS.					SUCCESS NOT ASCERTAINED.						WITHOUT RESULTS.					
Re-vaccinated Successfully.	Character of Cicatrix.				Re-vaccinated with Partial Success.	Character of Cicatrix.				Re-vaccinated with Results Unknown.	Character of Cicatrix.				Re-vaccinated—Results Unknown—State of Cicatrix unknown.	Re-vaccinated without Results.	Character of Cicatrix.				TOTAL RE-VACCINATED.
	of Vaccination.			Small-Pox.		of Vaccination.			Small-Pox.		of Vaccination.			Small-Pox.			of Vaccination.			Small-Pox.	
	Normal.	Imperfect.	Wanting.			Normal.	Imperfect.	Wanting.			Normal.	Imperfect.	Wanting.				Normal.	Imperfect.	Wanting.		
1208	664	259	281	4	956	572	278	104	2	223	149	71	1	2	691	1722	957	500	259	8	4802

should be more *energetic* than that of children, for the same reason that we find measles, scarlet fever, and even the small-pox itself, to be more *dangerous* in adults than in children." He next throws out the very ingenious hint, that it was to the employment of vaccine matter from the arms of *adults*, that Jenner was indebted for the splendid success which attended all his first trials with the vaccine." Dr. Heim evidently considers this as the chief feature of his memoir, and he invites the attention of physicians to it, in an especial manner, as to a subject of great scientific and practical interest.

Dr. Heim supports the doctrine of a gradual return of the vaccine susceptibility in persons even the most effectually vaccinated in early life, but his further speculations on this subject are confused and scarcely intelligible.

Dr. Heim has made no experiments with the variolous contagion, so as to determine positively, whether the susceptibility of small-pox and the resusceptibility of cow-pock acknowledge the same law. This important branch of the subject requiring, on the part of the experimenter, such extreme caution, must be left to be determined by future inquirers. Dr. Heim indulges the notion, that the resusceptibilities of small-pox and the vaccine do *not* coincide, but he throws out the suggestion without offering any adequate proofs in its support.

Dr. Heim details a curious case in which small-pox and measles occurred simultaneously, and he seems to hint that there is some sort of analogy in the relations of cow-pock to small-pox, and of small-pox to measles.

The memoir, of which an abstract has now been given, would have some claims upon the attention of Englishmen, were it only for this single circumstance, that it records the history of 4802 individuals of the Royal army of Wirtemberg, and that among them *sixteen* persons only exhibited upon their arms the cicatrices of small-pox inoculation. 4786 had been vaccinated!!

"Quæ regio in terris nostri non plena laboris."

ILLUSTRATIONS OF THE PRECEDING PAPER ON RE-VACCINATION.

To the Editor of the Medical Gazette.

Sir,

You will oblige me by giving insertion, in your ensuing number, to a few observations, illustrating the Paper on Re-vaccination, which I had the honour to submit to the Royal College of Physicians on Monday last. The observations are intended to show how far the experience of the German physicians has been borne out by that of the Small-Pox Hospital, and what is the essential improvement in our pathological views of vaccination, deducible from the facts now brought forward.

Various opportunities have occurred to us, at the Small-Pox Hospital, during the last five years, of presenting, upon an extended scale, researches into the phenomena of Re-vaccination. At the Deaf and Dumb Asylum, at the Bloomsbury National School, at the St. Pancras and other parochial workhouses, we have re-vaccinated large masses of persons; but hitherto our experience has been limited to persons *under the age of eighteen*. The Germans, on the other hand, have had the inestimable advantage of operating on persons between the ages of 20 and 30. This will account for the fact, that their results have been clear and decisive, while our's have been obscure and imperfect.

The great principle in pathology which the German experiments tend to establish is this—that the influence of vaccination on the constitution of the *adult*, is not identical with that which it exerts upon the constitution of the *infant*. Such a principle was wholly unknown to Dr. Jenner. Such an idea, if it ever did occur to him, was scouted, as being contrary to the analogy of variola; and unquestionably nothing could have induced a belief in it but experience, on the extended scale which Dr. Heim and other German physicians have recently enjoyed. An idea, that the influence of vaccination diminished as life advanced, has long prevailed; and I have alluded to and discussed it on several occasions in the London Medical Gazette, more particularly in vol. i. p. 589, and vol. v. p. 795. The notion, however, hitherto entertained by myself and others, has

been, that the decay of vaccine influence (supposing it to exist) was *gradual;* that if an infant, aged six months, was preserved by it effectually till he was (say) twelve years old, another child vaccinated at twelve years of age, would be safe until he was twenty-four. The German experiments afford no support whatever to such a doctrine, which is probably erroneous. They tend to show that the change in the susceptibility of cow-pox, (and probably of small-pox also), has reference simply to the *adult* constitution, and that the young man of 21, vaccinated at 12 *years* of age, would be just as open to small-pox as another of the age of 21, vaccinated at six *months*.

The experience among the re-vaccinated *out-patients* at the Small-pox Hospital, has been much too limited to determine the truth of this principle; but other facts, drawn from the interior of the hospital, from the wards appropriated to the reception of patients under small-pox, are decidedly in its favour; and certainly enough has transpired already, to shew the necessity of investigating how far the changes which the system undergoes between the ages of fifteen and twenty-one (when the adult constitution may be considered, in physiology as in law, to be fully established), tend to impair the vaccine influence previously existing in the habit.

If it should be found that such changes do, in a large proportion of cases, sensibly diminish and impair vaccine security, then the plan of *adult re-vaccination* may be recommended upon strong and intelligible grounds, and with the fair prospect that such re-vaccination will give to the individual, for the remainder of his life, every security from the attack of small-pox which he can reasonably expect.

I have the honour to be, sir,
Your very obedient servant,
GEORGE GREGORY.

31, Weymouth-Street,
July 5, 1834.

CASE OF

SUPERFŒTATION(?) IN THE SHEEP:

With Observations.

By R. ARROWSMITH, M.D. COVENTRY.

PHYSIOLOGISTS in general are extremely sceptical concerning the possibility of superfœtation, or the conception of a second embryo during gestation. And those even who have been disposed to admit the mere possibility of such an event, have restricted it, either to the first few days following the former conception, before the *membrana decidua* was organized, or to the rare instances of double or bilobed uteri*. It cannot, however, be denied, that there are on record a few cases—and very few there certainly are—in which the facts seem clearly to indicate that a second impregnation had taken place many weeks subsequent to the former conception, of which the product was then borne in the womb, and continued to advance to maturity; and in one remarkable example of this kind, in the human female, it was ascertained by post-mortem examination that the uterine cavity was single.

The instance of supposed superfœtation which is the subject of the present communication, occurred in the sheep. In that animal the uterus, as the reader is doubtless aware, is bicorned. The os uteri is common to each cornu, and the division of the uterus into the two cornua commences about an inch from the os uteri. Thus there is a free communication between the two cornua; nor does there appear any thing in the anatomical structure or relations of the parts more favourable to superfœtation than in the single triangular cavity of the human uterus. The circumstances of the case were as follow:—

A ewe, two years old, the property of Mr. Goode, of Allesley, near this city, produced a lamb on Saturday night, 2d March, 1833, or early on the following morning; and on Saturday morning, the 30th of the same month, that is,

* The malformation of the uterus, improperly called "double," but really a *uterus bicornis*, (an example of which has recently been given in the Gazette, vol. xiii. page 898,) is not very uncommon. Dr. William Hunter has preserved several specimens of it in his collection.—(Anatomy of Gravid Uterus, p. 6.) The division of the uterus into two cavities is sometimes more complete; and Otto states that it has been found trilocular.—(Handbuch der Pathologischen Anatomie, 363.) The uterus is sometimes found truly double, that is, the body of each uterus is distinct, and with a separate os uteri.—(Beek's Med. Juris. Dunlop's edit p. 91; and Dr. R. Lee, in Med.-Chir. Trans. and Med. Gaz. *loc. cit.*) In the interesting account of bicorned uterus published by Dr. R. Lee, the right cornu had been impregnated. Dr. Lee states, that the unimpregnated cornu was also coated with deciduous membrane; and he thinks that whenever conception has taken place in a bicorned uterus, a *membrana decidua* may be found in the unimpregnated cornu, as in the case described by him, and as is usual in the lower animals.

twenty-seven days afterwards, she yeaned a second lamb. The observations made by the shepherd left no doubt that each of the lambs was really dropped by the same ewe. The first lamb was yeaned exactly at the expected time: it was strong and evidently mature. When the shepherd inspected the sheep early on the morning of the 3d of March, he found three had lambed, the ewe in question being one of them. He applied the lamb to this ewe, and found that she had plenty of milk, which would not have been the case had she lambed a month before her time. The lamb was quite equal in size and activity to the other lambs dropped that night, and walked away well with the mother. The shepherd never thought of the ewe having another lamb. The lamb in question grew, and went on exceedingly well.

On the 30th March, on going into a field where this ewe and three others were, (two of them having two lambs each, and the other one,) he was surprised to find, in addition to the first lamb, which was by her side, another very recently "dropped" lying before her, which she was licking. The "cleansing" was close to the ewe, as she had not risen from the spot since its expulsion. As, during a period of thirty years that the man had been a shepherd, he had never observed any thing analogous to this circumstance, he was greatly surprised at the occurrence.

The second lamb was perfectly formed, strong, and healthy. The ewe had both lambs with her for two days, and appeared equally attached to them. The older one, being so much the stronger, took most of the milk; the younger one, therefore, was put to another ewe, which had lost her own lamb. It continued to thrive and do well.

The ram was admitted to the flock, of which the ewe in question was one on the 8th of October, and remained with them a month or five weeks. It was discovered, by the usual mark, that this ewe took the ram some day during the first week, but whether subsequently also it was not ascertained. From the 8th of October to the 3d of March, both inclusive, (that is, from the time the ram was put to the sheep to the time this ewe and *others* lambed,) is 147 days, or 21 weeks, which is the natural period of utero-gestation in that animal. The ewe was slaughtered on the 21st of September, 1833, in order to ascertain whether the uterus was normal in its structure: no deviation from the usual condition of the parts was discoverable.

The facts now detailed lead to the inference, that, in this case, either superfœtation took place, or that gestation was protracted 27 days (that is, one-fifth of the whole period) beyond the usual duration. The observations of M. Tessier* have rendered it generally known, that the duration of gestation, in certain species of the mammiferæ, is liable to considerable variation. In horses and cows, for instance, the variation amounts occasionally to one-fourth of the whole period. In sheep, however, the variation is confined within much narrower limits; for the observations of Tessier on these animals shewed that the mean term of utero-gestation was 150 days, and the extreme difference only 11 days.

It is evident, therefore, that if the anomaly in the instance related in this communication be referred to the protracted gestation of one lamb, the deviation from the order of nature would be as unprecedented, if not so inexplicable, as superfœtation in the human subject, to which, for the purpose of illustration, it will be convenient here specifically to advert.

Marie Begaud, æt. 37, was delivered on the 30th of April, 1748, of a boy, which was living and mature. She had some discharge at the time only: neither lochia nor milk were secreted. A quarter of an hour after the delivery, she was sensible of a motion in the womb, which continued in the manner which is usual when the uterus contains a living fœtus. On the 16th of September following, (four months and a half after the former delivery,) she gave birth to a living full grown girl. On this occasion the secretion of milk and lochia was natural. The first child lived two months and a half; the second rather more than a year. Subsequently she was twice pregnant, but the delivery on these occasions was unattended by any peculiar circumstance. She died in 1755: the body was examined, and the uterus and its appendages were found in all respects normal.—(Eisenmann de Utero Duplici; Argent.)†

Desgranges, of Lyons, communicated the following case to Foderé:—

* Edinb. Med. and Surg. Journ. xxv. 257.
† Dict. de Medécine, xx. 76.

Benoite Villard, about 27 years of age, had a misearriage on 20th May, 1779. She became pregnant again a month afterwards; and on January 20, 1780, eight months after her former delivery, and seven months after eoneeption, she suddenly gave birth to a girl. She had no fever; neither secretion of milk nor loehia; nor did the abdomen altogether subside. She was obliged to procure a nurse for her child. Three weeks afterwards, she perceived movements in the womb, sueh as are usual in pregnaney. The abdomen continued to augment in volume, and on the 6th of July, 1780, (five months and sixteen days after the former aeeouchement,) she was delivered of another girl. The lochia and milk followed naturally on this oeeasion, and she suckled her ehild. Both ehildren were living in 1782. "Nul doute," says Desgranges, "que ees deux enfans n'eussent été formés dans la même grossesse, et dans un intervalle de quelques mois: le second n'a pu être conçu après l'aceouehement du premier; ear le mari de Benoîte ne lui avait renouvelé ses earesses que 20 jours après, ce qui n'aurait donné au second enfant que quatre mois vingt-sept jours.*"

Other cases might be added, but it is unnecessary to quote them, as the two preceding are probably the most unequivocal on reeord. To refer the explanation of these and similar eases to protracted gestation, is an assumption unsupported by any direet proof, and appears, moreover, inconsistent with analogy; sinee no instanee of *single conception* can be brought forward in the human female, wherein the natural period has been exceeded by more than a few days, or at farthest two or three weeks†. And in the absenee of any such analogical proof, ought we to believe that in some examples of *double conception*, gestation may be going on (of one of the products of that conception) for twelve or thirteen months, while the other is expelled mature at the usual time? In the case of twins or triplets, inequality of development of the fœtuses is not unfrequently observed; nevertheless, in these instanees, the different fœtuses are all expelled within a short time of each other. If the cases of Marie Begaud and Villard be viewed as cases of protracted gestation, and were explieable on the hypothesis of inequality of development, it ought, on the contrary, to be shewn, that in instances of double or triple eoneeption, the expulsion of one or more fœtuses *not unfrequently* takes plaee, (not, indeed, at sueh remote intervals of time as in the eases of Begaud and Villard,) yet at more or less distant periods, corresponding with their respeetive degrees of development; but in praetice this is *never* found to happen; all are expelled at the same labour, if the natural term of gestation have been completed. A blighted fœtus of a few months may be retained in utero, and expelled at the natural period with a living and mature child*: and a blighted fœtus, with its placenta, has been expelled, leaving another fœtus in the womb for many weeks, whieh was born alive, and well developed, at the usual time†. But in these circumstances there are no points of strict analogy with the eases of presumed superfœtation alluded to in this paper.

From the facts and eonsiderations now advanced, it appears to me, that, under some rare eombination of eircumstances, superfœtation must be admitted to be a possible occurrence even several weeks subsequent to eonception, and that it did take plaee in the case of the ewe detailed in this communieation. The eonnexion of this subjeet with medico-legal inquiries, it is unneeessary here to detail. In the learned and judicious artiele, *Succession of Inheritance*, in the Cyelopædia of Praetieal Medicine, superfœtation is not alluded to. That article, however, presented the appropriate opportunity in the Cyelopædia to explain the eonnexion of the subjeet with medico-legal praetiee: the omission was probably owing to the disbelief of the talented author (Dr. Montgomery) in the possibility of superfœtation.

I eannot pretend to explain in any way how such a deviation from the

* Foderé, Traité de Med.-Légale, i. 485. Desgranges was a man of great sagacity, experience, and probity; very unlikely to be imposed upon, and incapable (I believe) of misrepresenting facts. Other practitioners were joined with him on this occasion, and the circumstances of the case were attested before the public authorities. I can conceive no fallacy in this instance, but by supposing that the first was a supposititious child,—a conjecture, however, unsupported by probability, and not meeting the particulars of the case in other respects.

† Dict. de Medécine, x. 462; and Cyclop. Prac. Med. iv. 90. See also Edinb. Med. and Surg. Journ. xxvii. 109, for review of Lyall on the Evidence in the Gardner Peerage cause.

* Med. and Phys. Journ. xvi. 53.

† Med.-Chir. Trans. ix. 194.

natural course of things takés place, yet if the evidence of the fact be irresistible, the difficulty of the explanation must be borne with. The mysteries of conception are numerous; nor does superfœtation seem more incomprehensible than some other concomitant phenomena; not more, for instance, than the fact, that "the influenee of the male, in the case of some animals, is not confined to the ovum which he actually impregnates; the peculiarities of a male animal that has once had intercourse with a female having been distinctly recognised in the subsequent connexions of that female with other males; as in the instance of an Arabian mare that had been once covered by a quagga.—Phil. Trans. 1821*.

Coventry, June 23, 1384.

ON

THE PATHOLOGICAL EFFECTS PRODUCED BY LOCALITIES,

(*Particularly Mountain Elevations,*)

NORTH OR SOUTH OF THE MAGNETIC EQUATOR.

To the Editor of the Medical Gazette.

Sir,

In my paper relative to the effects of mountain elevations upon the human body, published in the Medical Gazette of the 10th May, I attributed the reverse affections of each hemisphere to electrieity occupying the upper part of bodies in the northern one, and the lower part thereof in the southern, determining, thereby, the blood toward the head in the former, and toward the feet in the latter, causing thus apoplectic symptoms in the one, and the usual symptoms of fainting in the other. The fact of Humboldt expcriencing a strong determination of blood to the head in his ascent of Chimborazo (ninety miles to the *south* of the equator), in no way weakens the view I have taken as to the eause of these diverse affections, but, on the eontrary, strengthens it, because, according to Biot's table of observations, the magnetic equator (the line encircling the globe where there is no dip of either point of the needle), is not parallel to that of the earth, but intersects it at an angle of 12, at the points of 66 deg. 46 min. east long. and of 113 deg. 14 min. west long., the magnetie equator extending to the *southward* of the terrestrial one between those points, and thereby rendering all places a little southerly of the latter within this limit (such as Chimborazo), liable to the determination of blood toward the head, experienced upon northern mountains. The present southerly limit of the magnetic equator in South America has not yet been defined, but it cannot extend beyond 8 deg. south in the linc of the Andes, from Bolivar's army suffering so severely from the southern disease in about this degree of latitude. The place where myself and companions were attacked was in 17 deg. south, the elevation of the head being here unequivocally demonstated to predispose to the disease, by the mules only suffering when moving up hill, while we suffered equally in moving up or down hill, or along level ground; the heads of the mules being only higher than their bodies when ascending hills, while our heads necessarily formed the highest part, whatever was the nature of the surface over which we walked. The relief obtained through a depending state of the head is ascribed, by the Peruvian peasantry, to the smelling of the earth by the person or animal affected.

The above reverse determination of blood in eaeh hemisphere, though more signally exemplified in high elevations, seems still sufficiently apparent in places approximating the level of the sea, by the scrofulous and catarrhal enlargements of the lymphatic glands of the neek, so common in the northern hemisphere, being almost unknown in the southern, the glands of the groin being those liable to disease in the latter. During a seven years' residence in southern elimes, I never witnessed a single case of enlargement of the lymphatic glands of the neck in either native or European, while enlargements of those of the groin eonstitute the greatest pests a medical man has to combat there. It is to this determination of blood toward the fect, instead of the head, in the southern hemisphere, that I also would attribute the rarity of mania and apoplexy, as well as of typhus and other fevers characterized by cerebral congestion, in all the southern climes I have visited.

* Alison's Outlines of Physiology, 401.

Elephantiasis, a disease commencing in an enlargement of the inguinal glands, is now almost confined to the southern hemisphere, although recorded to have been formerly as common in some of the West-India islands north of the earth's equator as it is now in the islands of the Pacific south of it. This decrease of the complaint in the West Indies, is, I conceive, referable to the westerly revolution round our globe, in the space of between eight and nine hundred years, of the points of intersection of the magnetic equator with the equator of the earth, through which a portion of country, extending to several degrees of latitude, and to 180 of longitude, on either side of the earth's equator, is placed under the northern influence for one-half of the revolutionary period, and under the southern influence for the other half.

This revolution in the magnetic equator is sufficiently indicated by the alternate progressive increase and decrease, within certain limits, of the dip of the needle, as the following illustrations will exemplify. The line of the magnetic equator encircling the globe, from being the centre between the opposing northern and southern magnetic forces, is therefore characterized by the needle maintaining a horizontal position along it, in consequence of the opposing forces (viz. the attractive force of the northern hemisphere for the north point, and of the southern hemisphere for the south point) being here exactly balanced, which balance being necessarily progressively impaired as we recede from the magnetic equator, hence the north point of the needle dips more and more downwards as we proceed from the latter to the north pole of the earth, and the south point similarly toward the south pole thereof, in consequence of the progressively impaired power of the hemisphere receded from enabling the hemisphere experimented in to dip down the point of the needle attracted by it with greater and greater ease toward the earth. Now, Biot's table shows, that by the magnetic equator interseeting the terrestrial one at two opposite points of the earth's circumference, it is thereby made to extend itself into opposite points also of the northern and southern hemispheres, in the form of an arc; through which eccentricity of position, with respect to the earth's equator, it will necessarily alternately diminish and increase the dip of the needle, at a given point, upon the earth, according as it approximates to, or recedes from, the place experimented in, during the period of its revolution. Now, as the dip of the north point of the needle was at its *maximum* in London, in 1723, therefore at this period the magnetic equator must have been at the *maximum* distance to which it is capable of receding from London during the period of its revolution, and, consequently, the line of longitude passing through London must have then bisected at right angles the concave centre of the arc described by it, in the opposite part of the southern hemisphere. But as this arc extends into the southern hemisphere, in the present day, at the points of 66 deg. 46 min. east long., and of 113 deg. 14 min. west long., and the line of longitude bisecting this being 23 degrees west of London, hence it is evident that a westerly movement of this arc, amounting to 23 degrees of longitude, has taken place since 1723; so that, presuming this movement to be uniform, the western point of intersection of the magnetic equator with that of the earth, must have been to the *eastward* of the West India island 230 years ago, thereby admitting the arc extending into the northern hemisphere, *westerly* of this point, to embrace some of the West India islands in its bounds, and thus render them amenable to southern influence.

The intimate connexion found to exist between magnetism and galvanism, and the wonder-working power again so strikingly manifested by the latter upon both dead and living matter submitted to its influence, naturally tend to the inference of the animal body owing its formation, its diseases, and its decay, to similar agencies; for when we contemplate the rapid destruction of old products, and the formation of new, by the galvanic currents circulating through the voltaic trough, we may readily infer the possibility of similar changes being produced in the animal machine by the galvanic currents circulating through it, and, consequently, that the same power which built it up may eventually effect its destruction.—I am, sir,

Your obedient servant,

P. CUNNINGHAM,

Surgeon R.N.

July 1, 1834.

PHYSIOLOGY OF THE VOICE.

REMARKS ON MR. RICHMOND'S MUSICAL PERFORMANCE.

To the Editor of the Medical Gazette.

SIR,

IN your Gazette of the 5th of April last I noticed some observations by Professor Mayo, explanatory of Mr. Richmond's musical performance, in which he endeavoured to point out the principle on which that curious phenomenon depends. As I do not think that gentleman has quite satisfactorily accounted for the mode by which those double sounds are produced, I shall feel obliged by the insertion of the following remarks.

It is now pretty generally known that the great peculiarity of Mr. Richmond's performance consists in his being capable of producing two kinds of distinct tones at the same time; one being a kind of drone, which is the graver or fundamental tone, — the other a more acute tone, resembling a kind of whistle.

That the higher or whistling sounds are not laryngeal, I quite agree with Mr. Mayo; likewise in the idea that the drone or lower tone is produced in the larynx: but I do not concur with him in the opinion that the tones are analogous to those of the Jew's harp, or that (as he states) "they are elicited out of or rather produced upon the bass sound, in the same manner as the tones of the Jew's harp." Now, in that instrument the two kinds of tones necessarily go together, which, upon investigation, we find does not occur in Richmond's case.

It may be easily detected that both the tones are not laryngeal; for had that been the case, the tongue would have remained free to articulate.

The circumstance of the pomum Adami remaining stationary during the variations of the higher notes, does not convey any conclusive argument that both tones are not laryngeal, inasmuch as we may imagine it possible that whilst the drone was effected by the vocal chords, the whistling tones might be caused by some peculiar position or state of the epiglottis; but that this is not the case, may be proved by examining the state of the tongue during the performance. I observed, that when the drone is produced alone, the tongue remains free to articulate, but in the production of the higher notes, the contrary takes place. He cannot speak, or move the tongue, whilst in the act of producing those sounds. When the lips are closed, the upper tones are inaudible, whilst the drone contines effective; reverse the experiment, by opening the lips and closing the nostrils, and the drone ceases. The production of the upper tones, perhaps, resembles the flageolet more than any other instrument; but I should say it approximates most nearly to the common mode of whistling. He is obliged to press the tongue firmly against the roof of the mouth, and to bring the dorsum of the tongue very nearly in contact with the palate, leaving a small chamber at the posterior part, with a small opening at the side of the tongue for the escape of the air. It is in this manner the upper tones are effected, which are varied by diminishing or increasing the cavity between the tongue and palate. Two distinct tones are produced, not mixed, as in the Jew's harp, with the fundamental one, but the drone issuing at the nostrils, while the upper tone escapes from the lips. These experiments of alternately closing the lips or nostrils, clearly indicate the manner in which the performance is effected. The upper notes are not necessarily harmonies resulting from the fundamental notes; their relative positions in the scale being under the control of Mr. Richmond's taste: he can vary them at pleasure.—I am, sir,

Your obedient servant,

JOHN BISHOP.

38, Bernard-Street, Russell-Square,
June 26, 1834.

EFFECTS OF

CERTAIN DISORDERS ON THE ROOTS OF THE HAIR.

To the Editor of the Medical Gazette.

SIR,

So far as I remember, I have not seen any remark upon the very singular fact, that no hair is ever observed to grow over the cicatrices of most or many old ulcers and wounds. I have sought in vain for the appearance of the hair in such cases where the ulcer or wound

had been of any extent, depth, or duration; and this remark must be familiar to every observer. I have lately examined the faces of several individuals severely pitted by small-pox, and could never satisfy myself of any hairs growing from their pits or cicatrices. What inferences can be drawn from these observations? The seat of the bulbs of the hair in these cases has been destroyed by the ulcer or the small-pox, and the root of the hair is completely destroyed along with it. The hair, which is a vegetation from the cutis vera, (and most probably from its inner surface,) must perish with the destruction of the soil that produced or nourished it.

If this theory be correct, it is easy to understand why in most skin-diseases the hair falls off, is temporarily destroyed, but again renewed, because its roots remain unaffected. Thus in porrigo the hair falls off; but as it is again renewed, we are entitled to say that the hair has not been radically affected, and the disease is comparatively superficial, and does not involve all the layers of the skin.

This theory may also, perhaps, shed some little light on the very uncertain seat of cutaneous diseases in general. In almost all, the epidermis is alone affected; and that the cutis vera is not involved, we presume to be proved by the hair not being destroyed irrecoverably.

From the foregoing observations, we may also speculate as to the reason why constitutional symptoms do not accompany all extensive diseases of the skin. In bullæ, for instance, we see innumerable blebs extended over the whole cutieular surface, without the slightest constitutional derangement. It is evident that the epidermis alone is here implicated, and that the sentient extremities of the nerves (to speak theoretically) which belong to the true skin are not exposed to the disease. Hence, in large superficial scalds, we have little disturbance of the system, and deeper burns alone affect the constitution. In scarlatina, &c. it is true we have great constitutional derangement; but, at the same time, we have the mucous membrane internally diseased, to account for this.

The last deduction I have to draw on this subject, is relative to a question which has been often agitated in the physiological world—viz. whether the skin on cicatrized surfaces is renewed precisely similar to the integuments which have never been injured?

The affirmative answer to this question has, of late, been assumed as one of the settled truths of pathology.

The different appearance of the cicatrix—the loss of substance which is always observable, as in pits of small-pox, &c.—the irregularity of the cicatrized surface, and other circumstances, seem to favour the negative of the question. That the new formations of skin are very different from the old, is evident; from the simple fact that the former is devoid of hair on its surface: and why is the hair absent? In all probability, because the layer of skin, or the peculiar part of that layer from which it springs, is never again renewed. I only insist on this as a probability, until a more reasonable solution of the query can be given; because the whole layers of the skin may be there renewed, and yet their functions may be changed or diseased.

Should these remarks have the effect of inducing any of your readers to examine the subject experimentally, my object in writing this note will be gained.—I am, sir,

Yours, &c.

J. B. Thomson.

Preston Dispensary,
June 24, 1834.

Case I.*—*Partial Paralysis of the Face.*

June, 1834.—Robert Sumner, æt. 45, a weaver by trade, had an attack of rheumatism about seven years and a half ago, at which time he felt darting pains occasionally, as he describes them, coming from the neighbourhood of the mastoid process of the left side of the head, and shooting across the face. Shortly afterwards, while sitting at dinner, he observed that he could not spit in a straight direction, the saliva being discharged, as it now is, by the left angle of the mouth. At present he discovers the following symptoms: — There is paralysis of the left side of the face, not a muscle of which is obedient to the will; the right side is drawn somewhat awry, and *inclined downwards;* the opposite, or affected side, is drawn distinctly *upwards*, and *considerably inwards*. The forehead presents a singular appearance, the right half being furred with deep wrinkles, the paralysed side remaining smooth and immoveable

* The two cases which follow have been communicated by Mr. Thomson.—E. G.

under every emotion and expression of mind. When he speaks or smiles, the whole countenance is highly grotesque and ridiculous. The left eye is partially closed by the drooping of the upper eye-lid; is considerably weaker than its fellow; and was subject, in the early periods of the disease, to frequent lachrymation. When he sleeps, the left eye is never fully closed; but he thinks it more so than it used to be, according to the accounts of those who observed him. There remains some motive power in the upper, but none perceptible in the lower eye-lid. For a long time after the attack, his speech was very indistinct; at present it is greatly improved, but still imperfect. When he eats, the masticated food accumulates between the cheek and teeth of the paralysed side of the face, which he is necessitated to remove by his fingers. His hearing has suffered a little, as he supposes, from his complaint, and he is troubled at times with ear-aches. The sensibility of all parts of the face on both sides is quite unaffected. This individual has never had any treatment for this paralysis of the portio dura on the left side of the face, because, he says, it does him no great harm, and never affects his general health. He is quite well otherwise, with the exception of his suffering a little from asthma occasionally. He is a great smoker; and one of the regrets of his disease is, that when he puts the pipe within his left cheek, he cannot draw the smoke. He used also to amuse himself with whistling: he has not been able for seven years to whistle a note. But his principal regret is, that he cannot indulge his fondness for music, being now unable to blow the flute, of which he used to be an amateur.

Remark.—A peculiar circumstance in this case is, that the paralysed side seems to drag the other towards it, the opposite of which is generally found, and might be expected from the action of the muscles.

Case II.—*Acephalous Fœtus.*

May 14th, 1834.—Mrs. O., æt. 32, was in the usual progress of labour, and at the full period of gestation delivered of an acephalous fœtus. She had a fall when about the fourth month of her pregnancy, since which she has never been properly well; and she has been almost starved for want of food during the latter period of her pregnancy. Had five children previously, all alive and healthful.

The eyes of the fœtus are in the vertex; the nose is broad, and almost level with the face; the mouth large; and the whole head (if the comparison be not odious) resembles very much that of a toad. There is scarcely any neck, the head running imperceptibly into the shoulders and body. It is full-grown, and has no other peculiarity except an imperfect development of the genital organs. There is a scrotum containing testes, but scarcely any likeness of a penis. It was still-born, and seemed never to have respired.

Dissection shewed no occipital bone, but very extraordinary attempts at formation in all the other bones of the cranium,—none except the temporal bones, and those of the internal ear, having any distinct, or even distant, approximation to those of the human subject.

An entire absence of brainy matter within the cranium; no cerebrum, cerebellum, or medulla oblongata, or even resemblances, could be discovered. The spinal cord, and other parts of the nervous system, complete. The great sympathetic perfect in all its connexions with the spinal cord.

Remarks.—That all the nerves take their origin from, or are a production of, the brain, as has been assumed by speculative physiologists, is directly opposed by this dissection, in which we found the spinal cord and other nerves of the body as usual, but no cerebral matter. That the medulla oblongata gives birth to the spinal cord, is equally at variance with this dissection, in which we found a spinal marrow, but no medulla oblongata. These observations agree with the investigations of Tiedemann and Serres upon the fœtal nervous system. These acute observers found first formed the spinal cord; then the medulla oblongata, the cerebellum, and, last of all, the cerebrum. From this dissection it also appears that the great centre of nervous communication and commerce, the sympathetic, although it may communicate with the sixth pair, is not an offset, or branch, originating from these, as some have said, but may exist, as in this case, independently of the whole cerebral *materiel.*

CASE OF

STRANGULATED HERNIA, INSIDIOUSLY FATAL.

To the Editor of the Medical Gazette.

SIR,

PERHAPS you may consider the following case of hernia of sufficient interest for publication in your journal. It appears to me of considerable practical importance, because it shows how closely a patient who has experienced the least uneasiness from an irreducible hernia ought to be watched by those about him, even in the total absence of pain or other symptoms indicating constriction of the protruded parts; and also because it shows that after those symptoms have taken place, the patient, as in this instance, may live but a very few hours; and therefore it is one of those cases which strongly argue the propriety of operating for the release of the incarcerated parts immediately upon danger becoming apparent.

William Clarke, æt. 63, a somewhat debilitated and rather imbecile sort of man, residing at No. 5, Coles's-place, Elder-walk, Islington, was admitted under my care, at the Islington Dispensary, on Thursday, June 5th, 1834. I first saw him at 3 P.M. on this day (from sixteen to twenty and a half hours before he died). There was a tumor on the left side of the upper part of the scrotum, about an inch and a half in width, and about two inches in length, a continuation of which might be traced upwards through the external abdominal ring into the inguinal canal.

There was little or no impulse communicated to the tumor when the patient coughed; but still its form and situation were sufficient to satisfy me as to its being hernia. The patient, moreover, said that he had been ruptured for the last six or seven years. I endeavoured to reduce it; but after making considerable pressure for about a quarter of an hour, I found not the least diminution of its bulk. During this time, I repeatedly asked the patient if he felt any pain from the pressure which I made, but he invariably declared that he felt none.

There was no distention of the abdomen, but pressure on this part caused some slight degree of pain; there was no sickness (save that the patient had vomited once the day before); the tongue was clean; the pulse soft, and about as quick and full as an ordinary pulse. I observed no anxious or desponding expression of countenance. His bowels, I understood from him, had last been open the day before, or on the Tuesday.

Four or five hours before I saw the patient, he had been at the Dispensary, and had been seen by the resident medical officer, who informed me, that after making pressure for about half an hour, he had reduced the greater part of the hernia, but that there still remained a something in the sac; that the patient expressed no pain from the attempts made at reduction; and that there were at that time (twenty-five hours before he was found dead) no symptoms which could induce him to suspect strangulation.

With regard to the previous history of the case, I could make out but very little. I understood from the patient that the hernia had existed for the last six or seven years; that two days before I saw him, he had received a blow, as far as I could learn, either on the abdomen or on the hernia; that the hernia had since that time caused him some uneasiness, and had increased in size, but was not then (when I saw him) so large as when he went to the dispensary four or five hours before. I could gain no satisfactory information from the patient as to whether he had usually been able, until the last two or three days, to return the whole of the protruded parts into the abdomen.

The hernia having existed for many years, the absence of all symptoms indicating constriction of a portion of intestine, and the degree of firmness about the tumor, induced me to think it probably an old omental hernia, and perhaps adherent. Under these circumstances, I directed the patient to go to bed, to take a small dose of castor-oil, with tincture of senna, and in case of vomiting or pain coming on, to send immediately to the Dispensary. I also explained to some persons living in the house the necessity of so doing. From these persons, and from other inmates of the house, I received the following information. The medicine was given soon after 3 P.M. of Thursday, and repeated at eight, as the bowels had not

been acted on by the first dose. Between these hours of three and eight, the patient had three times walked up and down stairs, from the second to the ground floor, as quick and as well as usual. At 8 P.M. on Thursday (fifteen hours and a half before he was found dead) he was last spoken with; when he locked the door of his room, saying that he had no pain, that he felt himself better, and should want nothing more that night. He, however, was heard groaning during the whole of the night, from twelve till about seven the next (Friday) morning, when he was heard to get out of bed; after which he was neither heard to move or groan. The door of his room was broken open, at half-past 11 A.M. of Friday, June 6th, when he was found dead—his body cold: so that he must have died from sixteen to twenty hours and a half after I had seen him, and from eleven to fifteen hours and a half after he was last spoken with, and had said that he was in no pain, and felt himself better. He probably died about the former period, as his body was quite cold. The medicine had not operated, nor had he vomited. It is right to state that the persons who heard him groaning were not those to whom I had given directions to send immediately to the Dispensary in case of pain coming on.

On examining the body (from twenty-five to thirty hours after death), I found a common inguinal intestinal hernia. A loop of about five inches of small intestine was in the hernial sac, at the mouth of which the internal abdominal ring made firm constriction on the intestine, the coats of which were much thickened; and though not in a state of sphacelus, yet from their colour (livid, with a dark chocolate tint) they seemed fast approaching that condition. The strangulated portion of intestine was but little distended, either with air, secretion, or alimentary matter. There was in the sac, perhaps, an ounce and a half, or two ounces, of a dark, reddish brown serum. The peritoneal coat of the sac was somewhat inflamed, being in places of a bright, though not deep, red colour; it was firmly adherent to the outer coat of the sac, which was of considerable thickness. Slight and recent adhesions were formed between the sac and intestine, and to the latter were attached some shreds of lymph. In the abdomen there was a small quantity of serum: the intestines were distended with air, though not excessively so; they contained a yellowish fluid, were in parts of a pale red colour, and their vessels much injected with red blood, as also were the vessels of the mesentery. The peritoneum lining the walls of the abdomen was not in the least inflamed.

One point in this case is clear, viz. that if I had operated when I saw the patient, his life would very likely have been saved; for at that period there could not have been constriction of the protruded portion of intestine so great as to have put a stop to the circulation of blood in it, otherwise it must have become sphacelated, as the patient lived at least sixteen hours after I had seen him. I think it is also clear that (at the time I saw the patient) it was impossible to perceive that an operation was necessary.

I have the honour to be, sir,
Your obedient servant,
WM. BEAUMONT.

2, Manchester-Street,
Manchester-Square,
June 26, 1834.

ANOMALOUS LABOUR CASE.

To the Editor of the Medical Gazette.

SIR,

IF the following case be worthy of notice, you will much oblige me by inserting it in your valuable journal.

I am, sir,
Your humble servant,
JOHN F. HARDING.

St. Bartholomew's Hospital,
July 7, 1834.

On July 1st, I was sent for to attend M. A——, a poor woman, residing in Whitecross-street, St. Luke's. On my arrival, at 11 P.M., I found her walking about the room. On inquiry, she told me that she was in her ninth month of utero-gestation, and that the labour pains had come on that day at 1 P.M., accompanied with a discharge of fluid quite transparent, and of a chocolate colour, with a few coagula of blood; the fluid amounted to about three pints. As the pains recurred every five minutes, and rather severely, I requested her to lie on

the bed, that I might make an examination, to ascertain the progress of labour, when I found the os uteri dilated to the size of a shilling, but could not make out the presentation: it appeared as if the membranes were partially protruding a little on the right side of the cervix uteri. The pains still continued regular every five minutes for two hours, when they became more frequent, but not so violent. I again made an examination, and found the os uteri more dilated; after which the pains became much less frequent, and less strong than before; but the parts being relaxed, I was enabled to ascertain their condition more accurately, and discovered, as I imagined, the placenta attached a little above the os uteri. The pains came on now every quarter of an hour, and very languidly; but the parts being relaxed, the uterus very high up, I gave the patient ʒij. ess. secale cornut. with a good effect, causing the uterus to contract. I deemed it not judicious to administer a second dose. The pains now were every half hour, and very feeble. The patient afterwards slept comfortably for above an hour; but awaking with a pain, and being very restless, I gave her tr. opii, ℥xl., which had the effect of composing her for some time, when I left her, desiring I might be called if required. It was now 5 A.M.; three hours after which I called again, and found her in *statu quo*; but in the interim about a quart of fluid was discharged, accompanied with pain. There being no unfavourable symptom, I left her again for four hours. On my arrival again, the nurse told me that there had been a still greater discharge of five pints, which I examined, and found of the same description, but rather fetid; but as there was no appearance of labour being complete for some time, I requested, if any change took place, to be sent for, and on the following morning (Thursday) I was summoned, when I found the patient much exhausted —almost prostrate. The pains had come on very suddenly. I immediately made an examination, and found to my surprise the vagina blocked up with a large membranous sac. On passing my hand beyond it, and finding it detached, I dislodged it. The liq. amnii had previously escaped; a few coagula came away; afterwards about half a pint. Fearing hæmorrhage, I immediately pressed my hand on into the uterus, and found it emptied; this membranous sac was of a pyriform shape, with a very narrow cervix, consisting of three membranes, and capable of containing about five quarts, but only four pints of firm coagula were found in it. At its fundus was merely a tapering cord, about three inches in length. There were also some gritty particles intermixed with the coagula, but no signs of any fœtus or placenta. No hæmorrhage occurred. Got the patient into bed, and left her a little revived.

Ordered her Tr. Opii, ℥xl. statim, e. Potas. Nitrat. grs. x.

July 4th.—In the morning, on visiting her, she was in a most alarming state. The pulse very low; scarcely perceptible, although quick. The tongue coated with a dry crust in the centre, with a marginal redness; the papillæ erect. Conjunctiva much reddened. Delirious, and twitchings of the levatores anguli oris alæque nasi muscles. Respiration quick and hurried; skin hot; passed but little water, and with lancinating pains. The abdomen much distended, and exceedingly painful, particularly over the hypogastric region. Suspecting the bladder to be distended, I wished to have passed the catheter, but the patient objected. I ordered hirudines, xxiv. abdominis regioni hypogast.; after which, constant fomentations of warm water.

Ext. Coloc. Co. gr. x.; Hyd. Submur. gr. iij.; stat. s; Sp. Æth. Nit. ʒiij.; Syr. Papav. ʒiv.; Liq. Ammon. Acet. ℥iss.; Mist. Camphor, ℥iiiss.; capiat Coch. ij. Magn. 3ta. vel 4ta quaq. hora.

The pains not abating, I requested the leeches to be again applied, from which she derived relief in every respect; but after many applications, sickness was allayed with the effervescing draught. I also persevered with the calomel and colocynth, which removed most offensive motions. The patient is now doing exceedingly well.

N.B. The account she gives is, that she had every symptom accompanying pregnancy; has had three children and one miscarriage. About six weeks after her supposed conception, she met with a severe blow on the abdomen, and the day previous to the occurrence of these pains, fell on her side; she also says that she has had an offensive smell from the nose, with an unpleasant taste in the mouth,

for some months; the abdomen was very livid, as were the nails; had also cold shiverings frequently; never passed above a tea-cupful of urine at a time, and with pain; the bowels were also very costive. All these symptoms have entirely left her. The quantity of fluid discharged altogether amounted to two gallons.

REMOVAL OF AN IMPACTED PLUG FROM THE NOSTRIL.

To the Editor of the Medical Gazette.

SIR,

IN your valuable journal of the 21st instant, there is an article under the title of "Contrivance for Extracting a Plug from the Nostril," which the author of the paper seems to consider an *improved* mode of practice; perhaps it may be so to many of the profession, though none to me, as I have been in the habit of using an instrument for that purpose—with which every surgeon ought to be supplied—viz. a female silver catheter inverted. I am the more anxious to submit to the public my mode of removing the plug, when impacted in the nostril, from my being *the surgeon* who did *not* attach any posterior thread to the plug, as mentioned in the above case of your correspondent M. D. It was, however, no omission in me, as I always considered the posterior thread a very great annoyance, as well as risk, to the patient,—more especially when I could always remove the plug, both safely and easily, from its situation, however impacted, by passing the threads (attached to it by the anterior parts of the nostril) through the large opening of the female (silver) catheter, and gently introducing the instrument into the anterior opening of the nostril, until it came in contact with the plug, thus forming by its strength and curvature a most efficient cannula. Then twisting the threads around the end of the index finger, I brought my thumb to the (perforated) end of the catheter, so as to assist the finger, and cause pressure on the plug by the end of the instrument, and with the least possible force could always remove the plug into the fauces; and the threads being cut close to the instrument, the catheter was easily withdrawn from the nostril.

The patient might then spit out the plug, or it might be lifted out of the mouth by the finger and thumb, or by means of a pair of forceps. The reason why I did not remove the plug in the above case by this mode, was my not being in attendance at the period when it was removed. After I had plugged up the nostril by the posterior aperture, I refused to continue my professional attendance on the patient, owing to his having consulted Dr. A——n *twice* in the forenoon of that day, without advising with me on the propriety of such measures,—which I considered as rather unprofessional in the physician, and extremely rude in the patient.

The patient was afterwards attended by the above physician, and by the medical gentleman who always had attended the wife of the patient during parturition—both of them very respectable gentlemen, to whom the professional world is indebted for the contrivance for extracting the plug from the nostril. The term extracting is, in my opinion, not well applied in this case, inasmuch as the bougie only displaced or removed the plug from its situation, but did not draw it out of the nostril (as extraction means); no, not even when removed from its impacted situation.

I would not, Mr. Editor, have thus encroached on your valuable pages, but from a remark in the concluding sentence of your correspondent's letter, which might have an injurious effect on my practice—viz. "Thus an *accidental omission* may lead to an improved practice," &c.; thereby intimating a neglect on my part; and as such neglect is, by the public, considered more frequently to proceed from want of skill than from want of attention—to the great detriment of any practitioner—I therefore hope you will insert this short paper, and allow me once more to affirm that the want of a posterior thread in the aforementioned operation, *was no omission on my part*, but in consistency with *my* improved mode of practice; the superior utility of which I beg leave to submit to the decision of the profession.—I remain, sir,

Your obedient servant,

THOS. BUCHANAN.

Hull, June 26, 1834.

OXYGENATION OF THE FŒTAL BLOOD.

To the Editor of the Medical Gazette.

Sir,

In attempting an answer to the inquiry of Mr. Phillips (contained in the Medical Gazette of June 28th), respecting the decarbonization of the fœtal blood, many difficulties naturally present themselves, arising from the almost impossibility of obtaining direct evidence of the facts—from the obscurity thrown over many of the functions of uterine life—and from the yet unsettled physiological question of the doctrine of respiration. Dr. Robert Lee, by his recent experiments, has succeeded in deciding the long-contested point regarding the double placental circulation. There is now no doubt of the fact, that the embryo is not only nurtured by its own blood, formed with its body, and increasing with its growth; but it also derives nutriment from the maternal blood by venous absorption. The gross *materiel* of the mother's blood can never enter the placenta, owing to a mechanical obstruction, the *membrana decidua* being interposed between the umbilical vessels, perfectly entire, and, when unruptured, presenting no perforations for the admission of vessels. "The placenta," says Dr. Lee, "is a single organ, made up entirely of a congeries of umbilical vessels, not containing a maternal portion, or possessing cellular structure."

But the blood of the fœtus must undergo some change, or vitality could not be supported. Dr. Ramsbotham, in summing up the result of his experiments, remarks, "The double circulation, then—that of the uterus and that of the placenta—are perfectly distinct from each other, and appear to be carried on in the following manner:—the spermatic and uterine arteries, entering the uterus in the most tortuous manner possible, are determined, in the greatest number, and of the largest calibre, to that spot where the placenta has attached itself, and speedily terminate in the uterine veins or sinuses by larger extremities than are observed in other parts of the body. The veins having received the blood, which has as yet *not become vitiated by the loss of its vital properties, detain it some considerable time* in their tortuous and dilated trunks, and reconveying it to the maternal system, deprived of those principles of life and nourishment which it has afforded to the fœtal vessels."

The latter assumption, as a general principle, we know to be contrary to all analogy; and it is probable there is some mistake in its specific application. The blood, the moment it has escaped from the extremities of the arteries, and is yet in the venous capillaries, has become completely carbonised, and is as totally unfit for the purposes of life as it is when it reaches the right side of the heart. There appears something in the very constitution of a vein repugnant to the *retention* of arterial blood. The magnitude of the vascular orifices, and the state of irritability into which the gravid uterus is thrown, will not go far in elucidating the theory of a *stagnation of blood*. The change which the blood undergoes, or effects, here, as in other parts of the system, takes place at the moment of its transmission from the arteries to the veins. The appearance of "*cells*," which the identifiers of the placenta with the lungs have observed, has been proved very satisfactorily to be fallacious.

As to the change, and the method by which that change is effected, in the fœtal blood, I conceive we may resort to Dr. Stevens's theory of respiration for a partial analogy; and this leads me indirectly to the question of Mr. Phillips: "Does the blood absorb oxygen in its passage through the lungs?" According to Stevens, this does take place; but not in the manner generally explained. He says, "the absorption of oxygen is not, as is supposed by most writers, the first step in the process of respiration; for the first effect of the oxygen in the lungs is instantly to attract the carbonic acid and bring it to itself; but carbonic acid does not possess any attraction for pure air, and for this very reason, blood which is saturated with this acid cannot attract oxygen into the circulation. In the commencement of the process, the air is the active agent, and removes the acid from the circulation; but when this is effected, the *blood* then becomes the acting power, and *attracts* a portion of atmospheric air, which contains oxygen and nitrogen, equal in quantity

to that which had been used in the extreme circulation*." But although this may be, and is doubtless, the case with regard to atmospheric air, it has never yet been proved that pure *blood* possesses the property of abstracting all the carbonic acid from venous blood, and of rendering it arterial. We know that oxygen exists in the circulation, but it seems scarcely probable that enough is absorbed by the lungs, or that a sufficiently *extensive surface* of pure blood is presented to the mass of the impure fœtal circulation to render it *completely arterial*. I am of opinion that comparatively little is requisite. If fœtal blood in the lower animals be examined, it will be found to differ very little in the arteries and veins throughout the body. In the one there is none of that bright, florid, arterial scarlet; nor in the other the dark, or modena red, of breathing animals: it appears to keep a middle course between the two extremes, and it would have been reasonable, *à priori*, to expect that such would be the fact. The wear and tear of independent existence has not yet commenced; the carbonised solids are not removed with rapidity; there is little occasion for the evolution of superfluous animal heat; no breathing is required, and but slight exhalation from the skin. Blood of the same properties is sent into the lungs, which circulates through the body. The animal is merely vegetating; and although we are justified in the conclusion that the blood of the embryo is acted upon by the maternal fluid, and undergoes some slight change, we are not warranted by facts in admitting the theory of the oxygenation of fœtal blood to its full extent.

I have the honour to be, sir,
Your obedient servant,
R. H. Allnatt, M.D.

Wallingford, July 8, 1834.

CONNEXION BETWEEN THE PLACENTA AND THE UTERUS.

To the Editor of the Medical Gazette.

Sir,

In the notice to correspondents, in your journal of the 21st instant, it is announced "that Dr. Robert Lee is still pursuing his inquiries on the subject," and that you "have not learned that there has been any change of opinion." What this "*subject*" may be is left to the conjecture of your readers; but as it refers to an "opinion" in which there has been no change, (a mode of expression which implies that it has been the subject of controversy,) it is probably connected with the structure and office of the placenta. If this be so, I am at a loss to reconcile what you are at present "enabled to state," with a former assertion, which seemed equally upon authority, in your review, exactly a twelvemonth since, of Velpeau's splendid work*. It is there stated, "that Dr. R. Lee, to obviate any fallacy, examined a gravid uterus in the eighth month, in which he had previously coagulated the maternal blood. He was able to satisfy himself and Mr. Lawrence, who was present at the examination, that coagula of the maternal blood extended from some of the openings in the lining membrane of the uterus into canals formed by the deciduous membranes on the margin of the placenta. These vessels or channels in the decidua could be traced only a short distance along the margin of the placenta, and between the lobes."

It cannot but be in the recollection of your readers, that Dr. R. Lee had previously stated the decidua to be an impervious membrane, and that no portion of the blood of the mother found its way into the placenta; and it is scarcely necessary to point out the discrepancy between these two opinions. To which, then, I would ask, does this eminent physiologist now adhere? Confiding in the accuracy of your representation in 1833, I was pleased with the candour which dictated the apparently authoritative announcement, as it proved that Dr. R. Lee was disposed to yield his former impressions, when subsequent observation had convinced him of his error, and to follow the truth whithersoever it might lead him, and as it tended to establish the accuracy and fidelity of William Hunter, who had previously described the same canals, which he considered to be veins. "All around," says this distinguished British anatomist, "at the very edge of the placenta, there are a

* Stevens on the Blood, pp. 54, 55.

* Medical Gazette, June 22, 1833.

great number of these veins; and many of them run a little way in the direction of tangents to a circle, in the very angle between the membranes and the placenta. When they have been filled by injection, while the placenta and membranes adhere to the uterus, and in that state are seen from the inside through the amnion and chorion, many of them appear evidently to collect their smaller branches from the adjacent parts of the decidua and of the uterus, and throw their trunks into the placenta*."

These canals may be always traced about the margin of the placenta, but they exist equally over its whole surface, although they are frequently rendered indistinct by compression, either from the action of the fibres during labour, or from forcible injection after death. They are mentioned by Dr. Ley, in his paper on the Placenta, and represented by him as formed of lamellated decidua, and as leading obliquely from the placenta to those apertures upon the surface of the uterus to which Dr. R. Lee has so particularly adverted, but which had been previously described by Albinus, Sanctorinus, Wallace Johnson, Morgagni, Astruc, Buxton, Donald and Alexander Monro, Roederer, and almost every writer of eminence of the last century. The same deciduous canals were soon after still more minutely described by Messrs. Stanley and Mayo in their joint communication; and these experienced anatomists, after examining minutely preparations in the Museum of the College of Surgeons, adopted the conclusion, that "there exist, formed in the decidua, and terminating on, or extending into or through the substance of the placenta, regular channels, one class of which is continuous with and receives blood from the uterine arteries, while the other is continuous with and returns blood to the uterine veins."

This statement, which, from the alternative propositions it involves, wants much of the usual perspicuity and accuracy of these generally lucid writers, admirably coincides with the account of the Hunters, corroborates their views derived from the same preparations, offers a most important tribute to their correctness and truth, and strengthens the more recent, in preference to the former "*opinions*" of Dr. Robert Lee upon the subject.

It may now, therefore, be considered as satisfactorily ascertained, by the concurrent observations of all British anatomists and obstetricians, that maternal blood does find its way into the substance of the placenta; and, if so, it is equally clear that it must get back again; and I believe, moreover, with the Hunters, Burns, Ley, Stanley, and Mayo, that in conformity with the usual mode of circulation in the human body, blood is poured into the placenta by very small arterial trunks, and that its return is secured by those deciduous canals, which, like the holes upon a sponge, fix themselves to the open orifices of the uterine veins, into which the blood, agreeably with the description of John Hunter, is squeezed by the gradually increasing distention of the placenta.—I am, sir,

Your obedient servant,
H. Hughson.

London, June 27, 1834.

[We must leave Dr. R. Lee to speak for himself.—Ed. Gaz.]

* Anatomy of Gravid Uterus, p. 45.

MEDICAL GAZETTE.

Saturday, July 12, 1834.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."
Cicero.

THE WARBURTONIAN PROCEEDINGS about MEDICAL REFORM.

The parliamentary session fast approaches to a close, and the Committee appointed to inquire into medical affairs still continues its weary labours. People begin to grow impatient, and ask one another when those labours are likely to have an end. No small alarm, indeed, would seem to be entertained by those who, looking at the number of witnesses already examined, the list of persons still to be examined, and the apparently endless arrivals of parties from all quarters, anxious to give evidence, can see no probability of any thing like

a speedy termination of these preliminaries for wholesale law-making. We believe, however, we are warranted in stating that the government have become alarmed at the great expense* already incurred, and that we shall very soon have to announce the close of this protracted inquiry.

We are, we must confess, of the number of those who anticipate no great good—at least none at all commensurate with the pains taken and the time expended — from the multiplication of persons now every day admitted to testify and state their volunteered (if not gratuitous) opinions. We had hopes, in the commencement of the proceedings, that a few months would amply suffice to obtain all the evidence needed for founding a systematic and legalized arrangement of the profession, and of professional education; and seeing the abundance of information procured by the Committee during the first few weeks of its sitting, from the examination of the heads of the profession in England, together with the most eminent general practitioners that the country can boast of, we expected that not only the foundation was laid, but that the superstructure already was in a great measure advanced, of a system likely to give general satisfaction. Months and months, however, have since gone by, and we cannot find that any thing more has been done than allowing a number of obscure teachers about town, and many more gentlemen than was at all requisite from the provinces, to come forward and to tender testimony which we rather fancy will look not a little odd in print: but to appear in print will be to the parties, no doubt, their great reward; and whether repetitions *ad nauseam*, or self-contradictions, be among the beauties most prominent in what they have said, no matter—their end is answered. They will have appeared "before parliament;" their "suggestions" will have been accepted, and placed upon record at the public expense.

It is surely not to be wondered at that many have formed the notion, that it has long since been determined to proceed no further this session than to receive evidence. As it is, the time has gone over, and is nearly expired; and we doubt what good account can be given of it. The Committee nominally sits almost daily; but it is Mr. Warburton alone who can be said to constitute that body; he is the Committee in his own person; seldom have we found him assisted by above one or two members at a time, and they more like casual visitors than persons engaged in a serious inquiry: frequently the chairman has been without a single assessor; and so little, indeed, do the proceedings seem to attract the attention of any body of late, that with the exception of a few loungers at the bar, Mr. Warburton, the short-hand writer, and the witness, have been the only occupants of the Committee-room. Matters were in a very different state at first: the novelty of the thing, and the importance of the persons called on to give their evidence, in the earlier portion of the inquiry, used to attract a daily crowd; but now, what with the tediousness of the process of examination—and truly it is tedious to an extreme degree—and what with the eternal sameness of the questions and answers, unless where some merely local arrangements, only interesting to parties immediately concerned, are gone into, it is not strange that the benches should be deserted by all who are not obliged to attend, and that the medical public should cease altogether to take any interest in the business, fur-

* Besides the immense mass of evidence to be printed, the witnesses from a distance are allowed their travelling expenses, and a guinea a-day.

ther than to inquire occasionally if it will ever be over, and when there is any likelihood of the Committee proceeding to report.

Here, however, there seems first a certain small difficulty to be overcome. The mass of evidence already collected, and which, if it be worth any thing, will require to be well sifted, what glutton of the first magnitude will undertake to digest? The labour of amassing the material is nothing compared to that which will be required for rendering it available; and if Mr. Warburton has found it impossible to keep his Committee-men together, to collect materials for the banquet, what is to become of him when he is left all alone to devour the numerous dishes? If he decline the task through utter inability, after having fairly attempted it, nobody will be much surprised; but if he do not wish to get himself into such a predicament—if he have made up his mind to reject the job, should it be attempted to impose it on him alone—why go on adding to the accumulation of difficulties?

But suppose Mr. Warburton, with the spirit and perseverance of another Hercules, should undertake the whole digest, have we reason to expect that much real good will accrue to the profession? The labours of the Committee, in appearance only, but actually the labours of Mr. Warburton, will of course be tendered to parliament with all due formality: but what great importance can be attached to the result of the deliberations of a single mind, engaged on a subject that would require the united co-operation in council of at least a dozen of the cleverest heads in the country? What hope can there be, under these circumstances, that any arrangement will be arrived at which can give satisfaction to the whole body of the profession? It will certainly be as surprising as gratifying to us if it should so happen; but we hesitate to be very sanguine about it. The chances cannot but be very unequal where so monstrous an undertaking devolves on an individual, and where the legislation for a whole profession is left mainly to depend on the ability of one man. We have as high an opinion of the talents of the chairman of the Medical Committee as any of his best friends ought to have: we know that the great experience he has had for years in the concerns of medical polity, and the deep attention he has bestowed on the external arrangements of the profession, render him perhaps the fittest man in England for the important post he occupies. But all this sort of qualification has its limits: there are circumstances which beset an individual so situated, from which it is not easy for him to extricate himself. The temptation, so captivating to human nature, to overvalue certain opinions, good enough in their way, but not so supremely and intrinsically excellent as to warrant the extravagant value set upon them, is one danger to which a person in the condition of the honourable chairman is exposed. But this is perhaps not the greatest danger: we can imagine a highly-gifted man placed in some such extraordinary predicament as that of an enchanter, who calls up powerful spirits by his charms, and, growing more and more bold, and beside himself, will have at last the whole legion up before him. They come in multitudes, and encroach as much as they dare do on the little protective circle, within which they may not pass. Unhappily, he grows confused, forgets the counter-spell by which he may dismiss his visitants: they perceive him at fault—grow bold in their turn—make strange demands upon him—and threaten to work him mischief, which they will do if he does not recover his potent spell and *lay* them.

We cannot, in truth, see the host of witnesses pressing forward from such various quarters—from city, town, hamlet, village, and open country—from

beyond the "vasty deep" itself—from the sister kingdom—all thronging to the chamber at St. Stephen's—without thinking of our friend the enchanter, and trembling for him lest he should not retain his power over his visitors when they arrive. Verily, we apprehend these proceedings will have the effect of bringing out from the shell many an embryo medical politician: and the hope of throwing light upon the present difficult state of things in the professional world, by being permitted to give evidence before parliament, has already, we fancy, acted as a powerful stimulant on many who happened to have the mischievous seeds of ambition for notoriety lurking within them.

It of course rests with the Committee, or rather with Mr. Warburton, its prime and almost its sole mover, to determine whether this system shall at once be ended, or spun out yet a little longer. In our humble opinion, enough, and more than enough, has already been effected to enable the Committee to proceed with the other part of their weighty duties,—to enter upon the investigation of the mass of evidence now before them, and to think of drawing up their Report betimes. Enough surely has been done in the way of *conciliation*, by allowing some of the very meanest of our contemporaries to think themselves of importance, because suffered to volunteer and vent their flippant nothings before the public. It is a harmless thing, to be sure, in some sense, that such a course should be adopted; but we would put it to those who have the power to order it otherwise, whether their time and patience might not be better employed?

We happen to know that the sense of almost all the well-informed members of the profession on this point is, that enough was long since done, or might have been done, to allow of things by this time being brought into a train of considerable forwardness; and that it will be in the highest degree unsatisfactory should parliament break up without any sign of a practical issue,—should the session conclude without any thing in the way of medical arrangement being concluded. In short, we cannot close these remarks on the proceedings in the immediate vicinity of St. Stephen's chapel, without deploring the lapse of so many months since they were entered upon,—proceedings which still have no visible termination, but which we are not without hope may, by a little decisive energy, in accordance with the *hint which has been given in the proper quarter*, be speedily brought to the conclusion of what may be considered their first act. Several other acts remain to be performed before the piece be completed. For this we are fully prepared; but time wears on apace; a prorogation of parliament cannot be far off, nay, even a dissolution (which, we understand, would prevent the evidence from being officially available) is possible; and surely the managers will not commit such an error as to spin out the first division of their performance for the sake of allowing some of the small fry of actors to tire the patience of the public, until the curtain abruptly falls, leaving the hope-sick audience to go their ways in dudgeon and discontent.

DEATH OF ST. JOHN LONG.

Mr. St. John Long died at his house, in Harley-Street, on Friday, the 4th instant, of what the newspapers have been instructed to call hæmorrhage from the lungs, but which we have reason to believe was common pulmonary consumption, accompanied by spitting of blood. He was unquestionably the most successful quack of the day—at least in one sense of the word—while the complaint which proved fatal to him forms a curious commentary on the blind and imbecile credulity of his dupes. He could not arrest in himself the effects of that disease which he gained a fortune by pretending to cure!

SURGICAL REPORTS
FROM THE
LONDON DISPENSARY.

By R. R. Robinson, Esq.
Surgeon to that Institution.

Case I.—*Incipient Inflammation of the Breast from distention of the lactiferous tubes —Abscess prevented.—Excoriated Nipples.*

Anne Russel, æt. 34, admitted April 25th, is suckling; has had irritable excoriated nipples some time.

Ung. Zinci. Lot. Plumbi. Mist. Cath. Ant. bis die.

May 20th.—She did not apply again until to-day, when she stated that the nipples soon got well under the means employed, but complained of great pain, redness, and swelling of the right breast, which is exceedingly hard, in a great measure from excessive distention of the lactiferous tubes. Glasses were applied to the breasts, by which much milk was removed, and great relief afforded: some fever.

Lot. Plumb. Pulvis purg. statim.

23d.—Pain and inflammation entirely gone. This day discharged, cured.

Case II.—*Mammary Abscess, unconnected with Lactation, speedily cured by puncture.*

Mary Jones, æt. 34, admitted 25th February; not married; has had pain, redness, and hardness of the right breast several days. There is now a circumscribed abscess at the inferior parts of this breast; skin covering it very tender to the touch; some fever. A lancet was put into it, and upwards of half an ounce of healthy pus evacuated, with relief.

Mist. Salin. Ant. ter die; P. P. ℈i.

March 1st.—Much easier; inflammation considerably diminished; discharge trifling; parts consolidating.

8th.—Quite easy; puncture healed. Discharged.

Case III.—*Simple Mammary Abscess during Lactation, cured by puncture.*

Mary Agar, æt. 27, admitted the 25th March; confined five months ago; has had pain, redness, and swelling, in the right breast three weeks; there is now a circumscribed swelling in the upper and outer part of this breast, with a distinct fluctuation. An incision was made, which let out ℥i. of genuine pus, but no milk. No fever.

Catap. Panis. Support. Mist. Salinæ, ℥iss. ter die.

March 28th.—Pain gone; discharge trifling; no milk escapes; no sinus.

April 1st.—Wound healed; parts consolidated. Discharged, cured.

Case IV.—*Two Mammary Abscesses, one opened by the lancet, the other naturally—Fistula prevented.*

Mary Robbins, æt. 30, admitted 25th May, was confined thirteen months ago; child still at the breast; has lately had a severe attack of influenza. Four weeks ago both breasts became tender, the left particularly, which also became tense and inflamed; and, in spite of repeated leeching and the application of cold, an abscess formed above the nipple, which, being opened, discharged a teacupful of healthy pus, without milk; fainted from the operation; great prostration of strength and night sweats.

Cat. Panis. Pulv. Cinchon. ℈i., ex vino rubro, ter die.

June 5th.—Wound continued to discharge pus for a week, and then a few drops of milk passed without any matter; the poultice has been continued; the hardness and pain are almost entirely gone; the wound is perfectly healed. She is considerably stronger; in fact, convalescent.

November 10th.—Another abscess formed in the same breast, at the lower part; to this a pitch plaister was applied, by the advice of a friend; and, after being for a long time very painful, it extended upwards, and broke at the part where I opened the former abscess: the opening made by nature was very large; the matter was evacuated very copiously, and it healed up more quickly than usual; but this breast was for some time afterwards painful: the breast is now quite well, and her health very considerably improved by the bark and wine.

Case V.—*Mammary Abscesses—Mammary Fistulæ, cured by pressure, &c.*

Raga Harris, æt. 28, admitted 9th of September; she is suckling; much out of health; she has had inflammation, with much pain in the right breast, fourteen days; this nipple is retracted; the child, therefore, can only suck at the other; right breast much inflamed still; skin very red, and tender to the touch; there is an extensive abscess at the external and lower part; an incision was made into it, which gave exit to ℥ij. of whitish pus, and afforded great relief; some fever.

Cat. Panis. Support; Mist. Cath. Ant. bis die.

September 12th.—Two fresh abscesses have formed in the same breast; one is situated at the upper and outer part,

which has broke by a pinhole, and discharged pus; the skin, to a slight extent around, is soft, red, and destroyed; the other is intermediate between the two; both were freely opened, and 2 oz. of pus discharged; greatly exhausted; former abscess healed.

Ol. Ricini, p. r. n., Quin. Sulph. gr. i. Acid. Sulph. Dilut. m. x. ter die; good diet; Cat. Panis.

Oct. 11th.—Very much improved in health; pain and inflammation gone off; wound of the middle abscess soon healed; the upper one, which broke of itself, still discharges a healthy pus, and now and then a little drop of milk, which comes from a sinus, extending from thence to the lower internal part of the breast. Pressure by plaister and bandage.

Nov. 17th.—Sinuses obliterated by one dressing; wound healed. She is now quite well.

Case VI.—*Abscesses in both Breasts—Numerous Mammary Fistulæ cured by Pressure, &c.*

Elizabeth Cranc, æt. 18, admitted Jan. 15th. Delivered of her first child six weeks ago; a difficult time. She had a large supply of milk, but the child would not suck much. The breasts swelled, and were very painful, but were not drawn. Right breast became the seat of abscess, which, a little before Christmas-day, broke, and discharged milk and pus. An abseess has formed also in the left breast, which broke a fortnight ago, and discharged in a similar manner. Both breasts are now very tender to the touch, and still contain milk; a small wound, covered by strumous granulations, on the outside of the right; and a similar, though larger, wound, occupied by a slough, on the outside of the left breast. An abscess appears also to be forming at the upper part of the right breast. Very pale, weak, and debilitated; sick upon the slightest motion; no appetite; nights restless; bowels costive; scalding on micturition; the vagina is tender to the touch, and discharges a muco-purulent fluid; bearing down of the uterus when she gets up.

Injectio tepida; Ol. Ricini, p. r. n; Mist. Effervescens, ter die; Cat. panis.

Jan. 17th.—Easier; less discharge from the vagina; sickness gone. A distinct fluctuation at the upper part of the right breast, from whence there was discharged, by puncture, ℥iv. of pus.

Inf. Gent. Co. ℥iss. ter die.

20th.—Easier; stronger. Copious discharge of pus from the wound I made, and of milk and matter from the others. A glass applied to each breast evacuated some milk.

Lot. Plumb.; bandage; rep. Med.

24th.—Stronger; breasts considerably easier. Copious discharge from both. Another abscess has formed at the upper and inner part of the left breast, which discharges itself from the lower opening.

30th.—Breasts continue to discharge large quantities of pus, with a little milk; sinuses extend from top to bottom of both breasts; copious purulent discharge from the vagina. In very great poverty; unable to procure proper nourishment. I have therefore advised her to go to the London Hospital.

Feb. 25th.—Admitted under Mr. Scott. By the nursing and care of the hospital, her health has very considerably improved. Discharge from the vagina has subsided; breasts free from inflammation, but the sinuses discharge pus freely; they have been dressed, and pressure applied by plaisters and bandage.

March 14th.—Inflammation and hardness considerably diminished; sinuses closed; discharge almost entirely subsided. General health perfectly established.

Remarks.—I have narrated these cases with a hope that they may prove useful to your junior readers—as they show, in a series, the different stages of one of the most distressing and troublesome diseases incident to the female. The first case illustrates the cause which most frequently produces mammary inflammation and abscess—namely, over-distention of the lactiferous tubes; it shows that upon the removal of the cause, the effects will sometimes also soon subside. It points out, therefore, the neeessity of preventing over-distention, if the case is seen sufficiently early, and of immediately removing it if it already exists.

The second case illustrates one of the least eommon forms of mammary abscess — that, namely, unconnected with laetation, which differs in no respect from abscess in other situations, and yields to the common treatment.

The third proves, that where the abscess is owing to distention of the lactiferous tubes, the formation of fistulæ may sometimes be prevented.

The fourth case shows the result of opening by the lancet, and opening spontaneously. The former plan will generally be found the best, especially in deep-seated abscess. In this case, the opening made by nature was large, and the contents of the abscess were evacuated all at once, which is more favorable than usual; for generally the opening is no larger than a pin's head, and is slow in healing up,

the skin being commonly much destroyed before it gives way. Even in this case that abscess was much longer in opening, and more painful, which burst spontaneously, than that which was opened by the lancet.

The fifth case more forcibly proves that the artificial opening is preferable to the opening by nature; for the first abscess, being opened by the lancet, was healed in three days; whereas that which broke of itself was much slower in healing, and was slightly fistulous.

This also points out the necessity of watching these cases: the woman was desired to call on me the following day, instead of which she allowed three days to elapse; otherwise I believe the fistulæ would have been altogether prevented.

The sixth case shews this disease in its most distressing and tedious form: the breasts were permitted to become overdistended with milk; both breasts were inflamed throughout; numerous abscesses, implicating the lactiferous tubes, followed; and she suffered severely for ten weeks.

The two last cases prove the great advantage of pressure (after the inflammatory stage is gone off), in obliterating fistulous canals—a plan which will in many instances prevent, if it does not altogether supersede, the necessity of extensively laying open the breast, a custom which used to be resorted to formerly for the cure of sinuses, and which is even in the present day, I believe, occasionally adopted.

Case VII.—*Diffuse Inflammation, with Extensive Abscess and Sloughing—Dental Irritation—Marasmus—Death—in an Infant eight months old.*

Henry Heyslop, æt. eight months, admitted April 29th; a pale, weakly infant. Extensive diffuse inflammation over the whole left buttock, with great tension of the integuments; softening, and tendency to point at one part; skin here livid; a distinct fluctuation. The inflammation has only existed four days; sucks well; nights restless; bowels relaxed; thigh firmly flexed on the body; an opening made with a lancet gave vent to ℥iss. of strumous pus, with some blood.

Cat. panis.

April 30th.—Much offensive pus discharged; integuments around the wound tumid and black, and together with the subjacent cellular tissue in a state of slough; nights restless.

Rep. Cat. panis; P. P. grs. x.; Quin. Sulph. gr. j. ter die.

May 3d.—Sloughing extending below; the cellular tissue connecting the integuments to the tuberosity of the ischium still sloughing.

Rep. Cataplasma.

4th.—Some sloughs have separated, leaving a clean and granulating surface, the size of a half-crown piece; sloughs separating also from over the tuberosity of the ischium, leaving a very large sloughy cavity, from which pus discharges; constantly crying; frequent vomiting; bowels open.

Ung. Ceræ; bandage; Inf. Gent. c. ℥ss.; Tr. Opii, ♏ij. ter die.

5th.—Medicine not taken; rests well; sickness gone off; takes food; wound healthy above; still discharging and sloughy below.

7th.—Much stronger; wound much closed; surface granular and healthy above; sloughing stopped also below, where the integuments are disposed to adhere to the granulations; can extend the thigh a little.

9th.—Wound contracting; an abscess has formed over the lower part of the left rectus femoris, from which I evacuated a little pus, and subsequently applied a bandage wet with Goulard wash.

June 7th.—The wound made by the lancet healed in two or three days, and the surrounding inflammation was soon entirely removed; the original wound also was reduced to the size of a sixpence, and the child seemed as if it would struggle through, when great irritation accompanied dentition; it refused all nourishment, fell into a state of marasmus, and died yesterday.

Remarks.—This case appears to me to be practically useful, as it enables us to compare in the most satisfactory manner—namely, in the same individual—the result of opening an abscess early and late. The reader cannot fail to have noticed how rapidly the second abscess healed in comparison with the first; and although it may be argued that this was neither so large nor so serious as the other, yet no one can say that it might not have become so; and no one, I think, will assert, that it would have done better if longer left to itself.

As I believe some difference of opinion exists among surgeons respecting the propriety of opening abscesses early, I will take this opportunity of stating, that, in addition to the case to which these remarks are appended, and those of mammary abscess related above, I have had two cases in which two abscesses occurred in the same person. In both of these one abscess was allowed to open of itself, the

other was artificially opened. In both, the one artificially evacuated was much less painful, and healed much more quickly than the other.

I may further add, that 66 cases of abscess have been admitted under my care at the dispensary, most of which I have opened as early as I could, and I have had no cause to regret it, as the generality have done exceedingly well.

This case is also interesting from the great rally which was made, in spite of the irritation which had been set up, and the destruction of skin and cellular tissue which occurred. There can be, I think, but little doubt that the child would have recovered completely, had it not been for the supervention of dental irritation, under which many healthy children sink, and which, consequently, it was not to have been expected that a delicate infant, whose constitution had been already severely shaken, could possibly support.

CASE VIII.—*Compound Fracture of the Skull, and Wound of the Brain, produced by a large Iron File.*

Thomas Pickard, æt. 11, admitted April 22, was at play between seven and eight on the evening of the 20th, when a boy was throwing an iron file, without a point, of very considerable size and weight, at a mark; which accidentally struck him rather above the right temple with great violence, but did not knock him down. He bled considerably that evening: the bleeding, however, after some time, stopped; he did not seem much the worse for the blow, and walked home. He rose the next morning well, made no complaint, enjoyed his dinner, and cheerfully assisted his father at work; went to bed last night apparently well, but was restless and did not sleep. This morning he complained of head-ache, was feverish, and constantly sick. A clyster was given him, which procured two motions; soon after which he became lethargic. I saw him this evening, at 8 P.M., for the first time. There is a puffiness around an irregular wound, the size of a split-pea, upon the right temple, which discharges a thin bloody ichor, with air. The bone seems to have been depressed and driven in by the file, which appears to have lacerated the dura mater, and to have wounded the brain. He is lying in a state of stupor, but is so far sensible as to put out his tongue when told. There are no convulsions. The breathing is free, not particularly slow, and certainly not stertorous. Intolerance of light; pupils slightly dilated; tongue white, and moist; skin generally hot; pulse regular, rather full, 80.

Cat. panis vulneri, adrad. capil. V. S. ad ℥x. Hirud. viij.; Cal. gr. iij. tertia quaq. horâ.

He died about three hours after I saw him, continuing till then much in the same state. A probe passed with facility from the wound, its whole length, into the brain, portions of which escaped, upon the least pressure, from the wound.

Dissection. — Considerable ecchymosis throughout the scalp of the forehead. Pericranium preternaturally vascular, and easily separable from the bone, which is also vascular. On the right side of the forehead, towards the temple, there is a round, though somewhat irregular, hole, the size of a sixpence, in the frontal bone; adherent to the edges of which, internally, are two small irregular pieces of bone, which are driven in upon the brain, and another piece of bone is embedded in the very substance of the brain here. Dura mater very vascular, and lacerated; the opening corresponds, in size and situation, with the hole in the skull. The entire right hemisphere of the cerebrum of a deep chocolate red, from bruise and ecchymosis. In the situation of the wound, and for a considerable extent around, as low as the optic thalamus, the brain is converted into a reddish fibrous pulp; in some few spots it presents a blackened mortified aspect.

Left hemisphere of the brain congested; all the sinuses very much gorged. The substance of the brain generally soft.

REMARKS.—It is curious that a blow, productive of so much injury, should neither have stunned or knocked the boy down; and that so little mischief should have been apparent at the time of the accident. This case is analogous, in some respects, to the punctures of the brain through the orbitar plate of the frontal bone, the symptoms not coming on until some time after the accident, and death speedily following.

I did not propose the trephine, because the symptoms appeared to me to be those of inflammation, rather than of compression of the brain. If the instrument had not entered the brain, or penetrated the dura mater, there would probably have been more complete symptoms of pressure; when it would, I think, have been a very fit case for the operation: but as it passed through both, the blood and broken up portions of brain escaped through the wound, and in great measure counteracted the effects of the depressed bone. Had the operation been performed, it would not have been productive of benefit; as the boy appears to me to have died, not from pressure, but from destruction and laceration of the cerebral substance.

I have called this fracture compound, because I believe it is to cases of this kind that that name is applied by some surgeons of eminence. In strictness, perhaps, it should be termed a fracture with a wound, as the latter was not made by the former, the true meaning of a compound fracture.

CASE IN WHICH

SAND WAS VOIDED BY THE MOUTH, RECTUM, URETHRA, NOSE, EAR, SIDE, AND UMBILICUS,

AND

Attended by various other Anomalous Symptoms *.

By C. Ticknor, M.D. of New York.

Miss Lucy Parsons, of Egremont, Berkshire county, Massachusetts, when about eleven years of age received an injury, by the fall of a barrel across her loins, which was followed by exquisite pain, and an almost total loss of the power of locomotion. The pain after a time subsided, and the ability to walk gradually returned, though partial paralysis of the lower extremities, accompanied with severe pain, would almost invariably recur after much exercise. This state continued till about seven years after the receipt of the injury, when some portion of the surface was attacked with an erysipelatous inflammation, which, by metastasis, fixed itself upon the abdominal viscera. The patient now suffered excruciating pains, particularly of the right lumbar region, together with all the various symptoms of diseased stomach and bowels, was unable to walk, and mostly confined to her bed, till I saw her in the autumn of 1831, more than twenty years after the attack of erysipelas.

My brother first saw the patient in consultation with her attending physician: he found her labouring under a profuse diarrhœa, which threatened a speedy termination to all her sufferings; food would pass in *ten minutes*, to all appearances precisely as it was taken into the stomach, without smell or change of colour. At this time a few grains of calomel put a stop to the diarrhœa, and the patient remained one hundred and nine days without any fæcal evacuation per rectum. The most active cathartics had no other effect than to cause pain and irritation of the bowels, and a vomiting of their contents. An injection thrown into the rectum would be vomited in a few minutes, having the same appearance as when administered, and free from any admixture of fæces. During this period of one hundred and nine days the patient experienced a regular vomiting each day of the food, properly digested, which she had taken the preceding twenty-four hours. About this time there was something of a peculiar appearance in the matter vomited, which, on washing, proved to be *sand;* and on examination it was found that sand was also discharged with the *urine*. The bowels resumed their office, and it now became the turn of the bladder to have its contents expelled by vomiting; the patient experienced a strong desire, without the ability, to pass the urine by the urethra, and on trying to introduce a catheter, the passage was found occupied by a hard substance, which rendered the operation impossible. The urine was now vomited for several days, though it occasionally passed *per rectum*, mingled with sand, till a quantity of sand stones, or lumps of concrete sand, were discharged from the urethra, when the urine again flowed through its proper channel.

The pain continued unceasing in the right side; a small abscess formed, which, being left to itself, opened and discharged, with a small quantity of pus, several lumps of sand; and in the efforts at vomiting, fæces escaped through the same opening.

June 8th, 1832.—No fæcal evacuation from stomach or bowels in forty days; has taken a great deal of cathartic medicine; vomited her urine; vomited injections in fifteen minutes after being administered, without the least appearance of any fæces; appetite pretty good, though she takes but little food; tongue of an inky blackness, except the edges, which are red; complaining of excessive pain in right side and stomach; says she can feel lumps of sand moving inside; sand passes through the external opening in the side, mixed with blood, and sometimes fæces; has had spasms of the muscles about the throat and jaws.

13th.—Pain very great; jaws spasmodically closed; mouth filled with lumps of sand—several pieces passed out at the nose; saw her eat some bread and milk, and in a very few minutes it passed out at the opening in the side; tried to introduce a probe into the orifice, but could not succeed; could feel the sand in the side.

14th.—Received a note from the patient's brother, saying that his sister this morning passed by stool, *at one sitting, forty-four lumps of sand.*

25th.—The lumps of sand discharged on the 14th vary from the size of the forefinger to the first joint, to that of a small pea; no fæcal evacuation from stomach or bowels since last date; for the first time

* From the American Journal of Med. Sc., May 1834.

during her illness, she has since last visit vomited purulent matter, and voided it by stool.

July 19th.—Received the following account from the patient's sister of her state since last visit. On the 6th inst. her jaws became spasmodically closed; bowels for three successive days regular, since then no discharge per rectum; regular vomiting once a day of fæcal matter, which is quite fluid, and escapes between the teeth. 8th, right ear began to bleed; 12th, discharged a watery fluid, resembling urine, with sand.

30th.—An abcess opened just above the symphysis pubis, and discharged a small quantity of pus, afterwards *urine mixed with sand*, which continued for a week. Present symptoms: appetite pretty good; takes liquids, which she sucks between her teeth, jaws being yet firmly closed; vomits fæces every day; the fluid part escapes between the teeth, and the more solid part is again swallowed; has lumps of sand in her mouth, which have been there *eleven days*; a dose of tartar emetic caused a little relaxation, and the mouth was emptied. At this visit I saw her vomit her urine, one gill, perfectly transparent, as if just passed by the urethra: saw a teaspoonful of fluid discharged from the ear with sand, and a lump of sand from the nose; sleeps little; suffers exquisite pain; another abscess seems to be forming in the right side; more emaciated than I have seen her at at any period of her illness.

August 1st.—Jaws yet closed; vomits, or passes by stool every few minutes, a whey-like fluid; retains *very little* food. Since last visit voided by stool at once a tablespoonful of sand, with a teacupful of pus; and soon afterwards there was discharged in the same way a membrane-like substance, of the size of a crown-piece, containing a number of fine, delicate hairs: there is voided now sand and urine by the mouth, rectum, urethra, nose, ear, side, and umbilicus! Treatment—Nit. argent. grs. x., op. xv., ft. pil. xx.; one every fouth hour; foment abdomen, side, and throat, with dedoction of cicuta.

13th.—Symptoms of same character, though much mitigated in violence. Continue the same treatment.

September 1st.—No diarrhœa; some vomiting; pain of left side; no discharge of sand since last date; deaf with right ear; vomits urine occasionally; for two weeks has been troubled with spasms resembling epilepsy; has twenty or more in a day; is warned of their approach by pain in the epigastric region: left leg strongly flexed upon the thigh; heel drawn up, and lying upon the glutei muscles, and has been so for twelve days: appetite good, but most of the food is rejected soon after eating. Take the following pill every fourth hour:—Ext. hyosciami, grs. iij., castor, grs. ij., nit. argenti, gr. i.; laud. and ext. cicuta between the pills in quantities sufficient to procure sleep or quiet; foment spine, side, and epigastrium, with decoct. cicuta.

10th.—No spasms since last date till today; omitted the pills yesterday, and today the spasms returned; no natural evacuation from bowels or bladder since 20th July; contents of both are vomited; appetite tolerable: has ridden out several times since last visit; left leg continues flexed. Treatment the same.

October, 1833.—Have not seen the patient for more than a year; her sister gives the following account of her condition during that interval. Her bowels soon became quite regular, and so continued for some time, then relapsed into their former obstinately costive state, when their contents have been vomited; appetite has been generally good; left leg, during the whole time, has continued flexed, and attempts to extend it have invariably caused frightly spasms; has ridden out frequently, done a good deal of needle-work, and may be said to enjoy comparatively good health. I ought to add, that during the whole illness of this patient her catamenial evacuations have been generally regular, though at times rather profuse.

A lump of sand discharged from the bowels, which Dr. Torrey and Dr. C. A. Lee, of this city, had the kindness to analyze for me, proved to consist of silex and lime, and a few *short hairs*; the former making much the greater proportion. Professor Averill, of Scheneetady, did me the same favour, with the same result.

Most of the facts related in the above case, besides being witnessed by my brother, Dr. L. Ticknor, of Salisbury, Conn. and myself, can be vouched for by the following gentlemen:—Dr. Bolton, of Egremont; Dr. Kellogy, of Sheffield; Dr. Wheeler, of Great Barrington; Professor Averill, of Union College; Drs. Cleaveland and Flallenbeck, and Mr. Oliver Whittlesey of this city.

The interesting features of this case will readily suggest themselves to the mind of every reader. It furnishes an extraordinary example of the amount of disease the system can sustain when nature is playing her wildest freaks.

In regard to the imposition which the patient practised, or at least *might* have practised, I may state that she has sustained a character for genuine piety, and to have lived with her two maiden sisters, who have had the sole care of her for more

than twenty years. They had a small estate, which, with frugality, would support them all, and there can be no reason why she or her sisters should wish to deceive. I have hesitated about publishing the case, but I believe it a duty I owe to my profession not to withhold the facts, however they may be accounted for, or however little they may be believed. I have thought it best to give the facts, and the facts only. The patient is still living about fifteen miles from where my brother is practising, and he may yet detect her in her imposition, if she is not put upon the look out by publishing speculations prematurely. Can these phenomena be accounted for without calling the aid of imposition?

NOTES OF A CASE OF

FISTULOUS OPENING OF THE STOMACH,

SUCESSFULLY TREATED BY

DR. J. H. COOK.

SOME time in the month of February, 1832, Dr. Bardwell and myself were called to visit the widow D., aged 39 years. We found her, as near as may be, in the following condition:—a fistulous opening, immediately by the side of the umbilicus, into which a buck-shot might have been readily passed. On removing the bandage, a gill of bile was suddenly discharged, after which, a small quantity of a different (the gastric?) fluid, came slowly away; these discharges were attended with great pain, on account of their acrid quality. The whole surface of the abdomen was excoriated, inflamed, and intolerably painful. We introduced a flexible catheter its whole length, thirteen inches, before meeting with any resistance, when the extremity suddenly met with an obstacle. By pushing it against the resisting body, or even by slightly agitating the instrument, strong efforts to vomit were produced.

Withdrawing the catheter, we desired her to drink a glass of water; she did so, and in twenty seconds the *whole* was discharged through the fistula, as we ascertained by measuring it. The direction of the fistula was upward, and slightly inclining backwards, with about the same inclination to the right side. We came to the conclusion, that the opening within was at, or about, the pyloric orifice of the stomach; and that the catheter entered the stomach, and pressed against its cardial portion. The patient even felt it there, and applied her hand externally over that part.

Treatment.—Taking a large beef's bladder, we cut it open longitudinally, spread it well with adhesive plaister, and after washing the inflamed surface, and dressing it with basilicon spread on fine linen, we applied the bladder over the abdomen, and made an opening over the fistula, through which the matter might escape. We then applied a bandage and compress, and directed that it should be reapplied immediately after each discharge. We advised mucilaginous drinks, and a diet of rye-mush and molasses, and nourishing enemata. The patient was much emaciated for want of proper nourishment, as every thing passed off undigested through the fistula. No evacuation had taken place in the natural way, for ten days previous to our visit. The external irritation of the abdomen soon healed, and the bladder was then applied to the skin as a protection, and continued there with the happiest effect. The bandage was gradually tightened, and a compress of a cylindrical form was laid over the course of the fistulous canal. By these means our patient regularly, but slowly, recovered. In a few days the alvine evacuations were restored to their natural outlet, and the discharges from the fistula began to decrease. In thirty days the opening was closed, and the fistula apparently obliterated. Several months have elapsed since that event, and she continues in excellent health.

All we could collect from her, as to the history of her case, was this: six months before, in one of the south-east counties of this State, she was attacked with constipation and violent pain at the pit of the stomach, which resisted every remedy, till the 19th day, when the fistula shewed itself.—*Western Journ. of Med. and Phys. Sciences, America*, January 1834.

CASE

OF

A FOREIGN BODY LODGED IN THE HEART*.—*RECLAMATION*.

To the Editor of the Medical Gazette.

SIR,

As the next meeting of the Association takes place on the 18th inst. I trust to your known impartiality for an insertion of the following in the next number of your valuable publication; especially as a full

* Med. Gaz. present vol. p. 344.

report of the case alluded to has appeared in the pages of the Gazette.

I am, sir,
Your obedient servant,
GEORGE SHEWARD.

Upton, July 6, 1834.

To the Members of the Provincial Medical and Surgical Association.

"Hos ego versiculos feci, tulit alter honores."
Virgil.

GENTLEMEN,—In the last volume of the Transactions of your Association, is the report of a case of a foreign body found in the heart of a child who died, some time since, at Baughton, in this neighbourhood; and as the author of that case has, in my opinion, been deficient in good faith towards me, I shall take the liberty of making a few observations on the subject.

In the first place, I distinctly state, that although the case is published "by Thomas Davis, surgeon, Upton-on-Severn," not a *tithe* of what is there printed was *written* by him, and that the notes on which such report was founded, were furnished by me, and received by Dr. Hastings in my hand-writing. I was requested by the Hon. Wm. Coventry, in whose service the father of the boy was, to be present at the opening of the body, in consequence of what he considered the extraordinary nature of the case; Mr. Davis having asserted that the boy had recovered from the accident, and sunk under an attack of ague. No mention of this is made in the report, or of the treatment for the cure of this supposed disease.

I saw Mr. D. previous to the examination; and meeting Mr. Coventry in the road, I requested him to accompany us. From the slight mention made of my name in the report, none of the readers of it would for a moment suppose that the examination was performed by me, yet such was the fact, Mr. D. having requested me to do so, (why, I leave you to suppose), merely saying that the instruments provided by him to open the chest of *a child*, (and I pledge my honour to prove the fact,) were *a butcher's saw and a small chopper!!*

On my return home I was requested by Mr. Davis to draw up a report of the case, which I declined; but, on being much pressed by him, I wrote down the anatomical appearances, which notes, even in their rough state, were sent to Dr. Hastings. On my calling on that gentleman, at his request, to explain the details more fully than I had done on paper, these notes were produced and acknowledged as mine; and when he told me that it was his intention to publish a report of the case, I requested, either at that interview, or in a note I subsequently addressed to him, that he would state from whence his information was derived, and by whom the notes were furnished to him. Judge, then, of my surprise, when, on perusing the case a few days since, I found the very words made use of by me in the last paragraph, are published as the opinion of "Thomas Davis, Esq., Surgeon!!" although Dr. Hastings well knew that he was incapable of giving any opinion at all upon the subject, and knew no more of the anatomical structure of the heart than the paper on which I am this moment writing. I state this from no feeling of animosity towards the party alluded to. I take no pride in authorship, or I would have published a report of the case myself: but however it may suit the *interest* or the inclination of the learned physician to dress up cases for persons who cannot do so for themselves, he can have no right to make use of the materials furnished by others, unless he states the sources from which they were derived; and in the present instance the paper might just as fairly have been ascribed to Mr. Coventry or Mr. Walker as to the party under whose name it now appears.

Having said thus much on the subject, in justice to myself, allow me to conclude with a few observations on a question which will, I am given to understand, be submitted to your consideration at the next meeting of the association.

I have always considered that one of the great objects of such associations was to add to the respectability, as well as to the information, of the profession. If such is indeed the case, how is it that persons who have never received a professional education, and a few years since were exercising their talents on the brute creation, become eligible to practise on the human race, and to be admitted members of your body?—and how is it that men, who are considered as standing at the head of the medical branch of the profession, do not hesitate to meet such persons in consultation, and in cases too which have generally been considered as belonging especially to the province of the surgeon? Yet such is the case with more than one physician in this neighbourhood; and while it is so, no one can be surprised that young men, who have spent their time in the acquirement of knowledge in a profession which requires more study and attention, and entails upon them a heavier responsibility than any other, feel the inutility of such exertions, when regularly-educated physicians do not hesitate to meet such practitioners as I have described, and to

conceal the egregious blunders that daily and hourly occur to them. It may, perhaps, be said that I have done the same thing; but this I deny. I have been called in, and have attended, in more than one instance, not out of the respect I bore to the practitioner, but from a desire to save the patient from much misery, from maltreatment; and where this has been the case, I have never derived the slightest emolument, because I consider that professional men have a duty to perform above the consideration of fee, perquisite, or reward.

There are other persons in the profession besides myself who have declined becoming members of your association, on this ground alone; and to this source may be traced one of the causes which have operated in producing the cry for medical reform. The remedy is obvious; and as a public association, embracing all classes of the profession, I call upon you to express your opinions on the subject; and while such continues the practice, I would advise the regularly-educated medical man to dispense altogether with the aid of a physician, and rather trust to the assistance of a brother practitioner, than countenance a line of conduct which strikes at the foundation of the best interests of the profession.—I have the honour to be,

Gentlemen,
Your obedient servant,
GEORGE SHEWARD.

Upton-upon-Severn,
June 30, 1834.

MEETING OF THE PROVINCIAL ASSOCIATION.

To the Editor of the Medical Gazette.

SIR,

As some of your readers, residing at a distance from this place, who purpose being present at the approaching meeting of the "Provincial Medical Association," may wish to know something of the proceedings likely to take place, I will, with your permission, give a slight outline of the arrangements, so far as I can learn them.

The meeting, as announced, will take place on Friday, July 18th. The President chosen for the occasion is Dr. John Johnstone, whose high character is too well known to need any remark from me. His influence, and that of his family, which has for so many years stood high in our profession, would alone ensure both a numerous and highly respectable attendance of the profession.

The proceedings of the day will commence with a meeting of the Council, at the Anatomical School, the Museum of which will be open, for the inspection of the members of the association. This Museum, though collected within a few years, has, by the indefatigable labours of Mr. Cox, the anatomical lecturer, with the assistance of numerous donations, become an object of considerable attraction, particularly the beautiful collection of wax models which it contains; and I would suggest to the notice of visitors a very valuable and rare specimen of extra-uterine conception, which may be seen under the same roof.

After the preliminary arrangements have been made by the Council, the general meeting will take place at the Philosophical Rooms. This meeting is expected to be one of no ordinary interest. The proceedings will be opened by the President; after which the annual retrospective address, which was to have been prepared by our late lamented townsman, Dr. Darwall, will be delivered by Dr. Conolly of Warwick, a gentleman who, from his high talent and intimate connexion with the deceased, was naturally designated as his most fit substitute on the present occasion.

In accordance with the plan found so successful by the British Association, reports on different branches of professional knowledge have been directed to be prepared for this meeting. The subjects selected for this year are—"A Report on Anatomy," "A Report on the Chemical Pathology of the Fluids of the Human Body," and "A Report on Vaccination." The reading of these reports will constitute at once a new and interesting feature in medical meetings.

Much valuable information has been received from the foreign correspondents of the society, which will also be communicated to the meeting. And, in conclusion, the necessary arrangements will be made for the ensuing year—the several officers appointed—the subject of the next annual reports selected, and the individuals named who shall be requested to prepare them—and, finally, the accounts will be audited.

But of course, sir, that important feature in all English meetings—provision for the bodily man—will not be neglected, though I scarcely expect that we shall in this respect be able to compete with our Bristol friends: such *turtle* as they provided for their guests, is but rarely met with at public dinners in Birmingham; and *champagne* seldom flows in the abundance in which they supplied it. I have not heard whether any of our members intend imitating the laudable example of the hospitable Mr. Hetling, who, at the Bristol meeting, invited the council to an excellent breakfast, at his own house—a

most propitious commencement of the day's proceedings. Although, however, we do not appear likely to rival our Bristol friends in the "*cuisine*"—a department where they have long shone pre eminent—I still entertain no doubt but the ensuing meeting will be more numerously attended, and go off with greater *eclat*, than any previous re-union of the Association. Our own body—and we muster no small number of the profession in Birmingham—all appear most anxious to promote the success of the meeting to their utmost. The great increase of members during the past year is perhaps the best testimony of the zealous interest which the profession at large take in the welfare of this great provincial society.—I am, sir,

Your obedient servant and
constant reader,
AN ASSOCIATE.

Birmingham,
July 1, 1834.

LEGAL MEDICINE AT MONTPELLIER.

A PROFESSORSHIP of Chemistry and Toxicology has just been founded by the Faculty of Montpellier. Toxicology is thus separated from the chair of Legal Medicine,—an arrangement by which several advantages are gained. Among these we may enumerate the following:—1. The course of Legal Medicine, already quite too extensive to be efficiently taught in a single session, is now divided into proper departments.—2. Legal Medicine, being detached from Legal Chemistry, can now be taught in a superior manner by accomplished physicians and surgeons, who may not happen to be profound in general physics or chemistry;—and, 3. Toxicology will be better taught by an individual specially qualified, and who shall be a first-rate chemist. M. Berard, already so distinguished as a physician and a chemist, has been named as the first occupant of the new chair. The professorship of Legal Medicine is at present vacant, but will be filled, by *concours*, on the 1st of December next.—*Journal Hebdomadaire.*

CURIOUS CASE OF SOMNAMBULISM.

A CORRESPONDENT of the American Journal of Medical Sciences informs the editor, that he had recently had an opportunity of seeing the celebrated Springfield somnambulist. The extraordinary part of the case, according to this gentleman, seems to consist in the marvellous susceptibility of the eye to its natural stimulus. "I played," says Dr. Bartlett, "a game of backgammon with the girl during one of the paroxysms. She saw perfectly well—calling her throws of the dice, and moving her pieces regularly—through several thicknesses of a white handkerchief bound closely over her eyes. She could read in a room lighted in the usual manner, with *eight* folds of a linen handkerchief laid over her eyes." The girl, it further appears, has no recollection in her lucid intervals of any thing that has occurred in her somnambulous state. During her paroxysms, there is determination of blood to the head; and she suffers other inconveniences, which render the constant care of Dr. Woodward, of the State Lunatic Asylum, necessary to her. The newspapers have contained several curious stories about this girl; but perhaps the most valuable and best-authenticated fact connected with the case is the physiological one—of the inconceivably small quantity of light with which it is possible for the eye to perform its functions.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *July* 8, 1834.

Abscess	3	Hernia	1
Age and Debility	30	Hooping-Cough	8
Apoplexy	5	Hydrophobia	1
Asthma	4	Inflammation	29
Cancer	1	Bowels & Stomach	1
Childbirth	2	Brain	4
Consumption	62	Lungs and Pleura	3
Constipation of the Bowels	2	Jaundice	1
Convulsions	32	Liver, diseased	5
Croup	3	Measles	7
Dentition or Teething	4	Miscarriage	1
Dropsy	17	Mortification	2
Dropsy on the Brain	10	Paralysis	2
Dropsy on the Chest	1	Small-Pox	3
Erysipelas	1	Stone and Gravel	2
Fever	3	Thrush	1
Fever, Scarlet	4	Tumor	1
Fever, Typhus	1	Unknown Causes	5
Heart, diseased	2	Stillborn	14

Decrease of Burials, as compared with the preceding week . . . 28

NOTICES.

A DOCTOR OF ST. ANDREW'S.—On looking over the paper a second time, we agree with our correspondent that a modification of it, such as we suggested in the first instance, would not be very practicable. We have accordingly forwarded it to the Chairman of the Committee.

Mr. Hetling's letter to Mr. Warburton appearing to be a circular, we are obliged to decline giving it insertion.

MACHAON.—We regret that we cannot find room for the letter from York.

SS.—The physiological contributions will not suit our pages.

W. WILSON, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JULY 19, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XLI.

PRETERNATURAL LABOUR.

TRANSVERSE PRESENTATIONS.

The second order of preternatural cases embraces all those in which any other part of the fœtal body presents, except the head, breech, or inferior extremities; and these will, next in order, occupy our attention.

When the fœtus lies transversely, the long diameter formed by its doubled body being across the uterus from side to side, the familiar term *cross-birth* is peculiarly applicable; and to this variety of preternatural cases it should, in strict propriety, be limited.

I have already stated it as my opinion that there is no part of the child's body which may not offer itself as a presentation under labour. You may therefore readily imagine, that when situated transversely, the head may lie upon the right or left ilium, with the face directed either forwards or backwards, so that either the right or left side, the back, chest, or abdomen, may be placed downwards.

Importance of discriminating between the first and second order of preternatural presentations.—It is of the greatest possible consequence that we should be able to discriminate in practice between the first and second orders of preternatural presentations, because those which are embraced within the first are, generally speaking, terminated by the efforts of nature alone, or with very little artificial assistance; while in those characterizing the second order the very reverse obtains: the child is so placed that nature unaided can scarcely ever effect her object; and an operation always attended with pain, difficulty, and danger, is requisite before delivery can be accomplished. This operation, if undertaken during the first stage of labour, is comparatively easy; but, if delayed until the process be much advanced, it becomes one of the most difficult in surgery; and, *cæteris paribus*, the danger attendant upon it is in proportion to the difficulty. It would be superfluous, therefore, to insist on the necessity of early forming a correct diagnosis.

Absence of symptoms previous to the commencement of labour.—There are no symptoms manifested previously to the commencement of labour by which we are able to determine that the child lies transversely in utero. It has been said that if the uterus, in its general figure, be broader than it is long, we may suspect a transverse presentation under labour: this, however, is by no means universally the case; it is but a vague supposition at the best, and no reliance can be placed on it; for the greater breadth of the uterus may depend on its containing twins; and although they both may be presenting either with the head or breech, it is evident that the organ must occupy more space laterally than if there were but one child lying in the natural position. An increased quantity of liquor amnii may also influence the shape of the gravid womb; and sometimes the uterine fibres are not developed with their accustomed regularity, but some, more rigid than the others, refuse to yield in due proportion, and thus occasion an unusual form. We can, therefore, by no means rely for a diagnostic mark on the external figure, as detected by the application of the hand.

No causes to which it can be assigned.—Nor are there any causes evident to which we can assign this peculiar presentation of

the fœtus. I have already mentioned that particular postures of the mother's body were supposed to regulate in some degree the position of the child in utero; but this observation is proved to be as incorrect in regard to shoulder presentations as it is to breech. I also stated that Mr. Barlow, in his "Essays on Surgery and Midwifery," tells us "he is induced to believe that preternatural presentations take place more frequently when the pelvis is distorted than otherwise;" that this coincided with my own observation: and indeed Denman himself, (than whom a more observant practitioner, or one more cautious in promulgating either statements or doctrines, has never appeared as a medical author,) assures us that in more than one instance, after he had fully satisfied himself that the head presented at the commencement of labour, he has left his patient for some hours, and on the next examination found that the situation of the child was materially changed, the shoulder being then at the brim, and the head having receded out of the reach of the finger. He states also, that "it is remarkable the occurrence has always happened to women who were deformed;" and adds, "that it may lead to an explanation of one cause at least of preternatural labours." It will be in your remembrance that I have detailed a case which happened under my own eye perfectly confirmatory of Dr. Denman's statement, if confirmation were required; and although the same must have been remarked by other practitioners, as yet no useful result has originated from the hint.

Transverse presentations are by no means comparatively more frequent among the poor than those in affluent circumstances: but some women seem to be naturally predisposed to this irregularity. Thus a patient of my own, whom I attended in all her labours, out of five children which she has borne, has been the subject of four transverse presentations: her pelvis is slightly distorted at the brim. And another woman, now dead, who always, under pregnancy, became a patient of the Royal Maternity Charity, in twelve labours suffered seven shoulder presentations. I delivered her myself five times under these difficulties, and my father twice.

Suspicious symptoms.—It is then only after labour has commenced, and when, indeed, it has made some progress, that we can positively detect a transverse presentation. We may *suspect* an irregular position, if the os uteri, although flaccid, opens slowly—if the membranes protrude into the vagina rather in the form of the finger of a glove than the round end of an egg—and if we cannot feel any part of the child, even when the finger is carried up to its full extent within the vagina; for you will easily understand, that when the shoulder presents, the fœtus cannot descend into the pelvis in the same way as when the head or breech offers at the brim, being supported by, and resting on, the alæ of the ilia.

We may also *suspect* that the child is lying transversely, if, when the membranes have ruptured, the uterus ceases to act for some hours; for it often happens that although the pains were frequent and powerful before the membranes rupture, they cease entirely for a considerable time directly the first stage is completed; and we presume that this is owing to the os uteri having lost the stimulus previously afforded it by the aqueous cyst while it remained whole; for as the fœtal body is, as it were, suspended by the sides of the maternal pelvis, the presenting part cannot immediately subside to the mouth of the womb, as occurs when either the head or breech is protruded first. But we can only *positively* detect a transverse presentation, by distinguishing the different parts of the child which indicate to us the mode in which it lies.

Although transverse presentations, in all their varieties, occur only once in about 320 cases, according to my tables, their infrequency should be no excuse for our neglecting to gain all the information in our power respecting their history, their diagnostic marks, and treatment. In *La Maternité*, at Paris, indeed, the frequency would appear greater; for, out of 10,724 children born in that institution, according to the last report, the proportion of those "presenting with the trunk" was one in less than 200 births.

Labour, then, would most likely at first commence less actively than under a head presentation; the uterus would become somewhat diminished in bulk before the dilating process commenced; but for the reasons I have assigned it would not so fully descend into the pelvis as when the head presented. At first the pains would be short and infrequent; they would then become more powerful; the membranes would burst; and after their rupture, the uterus would probably remain an indefinite time inactive. On the resumption of its powers, however, the presenting part would be more or less forced down into the pelvis; and in time, provided the case were left entirely to the natural efforts,—no artificial assistance of any kind being rendered,—one of three things must happen; either the uterus, by its own inordinate action, must rupture its own structure—an accident which is almost invariably fatal; or by a continuance of its strong exertions it must wear itself out, and gradually cease to

act, on which state exhaustion will supervene, and death will sooner or later occur; or, thirdly, the child's body will be squeezed into a smaller compass—will be propelled through the brim into the cavity of the pelvis, and will eventually pass double: for I can scarcely believe it possible that the woman would survive the entire dissolution of the fœtal body by putrefaction, the separation of its limbs and other component parts, and their evacuation in a disjointed state. The doubled expulsion, or, as it has been called by Denman, the "spontaneous evolution," (of which I shall speak hereafter), has, indeed, been occasionally observed, but it is very rare. It is certainly not to be expected, and scarcely can be hoped for, if the term of gestation be completed. Provided, indeed, the woman have not exceeded seven months, since the child is then comparatively so small, we may with some confidence look for it; but beyond that period we must be prepared to terminate the labour by art.

Marks of a transverse presentation.—As with regard to the breech, so also in every variety of transverse presentations, there are some marks both negative and positive, which assure us of the particular part that offers at the pelvic brim; and first we will give our attention to that which, of all others, is the most frequent—the

Shoulder.—This part of the child's person is not so large in bulk, nor so hard and bony, as the head; and it has neither the general figure of the head nor its sutures. Again, it is not so large in its rotundity as one of the nates, nor is it so fleshy; we cannot feel the anus, or the parts of generation. There is but little chance of our confounding the shoulder with the cranium; but the diagnostic marks between it and the breech are not so easily made out; there is a degree of similarity to the touch between the top of the shoulder and one of the nates, which it is not always easy to particularise. The positive marks are—the pointed acromion, most dependent,—being able to feel the spine of the scapula posteriorly, and the clavicle, and perhaps the ribs, anteriorly,—being able to get the finger within the axilla, and not encountering any structures similar to the anus or genitals. If we possess the advantage of discovering all these marks, we can never be disappointed in distinguishing that the presenting part is the shoulder.

Elbow.—It sometimes happens that the child presents perfectly transversely, and the elbow comes down doubled into the pelvis, offering itself in the vagina. Immediately that the elbow meets the finger, we can be positive that a limb presents; we may easily know that it cannot be the head—that it cannot be the breech; but it must be either the superior or inferior extremity. The fœtus, then, may be placed transversely, in that situation which requires the performance of an operation, or it be so situated that nature, unaided, may be able to accomplish delivery; so that it is a matter of the greatest consequence to determine accurately which limb it may be. Now, of all points of the body, it is most difficult to discriminate between an elbow and a knee. In the knee, however, we have the rounded patella, with its flat surface, which is more or less moveable on the condyles of the thigh-bone. On the contrary, in the elbow we observe the pointed oleeranon sharper than the patella; we look in vain for the smooth flat surface which the knee presents, and we can by no means move it from side to side. The last distinctive mark, however, which I have mentioned, should not have much stress laid upon it, as, in this respect, we are likely to be deceived; for when the knee presents, and the leg is turned back on the thigh, the patella is fixed, in consequence of the extension of the rectus femoris, and its mobility is consequently considerably impeded. However, if the other marks are borne in mind, we cannot well be deceived. Should any doubt still exist, let us not lull ourselves into dangerous apathy by calculating the many chances which there are in favour of its being a knee rather than an elbow presentation; but let us institute a more careful examination. If no part of the child's body except the presenting limb is to be felt—provided the membranes are broken—it would be right to bring the folded extremity fully down,—avoiding, however, all traction,—so that we may be able to ascertain whether it be an arm or a leg; for even should the shoulder or side be occupying the brim, this proceeding would add no difficulty to, and not in the least embarrass us in, the subsequent operation of turning.

Hand.—It is not often that the hand presents alone, so that we are unable to feel any other part of the child's body: should that, however, be the case, it would be known from a foot (and that is the only part with which it can possibly be confounded), by negative as well as positive signs—by there being no rounded instep—no prominent heel; by the digits not all being in one line. The positive signs are the flattened palm, the fingers being longer than the toes, and themselves not all of the same length, and by the thumb forming an antagonist power to the other four. But the being able to feel the hand does not necessarily imply that the child lies in a transverse position; for it is by no means unusual for this member to be placed upon the ear, or by the side of the

nates, and to prolapse before either of those parts, as soon as the membranes break; which cases, as we shall hereafter learn, require little or no artificial interference. It becomes our duty, then, as soon as a hand is detected, to examine most minutely, for the purpose of ascertaining whether the head, breech, or shoulder, be at the brim; and to act according to the information we then obtain.

Side.—The side of the child may present, and here also we have some negative as well as positive marks to guide us. The side possesses neither the roundness nor the firmness of the head, nor any sutures or fontanelles; neither is there the double rotundity nor the soft fleshy feel of the breech: we cannot distinguish the parts of generation nor the anus—we can feel no other part of the child except the ribs, and, it may be, the arm also. The side may be principally distinguished by the spaces between the ribs; and if two of these can be clearly traced, there can exist no doubt as to the presentation. The head is the only part of the body for which the side is likely to be mistaken, and I have actually known this mistake occur. If we could only feel two ribs and one intercostal space, it might be possible for us to be deceived; we might suppose the margin of the ribs to be the edges of the parietal bones, and the space itself the sagittal suture. But I have already instructed you, that if there be any doubt with regard to the presentation, under the mode in which we usually examine—with the first finger of the right hand—two fingers of the left should be introduced, or that we should even pass in the whole hand, and ascertain the presentation, rather than allow hour after hour to elapse in doubt on so material a question.

Back.—A child may present with its back, although this is a very uncommon position for it to lie in. Three or four of the spines of the vertebræ can be felt by the fingers; and we can also detect the origins of the ribs, even before the os uteri is completely dilated. When the back presents, it is of no consequence whether the part directly over the os uteri be near the shoulder or the breech, —whether we feel the lumbar or the dorsal vertebræ, the same thing is required to be done—the breech or the legs must be brought down by the introduction of the hand, and the case terminated by turning.

Chest. — The chest may present,—any point of the sternum meeting the finger. There would be difficulty in detecting this presentation by the first finger of the right hand; but by introducing two fingers, or the whole of the left hand, we shall feel the sternal bones, the continuance of the bony plane, the ribs, or rather the cartilages, at their origin from the sternum, and the intercostal spaces. This is perhaps a rarer presentation than any of those I have previously mentioned.

Abdomen. — The rarest presentation of all is the abdominal. Out of more than 120 transverse presentations in which I have myself operated, I have only met with this peculiar position once. Smellie has furnished a plate, in which he has delineated the navel as the presenting part. If the case were allowed to proceed without interference, the abdomen would be squeezed somewhat through the brim of the pelvis after the membranes break. There can scarcely be a chance of mistaking this presentation. We shall feel the large, soft abdomen, possessing no osseous formation; we shall perhaps be able to distinguish the ensiform cartilage; but a more positive mark is the insertion of the funis umbilicalis. Whenever we can feel the commencement of the cord by the finger, there exists a belly presentation most undoubtedly. Do not, however, let us suppose that all cases in which a fold of the funis comes down into the vagina must turn out presentations of the abdomen, because the cord frequently prolapses when other parts of the child are at the brim.

Modes in which the operation of turning may be performed.—Having determined that the case under a transverse presentation is not to be left to nature—that it is more likely that the uterus will rupture, or that the patient will die exhausted, than that the child will pass double—we must make up our minds to change its position by an operation. First, then, let us inquire in what that operation consists; and, secondly, what period of the labour we shall select for its performance.

Three different modes have been recommended, and they all, perhaps, enjoy their peculiar advantages.

The first is, that we should raise the presenting part, introduce the hand into the uterus, seize hold of the head, bring it to the brim of the pelvis, and convert the case into a natural presentation. The second advice is, that we should introduce the hand into the uterus, run it along the abdomen of the child, take hold of the breech firmly, grasp it with the fingers, bring it to the pelvic brim, and make it a breech case. And, lastly, we are told to introduce the hand as high as the fundus uteri, running it along the body of the child, search for the feet, and, bringing down one or both, make the child perform a complete evolution, and extract it footling. The ancients were fully impressed with the danger of transverse presentations; and in the works of Ætius, who was a compiler from previous authors,

many hints may be found relative to bringing down the feet by turning, under such an unfortunate situation. The operation, however, was not generally adopted till the time of Parcy, to whom, although he be not the first suggester of this great practical improvement, is justly due the honour of having satisfactorily proved its safety and utility, and enforced its adoption, both by his precepts and example. Before Parcy's time,—the middle of the sixteenth century,—there seem to have been instituted no precise rules for the management of this order of preternatural cases; it was the almost invariable practice to endeavour to bring the head over the pelvic brim, but, failing in that attempt, most writers directed that the child should be extracted in whatever manner was most practicable. In Celsus, indeed, we find it recommended, that *if the fœtus cannot be brought into a proper direction*, the head should be separated from the body, as the only likely means of accomplishing delivery. —"Remedio est cervix præcisa; ut separatim utraque pars auferatur."

Of these three modes, that of raising the shoulder and bringing down the head would be the safest to the child, because there would then be no chance of pressure on the funis umbilicalis; and it is that pressure which usually destroys the fœtus under the operation of turning;—but, although safest for the child, it is the most dangerous to the mother, as well as the most difficult to the operator; and the danger, as might be expected, is in proportion to the difficulty. The form, size, and slippery nature of the cranium, all combine to produce this difficulty. Even although the shoulder may be raised from the brim, and pushed entirely out of the way, it is no easy matter to grasp the head, so as to bring the vertex over the centre of the superior aperture; and in these attempts, which will most likely require to be repeated, lacerations and contusions of the uterus or vagina could scarcely be prevented. From the danger and difficulty accompanying this operation, it is now, I believe, entirely abandoned in England as a means of delivery under transverse presentations, although recommended by Dubois, in France, as applicable to some few cases. Thus the last reports from *La Maternité* inform us, that out of fifty-nine cases of transverse presentation, two were terminated by bringing the head to the pelvic brim.

Delivery by the breech offers an expedient second in degree safe to the child; for, as already mentioned, when the legs and feet lie up against the child's body, the funis umbilicalis is, to a certain extent, protected from pressure, and the passages having been considerably distended by the transit of the breech, are rendered in a fitter state for the easy escape of the shoulders and the head, than when the feet are first brought down. But this mode of acting is also both difficult and somewhat dangerous, and that from the same causes as embarrass the operator in the attempt to bring down the head. We are not able to encompass the breech readily; we cannot easily grasp it, so as to bring it over the pelvic brim, in consequence of its bulk and form.

The third means is by far the most dangerous to the child, but by far, also, the safest to the mother—that of grasping one or both feet, bringing the breech, through the hold they afford, into the pelvis, and extracting the fœtus by their agency. This is the mode of delivery which I myself adopt, and which I strongly advise you to follow in all cases where there is a necessity for turning the child.

Period when the operation should be performed.—The time most favourable for the operation of turning, is when the os uteri, vagina, and external parts, are perfectly relaxed, while the membranous cyst remains still entire. The operation, then, should be delayed as long as is consistent with the integrity of the membranes, and the preservation of the liquor amnii within the uterine cavity; for the presence of the water allows the easy introduction of the hand completely within the womb, and permits the child to be turned, by means of the feet, in any direction which is desirable. If, then, we have the conduct of the case from the commencement, let us operate before the membranes break.

But again, we cannot,—and it would be idle to suppose that we could,—pass our hand into the uterus before dilatation has gone on to some extent both in the vagina and in the os uteri. We could not expect to be able to effect our object, when the os uteri is not opened beyond the size of a sixpence or a shilling, unless, indeed, it be much softer than usual under this slight degree of dilatation; but when it has acquired the diameter of half a crown, or a crown, it will generally suffer itself to be dilated to such an extent as will admit the hand, without injury to its structure.

Since, then, we have ascertained that it is highly desirable, under a transverse presentation, that the position of the child should be changed previously to the rupture of the membranes, but that it is almost impossible to accomplish this object before the os uteri has acquired a certain diameter—since, also, there is great danger of the membranes breaking as soon as the bag occupies the vagina, so as to press at all upon the external parts—it becomes of importance that we should lay down

some rule for our guidance in these cases, not perhaps universally to be followed, but to be classed among our general precepts; and the following seems the most applicable.

As soon as the mouth of the womb is sufficiently open to admit the four fingers and thumb as far as their second joint, we may expect that it will offer but a slight impediment to the passage of the hand, and it would be unwise to delay delivery until the perfect dilatation of the organ; because, while we are procrastinating, the very accident may happen which it is so desirable to avoid: the membranes may give way, the liquor amnii may flow out, and the uterine parietes may strongly contract around the fœtal body. It is possible, however, that before the os uteri has attained the diameter I have just specified, while we are anxiously watching the progress of the case, the membranes may unexpectedly break. Under this unfortunate occurrence, even though we should have some difficulty in dilating the os uteri, we must proceed to the delivery, if it can be effected without injury. It is better that we should act thus than wait patiently for a greater degree of dilatation, during which supineness on our part, the lapse of every minute is adding to the danger and distress consequent on the case.

The principal difficulty in operating previously to the rupture of the membranes will usually be found to consist in the dilatation of the os uteri, the external parts, and the vagina: the difficulty afterwards is not so much in passing the hand up to the mouth of the womb as in introducing it into the cavity itself; and the difficulty of dilating the orifice is trifling in comparison with that of overcoming the strength of the uterine contractions.

I have remarked that the uterine aperture has been more difficult of dilatation when the labour was premature; and I attribute the resistance offered to the introduction of the hand, under such a case, to the imperfectly developed state of the cervix, rather than to any spasmodic action in the fibres of the os uteri itself.

Mode of performing the operation.—In the conduct of a case of this kind, then, it is very possible that in our first examination we may not detect positively the nature of the presentation, because the child lies too high for us to reach it easily; but if we find this the case, and that the membranes are coming down in the form of the finger of a glove, these two suspicious circumstances may awaken in our mind a well-grounded fear that it is a transverse presentation. So long as we are in doubt, we must watch the case attentively, and by no means leave the house, even though the os uteri should not be dilated to the diameter of a shilling: but when, in process of time, we have become assured that the child lies transversely, let us not exhibit any indication of anxiety or alarm, for the woman's sake; let us evade her solicitous inquiries as to the "fairness of the case," by some general reply, and hold ourselves in readiness to act with promptitude, should the bag of waters break, or any other untoward occurrence take place. At the same time, however, that we keep her in ignorance of the unfortunate position of the child, it is right that we should apprise her friends that it is "a cross-birth;" that there are no dangerous symptoms at present; that she will require to be delivered by art before the labour can be terminated; and that the delivery, as it must be an artificial one, will necessarily be attended with hazard. After this explanation, if a consultation be required, it is much better to acquiesce than take the risk of the event entirely on ourselves.

Having conscientiously discharged this part of our duty, it is not desirable that we should be in constant attendance by the bed-side of the patient, nor make frequent examinations, lest we should rupture the membranes. We have gained all the information we want; we cannot at present afford any assistance, and we may do irreparable mischief. We may pass our finger occasionally, in the absence of pain, to watch the degree of dilatation that has taken place; and when the os uteri will readily admit the extremities of the four fingers and thumb as far as the second joint, we are warranted in commencing the operation. For the first time, let us now tell the patient that "the child is not exactly in that situation which we could wish, but that we are about to do something to remedy its position." She will most likely be somewhat alarmed, and inquire eagerly if her "child is across." We may reply that its situation requires some alteration; assure her that what we shall do will not place her in danger; and that she will not experience more pain in the aggregate than if the child were presenting naturally, although it may be a little more acute for a short time.

In proceeding to the operation, the first thing to be attended to is the position of the patient, and the second that she should be confined to a certain posture, so that she may not be able to move out of our reach. Unless we put her in a favourable position, we might as well expect to extract a stone from the bladder with the knees close together, as hope to effect a safe delivery. Dewees recommends that the patient should lie upon her back, with the nates brought to project somewhat over the edge of the bed, and the feet sup-

ported by two chairs at a convenient distance; the legs being separated, and the knees bent. I cannot but think that this posture would be very irksome and distressing to the patient, as well as inconvenient to the operator; and I much prefer the position adopted universally in this country,—which is, indeed, that commonly taken under labour,—on the left side, with the knees drawn up towards the abdomen, and the feet resting against the bed-post. The only alteration required, is that she should be brought close to the edge of the bed, that we may have her perfectly under our command. The next point is to restrain her in this position; and this we may accomplish without her knowing that we are confining her at all. We request a friend to take hold of her hand, so as to steady the shoulders. Women know that in common labour, when the expulsive pains come on, they are much assisted by fixing the upper part of their person, through the means of a towel fastened to some unyielding point; and they therefore seldom object to grasping the hand of a friend instead. We must not fail, however, to give this assistant a previous intimation to resist any efforts which she may make to draw herself away. The nurse must then raise the patient's right knee, and separate the legs; by which also the pelvis is steadied in one situation;—and unless she makes a violent effort, she is not likely to move far away. The operator must then take off his coat, for without this precaution he will necessarily be foiled: if he simply turns up the cuff, the hand is only admitted half within the uterus, and no advantage can be gained; should, indeed, the sleeve even allow of being turned up above the elbow, it compresses the biceps, cramps the other muscles, and prevents the free motion of the arm. The left arm and hand must be bared, and anointed with some unctuous application; care being taken not to grease inside of the fingers, or the palm. Kneeling then by the bed-side, rather than sitting, he must bring the tops of the fingers and thumb close together, nearly into the same level; and thus, forming the hand into the shape of a cone, commence the process of dilating, with the utmost delicacy and caution, first the external parts, next the vagina, and lastly the os uteri itself. While thus introducing the hand, it is better not to pause until it be fairly passed within the uterine mouth—unless, indeed, more than ordinary opposition be experienced—and we must by no means withdraw it, lest we lose the advantage we have already gained: we must therefore be prepared to resist the entreaties of our patient to desist from our attempts. My reasons for recommending the left hand to be used in preference to the right, I shall hereafter trouble you with.

The stimulus of our hand will most probably occasion an accession of uterine contraction, and the membranes will be protruded downwards against the extremities of our fingers, and burst without any effort on our part. A small quantity of liquor amnii will escape externally; but as the pelvis is more or less occupied by our hand and arm, a plug is formed, which prevents the entire evacuation of the water; so that this fluid being retained in the uterus, permits the child to make a perfect and easy evolution. The membranes being broken, the hand enters into the centre of the cavity of the ovum, and comes into immediate contact with the fœtal body. It is not absolutely necessary minutely to ascertain the position in which it lies—although to have obtained such knowledge earlier might be desirable; for the fingers being carried round to the chest, the hand may be slid along the abdomen until one or both feet be felt, which must be firmly grasped, and the breech carefully and slowly brought down into the pelvis. I think it highly desirable that both feet should be taken hold of, if they lie together and can be grasped by the same effort, because the evolution of the fœtus is so much more easily accomplished when both are brought down; but if one only be obtained, it is neither necessary nor proper that we should spend time, and inconvenience our patient, by searching for the other. Mr. Radford, of Manchester, indeed (whose name I have before had occasion to mention to you with respect), in a late paper in the Edinburgh Med. Journal, has insisted on the advantage to the child of bringing only one foot down under the operation of turning, because the other leg, bent up towards the abdomen, protects in some measure the funis from pressure, and because the nates thus prepare the passages for the exit of the upper parts of the child's body. The breech being in the pelvis, the principal difficulty of the case is over; it is reduced to one of the first order of preternatural presentations, and must be terminated by the rules of which you are in possession. Some have recommended, after the feet are brought down, that we should leave the case to be concluded by uterine action; but the highest authorities all agree that it is better to terminate it by a continuance of artificial efforts—gently, tenderly, and cautiously applied—taking advantage of the assistance of the pains, and avoiding all hurry or violence.

Precautions. — Simple and easy as this operation may be to an expert hand, it is by no means unlikely that a younger

practitioner will become somewhat embarrassed; and there are some other points, therefore, to which we must devote a portion of our attention, besides the rules already given.

In all cases where it becomes necessary to form this artificial change in the situation of the fœtus, a piece of strong tape should be procured, at one end of which a running noose must be made, to be applied, if requisite, over the fœtal ankle; for this will assist us materially in extracting the breech, provided any difficulty be experienced in performing the proper evolution. This is, indeed, particularly useful, and almost indispensable, when the operation is performed under a strongly-contracted uterus; and its mode of adaptation will be discussed when such cases come under review.

Again, before proceeding to the operation, it is right that we should satisfy ourselves that the bladder is empty; and if distended, it should be evacuated, either by the voluntary efforts of the patient, or by the catheter. We are recommended by some practitioners, indeed, to throw an enema into the rectum in all cases, as well as draw off the urine. If the bowels be much loaded, such a precaution may be highly proper, for the purpose of ensuring as much room in the pelvis as possible, but ordinarily it will not be required; and if not necessary, may be hurtful, partly by complicating our duty, partly by the loss of time which must attend its administration, but principally from the stimulus which may be propagated to the uterus, and which may excite increased action in that organ—a circumstance we should be solicitous to avoid, at least before the introduction of the hand.

Nor is it of small importance that we should perfectly satisfy ourselves it is a foot we have grasped, before proceeding to extract; for if we allow ourselves to be thrown off our guard by agitation, or, anxious to finish the delivery, are seduced into reprehensible haste, and, by mistake, bring a hand down into the vagina, the shoulder will come to occupy the brim; the fœtus will still be transverse, and we have to renew our attempts at "turning" under very much increased difficulties. The two limbs may be discriminated from each other by the marks before enumerated. And I would here strongly recommend you to take every opportunity you can of becoming acquainted with the peculiarities which the different parts of an infant's body present to the touch; and this you may easily effect without exciting observation, by carefully handling those children for whom you may be at different times consulted.

After what I have just now said, it is unnecessary for me to repeat the so-often inculcated doctrine, that force in the introduction of the hand being never called for, is to be deprecated in the liveliest terms; that hurry in the extraction of the child is not desirable, and seldom necessary; and that nothing can warrant us in having recourse to violence. And I shall close my remarks this morning by insisting on the necessity of the hand being directed by the mind, even in the most trifling of our proceedings; and by conjuring you ever to bear in remembrance—during this as well as all other obstetrical operations—the delicacy of the structures within which you are acting.

PHYSIOLOGICAL AND PATHOLOGICAL OBSERVATIONS ON THE BLOOD AND URINE;

Being the Gulstonian Lectures for 1834, *delivered at the Royal College of Physicians.*

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Lecture II.—*Recapitulation of Lecture I.—Effects of alkalies, alkaline salts, and neutral salts, in combination with vegetable acids, on the animal œconomy — Sir G. Blane's opinions on the use of carbonate of potash—Prout's explanation of the power of alkalies and alkaline salts to control the lithic acid diathesis of the urine—Observations and experiments on the albuminous urine of dropsies—Albumen in urine not always to be detected by the application of a heat of* 160°*—Power of the alkalies as solvents of albumen and other soft solids of the body—Their power of promoting absorption — Huxham's case of long-continued use of carbonate of ammonia —Effect on the blood—Use of muriate of soda in digestion—Dr. Stevens's opinions on the use of the salts in the blood—On the production of carbonic acid and animal heat in the capillaries—The alkalescent state of venous blood favourable to absorption of soft solids and coagulable lymph of inflammation.*

Sir,

In my former lecture, I took the opportunity of briefly tracing the rise of the humoral pathology in this country, under the auspices of Linacre and Caius, and following up its history to the time of Cullen, when it met with its complete overthrow. I adverted to a paper from the pen of Dr. Stevens, which was read to

this College, detailing the effects of various substances (particularly acids, alkalies, and neutral salts) upon the human blood. Without wishing to disparage the contributions to physiology made by that gentleman, I recalled the attention of my audience to the experiments and opinions of Boyle, Boerhaave, Huxham, Pringle, Haller, and Hewson, upon this same subject—the effects of acids, alkalies, and neutral salts upon the human blood; and I repeated the experiments of these great men, and exhibited them upon this table. I concluded by stating that the effects of these chemical agents, when injected into the veins of man and living animals, were very similar to their operation upon the blood when fresh-drawn from a vein.

I shall now proceed to consider what are the effects of alkalies, neutral salts, and acids, upon the animal economy—what uses they are applied to by practical physicians—what injurious consequences arise from an excessive quantity of them taken into the system—and what reasons there are for believing that it is through the blood that these, and some other analogous medicines, act upon distant organs.

Alkalies and alkaline salts are extensively employed in the treatment of diseases. In moderate doses they are some of the most innocent remedies in the Pharmacopœia; while in large, or frequently-repeated doses, they form a class of most destructive poisons.

Sir Gilbert Blane long since pointed out the value of doses of carbonate of potash, in certain diseases of the urinary organs; and he came to the conclusion, "that the neutralizing property of alkaline substances does certainly reach the urinary secretion; for (says he) by the internal use of them, the urine loses its quality of reddening litmus paper; nor can it be doubted that alkalies, magnesia, and lime, have the effect of aggravating cases in which the concretions consist of the triple phosphates."

But it is to Dr. Prout that we are indebted for the greater part of our knowledge of the effects of alkalies and alkaline salts on the urine: no one can rise from the perusal of his treatise without feeling he has gained much valuable practical information. He has shewn us how effectually he has been able to control the lithic acid diathesis by a judicious use of alkalies combined with vegetable acids*: when the urine is steadily acid, high coloured, and concentrated naturally, it may, by repeated small doses of saline compounds, containing a vegetable acid, be effectually rendered alkaline, and kept so for any length of time.

* Prout on Urinary Diseases, p. 292.

There is another class of cases in which alkaline salts, and alkalies in combination with a vegetable acid, are employed very generally, and with success;—I advert to that form of dropsy termed inflammatory dropsy, generally arising from the specific action of cold upon the constitution.

It is well known that the urine in such cases, when tested with litmus paper, is found to be acid, and that when heated to 160° Faht. a more or less abundant coagulation of albumen takes place.

The presence of these qualities in the urine is taken by many excellent practitioners as an indication for a particular kind of treatment, which is usually antiphlogistic or depleting, accompanied by the internal use of saline diurcties, very commonly the tartrate of potash.

This plan is continued for a few days, the urine is again tested with litmus paper, and no excess of acid is detected: it is submitted to heat, and no coagulation of albumen ensues, as upon the first occasion. A certain step is now considered to have been gained: the physician flatters himself that he has got rid of the albumen; but this I believe to be sometimes an ill-grounded conclusion.

From the observation of a number of cases of dropsy in St. Bartholomew's Hospital, some arising from cold, others from organic disease, especially of the kidney, I have remarked for some time past that patients with albuminous urine likewise had that secretion preternaturally acid. As soon as the urine was rendered neutral, or approaching to an alkalescent state, by the internal use of saline compounds with vegetable acids, no coagulation of albumen took place, upon the application of the test before employed, a heat of 160°.

It often struck me as a most remarkable fact, that upon one visit to St. Bartholomew's Hospital I found several patients in a ward with albuminous acid urine; and, upon revisiting the patients after the interval of twenty-four hours, that their urine had become neutral, or nearly alkaline, and that no albumen could be detected; while, after another interval of twenty-four hours, their urine had again become acid and albuminous.

Reflecting upon these observations, it appeared to me probable that the change from an acid to a neutral or alkalescent state of the urine in these patients was the cause of the non-appearance of coagulated albumen upon the application of heat, rather than that this secretion should have undergone such rapid transitions in its component parts, in such short intervals of time. To elucidate this opinion, I made the following experiments upon the urine of persons labouring under dropsy

from structural change of the kidney, from the injurious effects of cold, and in that form which appears after scarlatina. In all of these cases the urine was albuminous and acid.

Case I.—A young woman, about 18 years of age, was admitted during last winter into St. Bartholomew's Hospital, under the care of Dr. Roberts, suffering from ascites and general anasarca. She stated that her complaints had lasted only a few weeks, and she attributed them to a violent cold caught by constant exposure to bad weather.

Her urine was scanty, turbid, and changed the blue litmus paper to a bright red. A portion of this urine, exposed to heat, at first became clearer; but as the heat advanced up to 180°, an abundant coagulation of albumen took place; so much so indeed, that the portion of urine was almost converted into a mass of coagulated albumen, resembling hardened white of egg.

To a portion of this urine, equal to that employed at first, I added a few drops of liquor ammoniæ, until turmeric paper was changed to a brown colour. I then submitted this quantity to the action of heat until it boiled, but no coagulation of albumen took place as before. The same result was obtained upon the employment of liquor potassæ, instead of liquor ammoniæ. In this instance, then, it is evident that heat alone was not a sufficient test for the presence of albumen. Upon neutralizing the alkali, and rendering the urine again acid, the application of heat readily coagulated the albumen. This woman died, and, upon dissection, her kidneys were found in that white, hardened condition, described by Dr. Richard Bright.

Case II.—In January last a man was admitted into St. Bartholomew's Hospital, under the care of Dr. Latham, suffering from dropsy arising from cold: the anasarca was general. At my request he passed two ounces of urine into a clean vessel: it changed litmus paper to a pale pink colour.

A small quantity of this urine was heated in a watch-glass over a spirit lamp, when flakes of albumen made their appearance. To this portion of urine thus treated I added a few drops of liquor potassæ, until it was decidedly alkaline. Upon stirring the fluid it gradually became clearer, and finally the coagulated albumen was entirely re-dissolved in the urine.

A few drops of nitric acid were then added to this same quantity, and albumen was again coagulated.

Another portion of this urine, equal in quantity to the former, was poured into a watch-glass, and a small quantity of liquor ammonia added to it, until the urine was decidedly alkaline: the application of heat had no power of coagulating the albumen when the urine was in this alkaline state.

A few drops of solution of corrosive sublimate were dropped into a small quantity of this urine, and an immediate white precipitate was formed.

In this experiment heat alone was proved to be an insufficient test of the presence of albumen in the urine; and it likewise appeared that liquor potassæ had the power of re-dissolving coagulated albumen. This property of liquor potassæ to retain albumen uncoagulated, when exposed to a heat of 160° or 180°, and also of re-dissolving coagulated albumen, I believe is not generally known.

Dr. Bostock, indeed, states that "the chemical relations of coagulated albumen differ from those before coagulation. After this," according to Dr. Bostock, "it becomes completely insoluble in water*." This, however, I venture to say is not perfectly correct; for if the albumen be coagulated by a feeble acid, or by the application of moderate heat, it is again soluble in an alkaline solution†. This property of an alkaline solution to re-dissolve coagulated albumen seems to strengthen the opinion of Mr. Brande‡, who regards liquid albumen as a solution of solid albumen in alkali; and that galvanism, acids, and alcohol, coagulate it by the abstraction of alkali. Albumen, in its liquid state in the egg, is also alkaline, being combined with soda.

Case III.—In the month of February last I examined the urine of a boy, dropsical after scarlatina. I found the urine acid, and upon the application of heat, flakes of albumen were very apparent. To a small portion of this urine, with its albumen coagulated, I added a few drops of liquor potassæ, which, after a short time, re-dissolved the albumen; and, upon the second application of heat to this alkalescent urine, no coagulation took place.

Case IV.—Charles Brown, æt. 20, a fisherman, was admitted into St. Bartholomew's Hospital, under the care of Dr. Latham, May 1, 1834, suffering from hæmaturia to a great extent, which he stated had first appeared after a violent strain. His urine was abundant, highly acid, of the colour of damson juice, and when exposed to the temperature of 160°,

* Physiology, vol. i. pp. 440 and 472.

† If the albumen be coagulated suddenly by a strong heat, I have found it much less soluble in alkalies than when a moderate heat is employed. In the former case the albumen undergoes some decomposition, for almost all its former properties are altered.

‡ Philos. Trans. 1809, p. 373.

exhibited a dense white cloud of albumen. He was ordered to take ʒss. of tartrate of potash every six hours.

In twenty-four hours the urine had increased in quantity; there was, however, much less appearance of blood in it.—When tested, it appeared quite neutral, and no coagulation of albumen took place on the application of heat.

After forty-eight hours from his admission, the urine again contained a larger quantity of blood. When it had stood some time, spontaneous coagulation of fibrin formed in it; the urine was still neutral, but no coagulation of albumen was produced upon heating it up to 212°, although, from the large quantity of blood in it, the albumen must have existed there in abundance. He then discontinued the internal use of the tartrate of potash, and his urine gradually became pale, clear, slightly acid, free from blood, and no albumen could be detected in it.

This case exhibits more strongly than the three former, that the alkaline salts, and salts with a vegetable acid, have not only the power of changing the urine from a highly acid to an alkalescent state in a very short time, but that when the urine is thus changed, that it has the power of retaining a very large quantity of albumen in solution, and that this is not detectable by the application of heat in the ordinary method.

The lecturer here performed the following experiments on albuminous urine:—

1, *Albumen coagulated by heat; 2, Re-dissolved by alkalies; 3, Albumen rendered permanently soluble by the addition of liquor ammoniæ, liquor potassæ, and carbonas potassæ; 4, Albumen not rendered permanently soluble by sulphate of soda, but coagulates on the application of heat; 5, Albuminous urine converted into a gelatinous fluid by the addition of solution of tartaric acid.*

From these experiments I think it may be inferred that the application of heat alone is not a sufficient means to determine the presence of albumen in the urine. Unless the urine be acid naturally, or rendered so chemically, the albumen may escape detection. Blackall long since made the remark, that he believed that "the serous urine (albuminous) at the time of its being made, is never without an excess of acid*." This observation of Blackall's, I think, may be thus rendered,—that, unless the urine be acid, no albumen will be discovered, or *the "serous" qualities will not be discovered.*

If the physician, then, should be called to a dropsical patient, whose urine upon analysis proves neutral, and where no coagulation takes place on the application of heat, he must not conclude that no albumen exists in that urine, nor that the kidney is not undergoing that change of structure which has been so faithfully described, and beautifully delineated, in the work of Dr. Bright*.

About four years since I met with the following case, in which I now think, from my then ignorance of the foregoing facts with respect to the great solubility of albumen in alkaline solutions and alkalescent urine, that I fell into an error.

A sailor was admitted into St. Bartholomew's Hospital, with ascites and general anasarca. From his own account his complaints had originated from cold, and consequently saline diuretics were prescribed for him. His symptoms, after a short residence in the hospital, indicated a deeper-seated cause of disease; and his kidneys were suspected to be affected. His urine was heated frequently during the last three weeks of his life, but no coagulation of albumen took place.

I examined this man's body after death, and found that the kidneys had undergone that kind of disorganization described by Dr. Bright. This case, at the time, appeared to me to militate against the statements of that author, whose object was to establish the fact, "that certain dropsical affections depend more on the derangement of the kidneys themselves than has been generally supposed; and that the albuminous nature of the urine frequently points out the particular cases in which these organs are the seat of the disease†."

In this case no albuminous nature of the urine had pointed out the change of structure in the kidneys. I therefore took the kidneys to Dr. Bright, who said they were excellent specimens of the disease he had described, but he could not satisfactorily account for the absence of albumen in the urine.

Little doubt, however, now remains in my mind upon this case. The test for albumen I had employed was insufficient, after the long use of saline diuretics by this patient. Subsequent experience has convinced me of the accuracy of Dr. Bright's observations; and I am inclined to believe, with that author, that in all cases in which I have observed the urine albuminous, that the kidney itself has acted an important part; that it was already functionally deranged, although, very often, not organically affected‡."

* Observations on Dropsies, p. 203.

* Diseased Kidney in Dropsy. Dr. Bright, &c. &c.

† Ibid, page 70.

‡ Ibid, page 4.

In the cacheetic class of dropsies, with albuminous urine, I have generally found the kidney disorganized; but in the slighter forms of dropsy from cold, and that coming on after scarlatina—in both of which class of cases the urine contains an excess of albumen—there have not appeared to me sufficient grounds for suspecting the kidneys to be organically affected.

The alkalies, then, not only possess the power of retaining albumen in solution uncoagulated, when exposed to a heat of 160°, but, under certain circumstances, they possess the power of re-dissolving coagulated albumen. But it is not over albumen alone, of the various kinds of animal matter, that the alkalies possess the power of solvents; it extends to almost all the soft solids of the animal body. Fibrin, fat, and medullary matter, are soluble in strong alkaline solutions; and it is well known that concentrated alkalies will soften many animal tissues, such as the skin and mucous membranes, reducing them to a pulpy mass.

Pus and softened tuberculous matter are almost entirely soluble in strong solutions of caustic potash. In short, it would appear that alkaline fluids are the proper solvents of animal matter in general.

How far do we find, from experience, that the internal use of alkalies and neutral salts, formed with vegetable acids, produces effects upon the blood and soft solids similar to those observed out of the body? Dr. Prout has fully established the fact, that the long-continued use of neutral salts, in which the acid is of vegetable origin, and alkaline remedies, will produce a tendency to the phosphatic diathesis with alkaline urine.

I have attempted to show, in this lecture, that these remedies, when taken internally, convert the urine into a fluid capable of holding a large quantity of albumen in solution, which is not coagulable by heat; and that this arises from the alkalescent state of the urine.

Is it not probable that these alkaline remedies will exert their influence at other points of the animal economy, as well as in the kidney, and thus render the soft solids, in different parts, in a state more fit for absorption?

In fact, physicians are continually employing the pure alkalies, their carbonates and salts, with vegetable acids, both to excite absorption, and the separation of a larger quantity of watery secretions, either as diaphoretics or diuretics.

It is now very generally believed by physiologists, that the human stomach possesses the power of decomposing all saline compounds, into which vegetable acids enter as ingredients, and of eliminating their alkaline bases. These, in the course of the circulation, are carried both to the skin and kidney; and it will be found upon inquiry, that almost every one of our saline diuretics, or diaphoretics, are compounds with vegetable acids. The alkalies, then, act as diuretics, or diaphoretics, according as the circulation is determined by accidental circumstances, or individual idiosynerasics, either to the internal or external parts of the body.

Nitrate of potash is the only saline diuretic which is not a compound of a vegetable acid with an alkali; and the mode of operation of this salt is certainly different from that of salts with vegetable acids. There is little doubt but that nitre enters the circulation unchanged, and stimulates the kidney to an increased secretion of urine, in which it has been detected unchanged*. There are symptoms also referable to the head not uncommonly attending the internal use of nitre, which, I believe, do not occur with other saline diuretics. I have been in the habit of suspecting that these violent pains in the head were caused by the large quantity of nitre circulating with the blood through the cranium.

But what are the effects of the alkalies, their carbonates, and salts, with vegetable acids, when administered in long-continued or excessive doses†? The fixed alkalies, and their carbonates, when administered in excessive doses, destroy the texture of the mucous membrane of the stomach and intestines. This lesion, of course, impairs the powers of digestion and assimilation; and a lingering death, almost resembling starvation, is the result.

Huxham illustrates the effects of a long-continued use of carbonate of ammonia upon the animal economy by a most interesting case, which I shall here relate‡.

"I had lately under my care," writes Huxham, "a gentleman of fortune and family, who so habituated himself to the use of vast quantities of the volatile salts, that at length he would eat them in a very astonishing manner, as other people eat sugar and carraway seeds. The consequence soon was, that he brought on a hectic fever, vast hæmorrhages from the intestines, nose, and gums; every one of his teeth dropped out, and he could eat nothing solid; he wasted vastly in his flesh, and his muscles became as soft and flabby as those of a new-born infant; and he broke out all over his body in pustules. His urine was always excessively high-coloured, turbid, and very fœtid. He was at last persuaded to leave off this pernicious cus-

* Thomson's Dispensatory, page 530.

† Christison on Poisons, first edit. p. 153. (Of the alkalies and alkaline salts.)

‡ Huxham, p. 54.

tom; but he had so effectually ruined his constitution, that though he rubbed on in a miserable manner for several months, he died, and in the highest degree of marasmus. And I am persuaded he would have died much sooner, had he not constantly drank very freely of the most fine and generous wines, and daily used large quantities of asses' milk and anti-scorbutic juices, acidulated with juice of lemon."

It was Huxham's opinion *, "that the volatile alkaline salts were often improperly administered in putrid pestilential fevers, or of the petechial kind, in which the blood was already too much broken and dissolved." He thought that when the blood is largely stocked with them, "that it becomes a kind of fiery lixivium, which is destructive of the nervous fibrillæ and ultima vascula."

He has given the above case in confirmation of his opinion; and Christison, in his work on Poisons †, briefly adverts to it, as a good specimen of the injurious effects of long-continued doses of a volatile alkali on the human body.

The alkalies, their carbonates, and salts, formed with vegetable acids, when administered in frequent small doses, exert their chemical properties on the urine; and this can only take place through the blood.

When, however, the alkalies are administered in excessive doses, it would appear that they exert their disorganising effects upon the blood itself, in a manner very similar to their effects on that fluid when mixed with it when fresh drawn from a vein.

The other saline compounds employed in the treatment of diseases, consisting of alkalies in combination with mineral acids, are almost all of them found existing in urine in perfect health. These salts are the sulphates of potash and soda, the phosphate of soda, and the muriates of soda and ammonia.

It is not ascertained that the stomach possesses any power of decomposing the sulphates and phosphates. When administered internally, they perhaps enter the blood in part unchanged, but the greater part passes along the alimentary canal, exciting the mucous membrane to increased secretion; and in this manner they act as purgatives.

The muriate of soda existing in the blood and urine in health in larger quantity than any other salt, probably performs most important services in the functions of digestion, assimilation, secretion, and perhaps, also, in the maintenance of animal heat.

The muriates found in human blood are triple the quantity of those in ox blood and that of other graminivorous animals, owing, doubtless, to the salt consumed by man in his food *.

I shall not scruple to quote the opinions of Dr. Prout upon the action of muriate of soda in the function of digestion.

The food having been previously broken down by mastication, and received a due admixture of saliva, is introduced into the stomach, where it has to become associated with water, in order that it may enter the lacteal vessels for the support of the body.

"Of the important secretion of the stomach," says Dr. Prout †, "chlorine, in some state or other of combination, is an ingredient. The powerful influence of this elementary principle seems mainly to contribute towards effecting the union of the food with water. It often happens, that instead of chlorine, a quantity of free muriatic acid is elicited. The source of this chlorine or muriatic acid must be the common salt which exists in the blood: to suppose that it is generated, is quite an unnecessary hypothesis. The chlorine is therefore secreted from the blood. Perhaps the decomposition of the salts of the blood may be fairly attributed to the immediate agency of electricity. What becomes of the soda," inquires Dr. Prout; and I would add, of the potash also, "from which the chlorine has been disunited?" These alkalies remain behind, partly, of course, in the blood, in order to preserve the weak alkaline condition essential to the fluidity of the blood, and partly combined with alimentary matter in the stomach, for the purpose of rendering it more readily soluble in water.

The opinion of Dr. Prout, that in the process of digestion chlorine is disunited from the muriates, and that the alkalies remain for the purposes just mentioned, appears to me to be strengthened by the following reasons, in addition to those adduced by that philosopher, in his recent essay on the chemistry of organization.

In the digestive process, it is evident that some agents are at work which have the power of reducing the heterogeneous mass of materials taken into the stomach at an ordinary meal into a homogeneous, almost colourless and inodorous fluid, the chyle.

Now chlorine out of the body is known to possess the singular properties of destroying animal odours and vegetable colours, and of combining with albumen. If an albuminous fluid be submitted to the action of the deutochloride of mercury, the attraction of chlorine is so great for albu-

* Huxham on the Use of Volatile Alkaline Salts, p. 54.

† Page 168; first edit.

* Berzelius's Chemical Properties of Animal Fluids; Med.-Chir. Trans. vol. iii. p. 220.

† Prout's Bridgewater Treatise, p. 494.

men, that one proportion of chlorine quits the mercury, and combines with the albumen, leaving the deutochloride in the innocuous state of protochloride, or calomel. Besides this, the alkalies, combining with albumen and other soft animal solids out of the body, possess the power of reducing them to a pulp, and rendering them more soluble in water.

This attempt to explain some of the phenomena occurring in digestion upon chemical reasoning, has this point in its favour, that it only assumes that principles whose effects out of the body can be studied and submitted to the test of experiment, have a corresponding action in the body.

Sir John Pringle made a very extensive series of experiments on the effects of various chemical agents mixed with animal matter, and he attempted to draw up a scale of their antiseptic virtues. Now I am aware that many of his conclusions are founded upon principles which modern chemistry has proved to be incorrect; nevertheless, it is singular that his experiments upon the effects of muriate of soda brought him to the following opinion:—

"Sea salt, the indispensable seasoner of animal food, has been supposed to act by an antiseptic quality, correcting the too great tendency of meats to putrefaction. It should seem, from my experiments, that salt is subservient to digestion, chiefly by its septic virtue—that is, by softening and resolving meats; an action very different from what is believed *."

Such, then, are the probable uses of the muriates of soda and potash, in the important function of digestion.

These and other salts of soda and potash would appear, from recent inquiries, to be of the utmost consequence to the healthy condition of the blood; and are, perhaps, likewise essential agents in the generation of animal heat at every part of the system.

It has long been known to physiologists, that a certain quantity of saline matter is constantly present in the blood, as well as in all other albuminous fluids; and they were naturally led to conclude that those salts performed some useful purpose in the animal economy.

Up to the time when Haller wrote his Elementa Physiologiæ, neither the nature of these salts nor their uses were understood. This physiologist informs his readers †, "Salem quidem sanguini inisse sapor prodit. Quis autem is sal sit, nondum inde definitus. Variis modis ad naturam hujus salis, aliorumque elementorum sanguinis indagandam acceditur, (1) admistis salibus, (2) per observatos morbos, (3) per putredinem, denique ignis torturâ adhibitâ ‡.

The conjectures of more modern writers, that these salts may stimulate the nerves of the heart—that they may aid in the operations of secreting organs—that they may contribute to the process of digestion—may all be found concisely detailed in Dr. Bostock's Physiology *.

Dr. Prout has recently given a more rational explanation of their operation in the digestive process, which I have detailed to you; and to his theory I have ventured to add a few corroborative facts. Dr. Stevens, on the other hand, has offered some most ingenious views of the agency of these salts on the blood, and of the manner in which animal heat is produced at every part of the system.

Dr. Stevens announced the followed propositions, supported by a long and rather tortuous train of arguments, and by many experiments, most of which I have repeated, and found to be essentially correct.

1. That the blood owes its red colour entirely to the presence of the saline matter in it.

2. That the dark colour of venous blood arises from the presence of carbonic acid.

3. That the oxygen of the atmosphere can only affect the colour of the blood by attracting carbonic acid from it; and

4. No arterial tint is produced on the blood by the action of the atmosphere, unless saline matter is present.

The effects of acids, alkalies, and neutral salts in general, when mixed with the blood, I have already mentioned and exhibited here.

There are now strong reasons for believing that some power similar to that existing in the stomach, and which decomposes the muriates there, exists in all other parts of the animal system.

It had long been a disputed question among physiologists, whether the carbonic acid expired in respiration was formed in the lungs, by the combination of the oxygen of the atmosphere with the carbon of the venous blood, or whether a certain portion of oxygen entered the blood in the lungs, and was carried into the circulation, and at the same time carbonic acid was formed in the course of the circulation, and only thrown off in the lungs. The experiments of Dr. W. F. Edwards have inclined physiologists of the present day to adopt the latter opinion—that carbonic acid is formed in the course of the circulation, and is merely thrown off by the lungs. The striking fact brought into public notice by Dr. Stevens, that oxygen has a powerful attraction for carbonic acid, corroborated this opinion of Dr. Edwards, founded upon experiments totally different

* Pringle on Diseases of the Army, p. 352.

† El. Physiol. Lib. 5. Sect. 2. Art. 23, 24.

‡ Haller himself, however, came to no very satisfactory conclusions, although he went through a number of experiments. With all his great qualities, Haller was an indifferent chemist; and he was himself aware of his defect, and lamented it.

* Vol. I, p. 479.

from those of Dr. Stevens. Admitting this view of the question to be the correct one, it is natural to inquire, where is this carbonic acid formed?

Dr. Edwards, Dr. Stevens, Dr. Hodgkin, and others, who have paid particular attention to this part of physiology, think that with the formation of carbonic acid at every part of the animal frame, heat is evolved; and that the production of this animal heat always bears a close and necessary relation to the quantity of carbonic acid produced. The carbonic acid existing in the venous blood is ascribed, by these physiologists, to the union of the oxygen imbibed by the blood in the lungs, with certain proportions of carbon at every part of the animal frame to which the circulation of the blood extends.

The feeble connexion between the alkali and the acid in the alkaline carbonates—the powerful attraction of the oxygen of the atmosphere for carbonic acid—even exerting its influence through membranes of some density, and the power which exists in the animal economy of decomposing saline compounds, are reasons which lead me to believe that the carbonic acid of the venous blood is in part due to the decomposition of the carbonates of the blood in the capillaries. This fluid is, then, left in a slightly alkalescent state. Now all solid parts of our frame must be dissolved, or rendered soluble in water, before they can be absorbed, or removed; and it will be remembered that most of the soft solids are soluble in an alkaline fluid; so that admitting the partial decomposition of the alkaline carbonates, a use is pointed out for their constant appearance in healthy blood; for, without them, it would be difficult for absorption of albuminous matter to take place.

I would here remind my audience how much pathology has been improved by the attention paid to morbid actions and reparative processes going forward on the surface of the body; especially in ulcerations and adhesions of divided parts. Information no less important in elucidating the operations of the animal economy, has been afforded by careful observations on diseases of the eye. In that disease of the eye termed iritis, if the inflammation be not controlled by art, the disease goes on to its second stage, and coagulable lymph is thrown out into the anterior chamber of the eye. The globule of lymph can be carefully watched through the transparent cornea, and if the inflammation proceeds, vessels can be seen forming upon it. It thus becomes organized, and its subsequent absorption is then slow and difficult. But if the inflammation be arrested by copious bleeding and the free use of mercury, we observe the globule of lymph becoming ragged at its edges, gradually diminishing in size, and eventually it disappears. I believe this lymph has been gradually and slowly dissolved in the aqueous humour. The lymph is a modification of albumen: the bleeding diminishes the quantity of the albuminous parts of the blood; the mercury not only stimulates the circulation, but it produces increased secretion from neighbouring glands, and has a tendency to increase the alkalescence* of the urine and other secretions. All circumstances, then, conspire to the solution and absorption of coagulable lymph effused in inflammations†.

QUESTION

OF THE

ORIGINALITY OF SIR CHARLES BELL'S DISCOVERIES IN THE NERVOUS SYSTEM.

To the Editor of the Medical Gazette.

Sir,

The following extracts from a paper in the Edinburgh Medical and Surgical Journal, which has just been published, will at once shew the object of the communication in which they are contained, and the spirit which animates the writer. M. Bellingeri, of Turin, is the author whose Inaugural Dissertation is the subject of the critical analysis. The passages stand thus:

"We propose, in giving some account of the inquiries of the learned foreigner whose writings are enumerated at the head of this article, to consider the doctrines taught on the fifth and seventh nerves by that physician, several years before the time at which Sir Charles Bell published; and to see to what extent they agree or differ from those inculcated by the latter author. It is a remarkable fact that five years, at least, before Sir Charles Bell communicated to the Royal Society of London his peculiar doctrines on the uses of the fifth and seventh pair of nerves, Bellingeri had published a most minute and elaborate view of the anatomical distribution and

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* Dr. Prout on the Phosphatic Diathesis of the Urine.

† In Dr. Burrows' lecture, in last number, page 504, last line of column 1, *for* "solida," *read* valida; page 504, line 6, column 2, *for* "regetantium," *read* vegetantium; page 508, line 34, column 2, *for* "eusque," *read* usque.

physiological uses of these nerves, and on many of the most important points of the latter department had anticipated the British physiologist. That this circumstance has not been known to English physiologists at an earlier period, is not wonderful, when we consider how indifferent the profession at large is to the elaborate and elegant productions of the anatomists and physiologists of the Transalpine Peninsula," &c. p. 114.

" We have no doubt that every one who peruses the dissertation with attention, will be satisfied that the Italian has given a much more clear and connected view of the anatomical and physiological history of these two nerves (the fifth and the portio dura of the seventh) than any one of the writers who have yet attempted the task."—Page 136. Again, the reviewer continues—

" It must be rather mortifying for Sir Charles Bell to find, that while, on the one hand, Majendie lays claim to his discovery of the different functions of the anterior and posterior roots of the spinal nerves, and concedes to him the merit of distinguishing the sensiferous faculty of the larger portion of the fifth pair, and the motiferous faculty of the small portion, and (of ascertaining the functions of) the seventh pair; on the other, the just claims of Bellingeri deprive him even of any title to the latter discoveries. The only parallel situation which we can remember is that of the old Abbot, who, when told that some person had before said all that he was now saying, addressed his remembrancer in the following characteristic language:—' Pereant illi qui ante nos nostra dixerunt.' "

Let me proceed at once to examine the nature of this alleged anticipation of the discoveries for which, it would appear, Sir Charles Bell has obtained so much misplaced and undeserved celebrity.

The fundamental principle which pervades every sentence of Sir Charles Bell's works, is this (and it was announced in the unpublished tract referred to by the reviewer, printed in 1811); that a single nerve cannot bestow both motion and sensation. The former endowment infers a nervous influence propagated outwardly from the brain to the moving parts; while the latter infers an influence conveyed in a directly opposite course—that is, from the sentient surface inwards to the sensorium. These two properties, he concluded, were incapable of existing together in the same nerve; whenever they are combined, it is a sign that the nerve is compound—that it originates by two distinct roots from the brain or spinal marrow.

Founding on this principle, Sir Charles Bell announced what were the functions of the two principal nerves of the face and head—the fifth, and the portio dura of the seventh. The fifth pair arises from the brain by two distinct roots, each possessing a peculiar anatomical character; one having a ganglion formed upon it, while the other has not. In accordance with the principle above stated, he said that this is a double nerve: in virtue of its two distinct roots, it possesses two separate functions; it is at once the nerve of sensation to all the head, and the motor nerve of the muscles of mastication. The portio dura, however, is of a totally different character from the fifth; it has only one root, and has no ganglion near its origin. This nerve has, accordingly, but one function—it is a motor nerve; it can carry a mandate only outwards to the muscles; it has no power of carrying a sensation inwards. These opinions received the fullest and most unquestionable confirmation from experiments upon living animals, performed by Sir Charles Bell himself, and also by his zealous assistant in all these pursuits, the late Mr. John Shaw. For example: when those branches of the fifth which are derived from one of the roots simply—viz. from the large portion—were cut across in the living animal, the surface was immediately deprived of sensation, while the parts continued to enjoy their power of motion as perfectly as before the experiment was made; that is to say, only one nervous endowment of the part was lost by cutting through the nerve. When, however, the branches of the third division of the fifth—viz. those composed of the two roots joined together, and which are distributed to the muscles of the jaws, as well as sent to the skin—were divided with the knife, the animal was at once deprived of two distinct functions. Not only did it lose the sensibility of the parts to which the particular branches were distributed, but the muscles of the jaws ceased, from that in-

stant, to contract. Thus it was indisputably shewn that the larger portion of the fifth was endowed with sensation solely, and had no power of stimulating the muscles; while the smaller portion conferred motion on the muscles engaged in mastication.

In reference to the portio dura, again, the experiments upon it were equally satisfactory: when its principal trunk was cut across, the muscles to which it is distributed immediately lost all power of motion; but the sensibility of the surface was not, in the slightest degree, diminished. It thus became evident that the portio dura was simply a motor nerve, and had no share in contributing to the sensibility of the surfaces on which it is distributed.

But it is not to be supposed that this principle—that a single root bestows only one function—rested altogether upon the experiments on the nerves of the face. It was not with these nerves that Sir Charles Bell began his investigations. It is well known that he commenced by experimenting on the spinal nerves. It was after having ascertained by experiments (first made so far back as 1811, and repeated in March 1821), that the two roots of these spinal nerves possess distinct endowments, that he was led to examine the functions of the fifth pair, and to classify it with the spinal nerves.

When we speak, therefore, of the validity of the proofs by which he established that the fifth pair is a double nerve—"the nerve of sensation and mastication"—we must not omit to take into consideration the experiments on the spinal nerves which corroborated his conclusions. Again, when speaking of the portio dura, a single nerve, which he said was endowed only with one property, viz. that of motion, we are not to forget his experiments on other nerves, which he likewise succeeded in shewing, when they had only one root, possessed only the power of motion, and did not bestow sensation. If we consult the several essays written by Mr. John Shaw, on the first promulgation of these views, we shall find that numerous experiments and observations had been made to confirm this principle, besides those on the two nerves in question, decisive as they must be allowed to be.

Let us now examine what were the opinions advanced by M. Bellingeri, by which, it is alleged, these subsequent inquiries of Sir Charles Bell were made so useless and nugatory.

The confident tone of the reviewer, in the assertions which we have extracted, would lead us to expect the most complete coincidence in the statements of the two physiologists; that they agreed in the general principle—that they pursued the same method in conducting their inquiries, viz. first, by examining the anatomy, and then by performing experiments—and that the results of their combined observations and experiments were the same.

The reader will find, on the contrary, in Bellingeri, a direct opposition to the principle which I have stated—a totally different mode of pursuing the investigation—a signal clashing of opinions and statements with those of Sir Charles Bell; and he will inquire in vain for any single point of harmony or concurrence on which to make out a case of anticipation.

In the first place, the Italian physiologist differs from Sir Charles Bell with regard to the functions of the principal root of the fifth pair—that is, the ganglionic root. Is this difference of a trivial kind? It is, on the contrary, upon a point the most essentially important of any that could be proposed by a person drawing a parallel on such a subject. Sir Charles Bell affirms that this larger root is dedicated entirely to one single office, viz. that of *sensation*. M. Bellingeri, on the other hand, pretends that, besides conferring sensation and numerous other qualities usually assigned to nerves, *it regulates the actions of the muscles of the face;* he terms it the nerve of "physiognomical expression." I have already mentioned that Sir Charles Bell related experiments to corroborate what he stated. *M. Bellingeri has not been at the pains to make a single experiment!* But he narrates a case which, he says, illustrates his proposition. The patient had complete paralysis of the muscles of one side of the face—that is, the frontal muscles, those of the nostril and of the lips, were deprived of motion: in short, all the muscles, except the masseter and temporal, were paralytic. Now this, M. Bellingeri presents as an example of an affection "of those muscles in which the large portion of the fifth is distributed." It is superfluous to say, that this was an undoubted case of disease

of the portio dura; yet this explanation is not once hinted at. The comments of the reviewer immediately following this notable illustration of the functions of the ganglionic root of the fifth pair, are worth extracting. "The particulars of this case, and the explanations, which were published, as we have stated, in 1818, shew, we conceive, most satisfactorily, that Bellingeri understood perfectly the distinctive physiological properties of the two divisions of the fifth pair, several years before the theory of the influence of these parts was explained by Sir Charles Bell. To us, also, it appears that the explanation given by Bellingeri is much clearer, more precise, and more methodical, more logical, and less embarrassed with irrelevant statements, than we any where find it in the writings of Sir Charles Bell." (p. 125.)

I presume that the writer, in mentioning "irrelevant statements," alludes to the *experiments* on the nerves of living animals, which accompanied Sir Charles Bell's explanation, none of which embarrassed that of M. Bellingeri! Or does he refer to the frequent statements made by Sir Charles Bell, in confirmation of his views, respecting the similarity between the two roots of the spinal nerves, and the two roots of the fifth pair? Does he mean to say, that the experiments on the spinal nerves, which established that the posterior roots are endowed with sensation only, do not corroborate those made subsequently upon the corresponding root of the fifth pair. If M. Bellingeri had, like Sir Charles Bell, first experimented on the posterior roots of the spinal nerves, and ascertained that they are dedicated to sensation only, having no influence over the muscles, should he not have escaped the fatal blunder of making the analogous root of the fifth pair preside over the actions of the muscles of the face? As it has happened, however, this lucky foreigner—neither making experiments on the fifth pair, nor on the spinal nerves, nor hitting upon the true functions of the nerve in question, but keeping all things in their pristine confusion—has gained the unqualified applause of this reviewer; nay, he is held up as one injuriously dealt with by the whole profession!

In the second place, the Italian physiologist differs from our English physiologist in regard to the functions of the portio dura. The opposition here too like that in the former instance, is just of such a nature as to present the widest breach between these physiologists that could be imagined by any one resolved to put them in absolute contrast. Bellingeri conceives that the portio dura is a compound nerve; that it is endowed with both *motion* and *sensation*. I have shewn that Sir Charles Bell describes it to be a nerve of *motion only*, and altogether devoid of any property of sensation. The words of the reviewer, however, are—"he (Bellingeri) infers that it is reasonable to think that the voluntary motion of the muscles of the face, and the animal sense of *touch*, depend on the influence of the seventh."—P. 124. In another part he says,—"Bellingeri next proceeds, in his second chapter, to adduce such evidence as may show that the seventh pair of nerves, or facial, presides over the animal functions of the head, face, and neck; that is to say, over *sensation* and motion, as animal or voluntary faculties."—P. 133. In the same page he gives the portio dura the name "musculo-cutaneous;" and he refers to it as being sometimes the seat of *neuralgia*. We are told that the Italian made experiments on the portio dura in rabbits: and let us see what were the results: — "Complete palsy of the eye-lids and upper and lower lips ensued, *and the sensibility of the face appeared very much impaired, for, on plucking out hairs by the roots, it gave no signs of sensibility or pain.*" —P. 134.

It remains only to speak of the lesser root of the fifth, or that which passes the ganglion, and unites itself with the third division of the nerve, to supply the muscles of the jaw, and the angle of the mouth. Here we have at length to acknowledge some coincidence in the views of the "transalpine" and "tramontane" physiologists*. Sir Charles Bell, proceeding on the previously-ascertained fact, that the ganglionic root of the fifth resembles

* *Transalpine* and *tramontane* are employed by the reviewer, with his usual attention to accuracy, as terms in contrast, signifying different sides of the Alps! That Turin is in the ancient Cisalpine Gaul has, perhaps, been floating in his mind; but his confusion in applying words having the same meaning, both to the English and to the Italian physiologist, and from being obliged to transport them or himself, alternately, across the Alps, that they may stand relatively on different sides, is like that of a man fallen into a vertigo.

the posterior roots of the spinal nerves, in structure as well as function, concluded that the other root, which has no ganglion, corresponded with the anterior roots of the spinal nerves, likewise having no ganglions; and that it conferred motion on the muscles of the jaw. The accuracy of this deduction from the anatomy was demonstrated by the experiments upon the third division of the fifth, performed by Mr. John Shaw*. Now, although Bellingeri made no experiments of any kind upon any part of the fifth, yet it is to be conceded that he was acquainted with the true physiological character of the lesser root. But he was indebted for this knowledge to one of his countrymen! He only repeated the assertion of another Italian in a neighbouring school,—I mean Paletta. It was this anatomist who first suggested (1784) that the anterior portion of the fifth pair, which he termed nervus crotaphitico-buccinatorius, and showed was distributed exclusively to the muscles of the jaws, must be a nerve of motion. He pointed it out to be the nerve which is affected in *trismus*. Neither has Sir Charles Bell omitted to give the credit due to Paletta for this observation. He has referred to his Dissertation in several parts of his last work on the Nervous System, and of the last edition of the System of Anatomy; yet this *liberality* (as I presume we must term it) the reviewer abstains from mentioning. But so little impression did this near approach of Paletta to the discovery of the true functions of the roots of the fifth pair make on our countrymen, or assist the learned gentlemen, who contribute to the Edinburgh Journal, in advancing the subject, that I believe Sir Charles Bell himself was the first person in this country who called attention to it.

Before leaving this point, it is justice to Paletta to say, that there is a wide difference between Bellingeri and him as to their claims for confidence in them as physiologists. Paletta, having made the observation referred to, modestly declined expressing any opinion as to the probable functions of the large root of the fifth: he professed to be unable to give a satisfactory explanation of its uses: its anatomical structure, its ganglion, and its distribution to the muscles, as well as to the cavities of the head and integuments, puzzled him. Bellingeri, on the other hand, recklessly made a bold guess at what its functions might be; he failed in his conjecture: he pronounced it to be at once the nerve of motion and of sensation, when it is known to be simply a nerve of sensation: he has thus thrown a cloud of error over his only correct statement. We admire Paletta for his philosophical caution and forbearance, as well as for being the originator of the idea. We must decline placing confidence in any assertion of Bellingeri.

Let us now reflect on the amount of the proofs which the reviewer has brought forward, of the coincidence in the opinions of Sir Charles Bell and Bellingeri. The English physiologist maintains that a nerve consisting of a single root can possess only one of the two functions, sensation or motion; that it cannot have these incongruous properties combined. Does Bellingeri subscribe to this fundamental principle of the whole discoveries? In each of the two nerves, which are the subjects of his Dissertation, he contradicts this principle in the most decided manner. The large root of the fifth he represents as conferring both motion and sensation, and he affirms the same thing with regard to the portio dura! The only thing in common established between these two authors is simply this:—they both composed papers, in which the names portio dura and fifth pair occur! Fluellen, with "his figures and comparisons," could have devised a far better parallel!

And here I must be allowed to ask, why has the reviewer confined his remarks exclusively to the fifth pair and the seventh? Why has he refrained from mentioning the opinions that Bellingeri holds respecting the functions of the *spinal nerves?* Is he ignorant that his favourite author composed an elaborate dissertation, expressly dedicated to the subject of the spinal nerves? We find the title of the treatise placed at the head of the article as one of those to be analyzed! The date affixed to it shows that it was written two years sub-

* "I cut the nerve nearer the brain, and at a point previous to its having given off the branches to the muscles; then the jaw fell, and the muscles of that side were powerless I varied the experiment, by irritating the nerve, where it lies in the spheno-palatine fissure, immediately after an animal was killed: the jaws then came together with much force; indeed, so as to nip my assistant's fingers severely."—*Med. and Phys. Jour.* Oct. 1822.

sequent to the announcement of Sir Charles Bell's discoveries, and five years after the author's own first dissertation. It must, therefore, be supposed to present Bellingeri's matured opinions on the distinct functions of the nerves, and perhaps also certain improvements suggested by Sir Charles Bell's inquiries: at all events, from the resemblance between the spinal nerves and the fifth pair, we might have obtained a correct insight into his views concerning this nerve, by ascertaining what he said, at that comparatively late period, of the spinal nerves. But the reviewer abstains from noticing the dissertation of 1823; he places its title at the head of his article, and yet refuses to bring its contents to light! I can at once explain the reason of his reluctance to drag his admired foreigner into the broad day. In speaking of the spinal nerves, Bellingeri maintains that both the anterior roots and the posterior roots are endowed with motion! and the difference between them is—let the reader mark—that the anterior roots control the actions of *flexion and abduction**, while the posterior control the actions of *extension and adduction!* It would not have answered the purposes of the reviewer to have stated this. But has he honestly performed his duty in concealing it? Is such concealment doing justice to the subject of which he treats? Are his eulogiums on the foreign author bestowed to encourage the prosecution of just and correct views of physiology?

The reviewer, in concluding, says, with admirable taste, and in a manner that exhibits the "animus" with which the whole is written, "It must be rather mortifying to Sir Charles Bell to find that, being deprived by Bellingeri of any title to the discovery of the fifth pair and portio dura, *Magendie lays claim to his discovery of the different functions of the anterior and posterior roots of the spinal nerves.*" It must, indeed, be a matter of disappointment to every well-constituted mind, that the important inquiries into the nervous system should be disturbed by the introduction of such impertinent and irritating remarks; especially when they proceed from an anonymous writer like the reviewer, so wonderfully ignorant of the subject on which he treats. He does not even know what is the nature of Magendie's opinions concerning the roots of the spinal nerves—how diametrically they are *opposed* to those of Sir Charles Bell! The French physiologist does not lay claim to any discoveries of Sir Charles Bell. What Magendie claims, is the merit (if such it be) of adopting a view of the functions of the roots of the spinal nerves *directly hostile* to that maintained by our English physiologist. He makes no pretensions as to priority of engaging in the investigation of the functions of the spinal nerves, which the reviewer would lead us to suppose. This is a ground which he has long abandoned. It is not on a question of rival claims for the same discovery that these physiologists are divided: the point is, whether one opinion, originally set forth and still supported by Sir Charles Bell, or another which has been placed in opposition to it by M. Magendie, is the correct one. There is no harmony between them in their views. Whoever upholds the opinion of the one must desert that of the other.

What Sir Charles Bell's particular views concerning the roots of the spinal nerves are, I have already had occasion to state, while speaking of the roots of the fifth pair. His opinion is, that the posterior root, which has the ganglion formed upon it, is for sensation, and exercises no power at all over the muscles—that the anterior root, which has no ganglion on it, but resembles in this respect the ninth, &c., is for motion alone, and has nothing whatever to do with sensation. This account of their properties is in accordance with the principle to which I have had to refer—that no nerve in the body which has a single root is endowed with motion and sensation conjointly. But what does Magendie maintain? In controversion of this distinct announcement, he has insisted, (and he has not spared life in endeavouring to support his view

* Constituimus superius, filamenta radicum anteriorum, enascentia a fasciculis anterioribus, vel cerebralibus medullæ spinalis, atque filamenta radicum posteriorum, exorientia a fasciculis posterioribus vel cerebellosis medullæ spinalis, motibus voluntariis esse dicata. Hoc postremum crederem profecto, et quidem nervos a cerebro ipsiusque productionibus exorientes, flexionis, et abductionis motibus in genere famulari: nervos vero a cerebello, ejusque productionibus enascentes, generatim motibus extensionis et adductionis esse dicatos. Quem descriptum nervorum antagonismum locum obtinere, et humana et comparata anatomes, physiologia, atque pathologia suadere videntur."—*De Medulla Spinali, &c.*, auctore C. F. Bellingeri, 1823, p. 98.

by experiments), that the two roots do not respectively possess this simple character; he pretends that *both sensation and motion* reside in each of the two different roots. Whether he takes the anterior root without the ganglion, or the posterior root with the ganglion, he maintains that he finds it combines both these qualities in it; that it possesses the power of conveying sensations inwards to the sensorium, at the same time that it is engaged in issuing mandates of the will outwards to the muscles*. This is the singularity of opinion for which M. Magendie is distinguished†. Since, therefore, he maintains a view, the direct tendency of which is to subvert the truth of the principle which runs through the whole course of Sir Charles Bell's investigations, how can it be said that he claims that gentleman's discoveries? How can the reviewer excuse himself for the offensive insinuation contained in the last sentences of his paper—that the discovery appropriated by Sir Charles Bell might, perhaps, if the question were sifted, be found in justice to belong to Magendie?

Having shown how groundless the assertions are, which the reviewer has with so much boldness sent forth, and how devoid he is of all knowledge of the opinions of the gentlemen whose claims he pretends to adjudicate, it is not for me to hazard a conjecture whether such futile attempts to depreciate what he has done, or such palpable indications of a desire to hurt him in the eyes of the profession, will "mortify" Sir Charles Bell. The public—the intelligent public—especially the medical public, will not fail to look upon these exhibitions in their true light. In the history of the reception of his discoveries, they will recognize the usual fortune which accompanies signal improvements in science—first, the facts on which the discovery rests are denied—then some one seeks to appropriate the discovery to himself—and, finally, when there remains neither doubt as to the facts, nor hesitation, in all well-informed persons, as to the merit of the observations, it is said that the discovery was made long ago. Then it is that, with a pretended liberality, some expression is tortured to a new meaning, and some author whom the profession is accused of having long neglected is brought forward into notice. There are men who find it an inexpressible relief to praise those who are removed by distance or by death, if by this they can save themselves from the necessity of acknowledging merit nearer home.

I have the honour to be, sir,
Your obedient servant,
ALEXANDER SHAW.

July 7, 1834.

* "Ils semblent établir que le sentiment n'est pas exclusivement dans les racines posterieures, non plus que le mouvement dans les anterieures."—(Magendie; J. de Phys. Oct. 1822.)

"L'isolement des deux proprietés dans chacun des ordres de racines n'est pas donc absoln."—(Magendie et Desmoulins; An. de Syst. Nerv.)

† See a paper by M. Müller, of Bonn, in the Annales des Sciences Naturelles, where he has demonstrated the incorrectness of M. Magendie's statements, and confirmed those made originally by Sir Charles Bell. The experiments upon the roots of the spinal nerves have been made by this distinguished German physiologist with so much care, and such ingenious contrivances to avoid error, that no doubt can be entertained of their perfect accuracy. Yet his arguments against Magendie's views—of each of the two roots possessing double functions—would have been much stronger, if he had followed the example set by Sir Charles Bell at the beginning, of considering the fifth pair in the head as analogous to the spinal nerves, and taking the experiments on this nerve as confirmatory of those on the nerves of the spine. There is this advantage in experimenting on the fifth pair, that its branches are superficial, and can be easily reached with the bistoury, and the results admit of no question; whereas, by confining the experiments exclusively to the roots of the spinal nerves, considerable uncertainty is produced, in consequence of the exposure of the spinal marrow, the effusion of blood around the roots of the veins, the extensive incisions through tendinous and ligamentous parts, and the necessity of violently breaking open the spinal canal. It has been from relying altogether on the experiments upon the spinal nerves that M. Magendie has been led into error. His views, entertained at one time, of the fifth pair being the nerve of smelling, of vision, of hearing, and of taste, prove on what false ground he was pursuing his investigations.

WILLIS ON THE FUNCTIONS OF THE NERVES.

BY JOSEPH SWAN, ESQ.

ALL the honour of the discovery of the functions of the nerves has very recently been allowed to Italy and France; but I shall endeavour briefly to shew, that if the earliest cultivators of physiology, Erasistratus and Herophilus, and subsequently Galen, have no claim to this distinction, a very great share of it is due to our distinguished countryman, Willis, and his able coadjutor, Dr. Lower.

An experiment is related by Willis, in which the roots of the spinal nerves had been laid bare and irritated, in a living dog; and from the firm tenor of

the whole of his observations, it may be inferred that this was only one of many experiments made in support of a truth or principle that had been, in a great degree, previously matured by dissection and deep reflection; for, in his Anatomical and Physiological Treatises on the Brain and Nerves, he points out the functions of each nerve separately, but previously states that the division or distinction of the nerves, by reason of their various respects, is wont to be manifold; to wit, as they are either soft or hard, singular or numerous, in their beginnings; or that they serve either to the faculty of sense *(quod sensui tantum)*, or motion *(aut motui)*, or both together *(simul utrique facultati.)* He says, the first is for the sense of smelling; the second, for the sense of seeing; the third and fourth, for motion; the fifth, for both sense and motion; the soft portion of the seventh, for the sense of hearing; the hard portion, rather for motion than sense, and also for pathetic motion; the eighth, for the involuntary functions of the parts to which it is distributed; the spinal accessory, for the involuntary functions of the arms, &c.; the ninth, for the motions of the tongue, requisite for the articulation of sounds, as the nerves of the fifth, spread into this member, serve for the distinguishing of the tastes, for the exercise of either faculty, viz. both motive and sensitive; the texture of the tongue being notedly fibrous, hath the virtue of a double organ; therefore, by the two nerves, besides the supplements of spirits, are carried on both the instincts of motion and the sensible impressions.

"But, in truth, this may be rather said—that all the shoots of the nervous and lesser branches remain distinct and singular amongst themselves, from the parts to which they are inserted, even to their beginning; so that a peculiar tract of the spirits, or way of passage, lies open from the brain and its medullary appendix, to every muscle and nervous part; for, in truth, although the nerves, according to their beginnings, may seem to arise from the greater trunks, yet it will easily appear, if you shall open the trunk and these branches, that in them many little nerves, only like hairs, for the sake of a better conduct, are collected together in the same bundle; yea, the coverings being separated, you may follow oftentimes the little nervulets, and these single, to the respective parts and membranes to which they are destinated."

For the history of the more particular functions of every nerve, the reader is referred to the work itself.

"Oro miserere laborum
Tantorum, miserere animæ non digna ferentis."

If the writings of physicians of the past century be examined, it will be found that the preceding doctrines were received as fundamental; that they then became neglected, and have since been revived and enlarged upon by the labours of our own distinguished countrymen, as well as by no less distinguished foreigners.

Van Swieten, in his commentary on Boerhaave's 1057th aphorism, says—"It is known from physiology that some nerves serve for the sense of feeling, and others for motion, which, though they are very different in their origin within the brain, yet being collected into the greater trunks of the nerves, are sent together to the different parts of the body; wherefore the functions of the motory nerves may be hindered, while the nerves for feeling remain quite unhurt, or at least not so much, and the contrary."

Other passages bearing on this doctrine may be seen in the commentaries on several subsequent aphorisms.

6, Tavistock-Square,
July 14, 1834.

PHYSIOLOGY OF THE VOICE.

MODE OF PRODUCING CERTAIN MUSICAL TONES.

To the Editor of the Medical Gazette.

Sir,

In the last number of the Gazette, Mr. Bishop has advanced some ingenious reasoning to disprove the correctness of Mr. Wheatstone's explanation of Mr. Richmond's musical performance, an account of which I gave in a former number of your journal.

Mr. Bishop admits that the bass sound is laryngeal, but supposes that the production of the higher notes in the performance "approximates most nearly to the common mode of whistling." It is evident, however, that if this were

the case, the higher notes might be produced, as they are in whistling, without an accompanying bass or fundamental sound. Now the contrary is the fact. Mr. Richmond *cannot* produce the higher notes but in conjunction with the drone.

Mr. Bishop further states, that "the higher notes [in the performance] are not necessarily harmonies [it should be harmonics] resulting from the fundamental notes; their relative positions in the scale being under the control of Mr. Richmond's taste." This remark, if true, would certainly subvert Mr. Wheatstone's hypothesis; but there is reason to doubt its accuracy. As far as it could be determined, on the occasion on which Mr. Wheatstone and myself witnessed Mr. Richmond's performance, the series of higher notes invariably bore a definite relation to the fundamental sound; each higher sound being in the relation of a multiple of the bass; and not otherwise under the control of Mr. Richmond's taste, than are the higher notes, in Mr. Eulenstein's performance on the Jew's harp, under *his* control.—I am, sir,

Your obedient servant,

HERBERT MAYO.

19, George-Street, Hanover-Square,
July 16, 1834.

OXIDE OF BISMUTH IN PULMONARY COMPLAINTS.

To the Editor of the Medical Gazette.

SIR,

I HAVE for years been much struck with the intimate connexion which subsists between the stomach and the lungs, and with the quick interchange of disordered function which takes place between those two important organs. I need not point out the strong analogy of the affections of the mesenteric glands with the disturbed actions of the alimentary organs, of which they form so essential a part. From long experience of the good effects of the oxide of bismuth, under certain conditions of the stomach, and from the above opinion, that that organ is much concerned in most derangements of the lungs, I have been induced to order oxide of bismuth as a remedy for pulmonary complaints; and the success in arresting the progress, if not of ultimately relieving many symptoms, of those affections, I think sufficiently justify me in calling the attention of my medical brethren, who have more frequent opportunities than I now have, to a subject of this, usually considered, intractable nature, and warrant my taking the liberty of giving you the trouble of this communication.

I have the honour to be, sir,

Your obedient servant,

GEORGE SMITH GIBBES.

Weymouth, July 10, 1834.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Illustrations of the Natural History of Worcestershire, with Information on the Statistics, Zoology, and Geology, of the County. By CHARLES HASTINGS, M.D.

THIS is, we believe, the first publication that has issued from the press under the sanction of the Worcestershire Natural History Society. The society itself is of a very recent date, being established since the suggestion for the forming of such scientific co-operative bodies was thrown out at the first meeting of the Provincial Medical Association. We have already taken occasion to express our decided approval of the principle of such unions; and we are glad to find the author of the work before us dwelling with earnestness on the advantages to be derived from them by the medical profession; while he does not forget to assume that credit for his professional brethren which they so richly deserve as cultivators of the natural sciences. "With that profession," says Dr. Hastings, "the inquiries proposed to be instituted have a very close connexion, since without a correct knowledge of the natural history of the locality in which the physician practises, he must be ignorant of many things which influence its mortality, and cannot discern what Hippocrates esteemed the greatest excellence in the art of physic—the constitution of the seasons, and the diseases which attend them." And again, in his

introductory address, he says, "the volume of nature lies open to all, but in an especial manner to the medical philosopher: in all regions he is excited to peruse her pages, to interpret her language, and to explain her laws. In the expeditions, whether of discovery, war, or commerce, made to remote parts, and to every climate of this earth, we see with a pardonable satisfaction that the most interesting facts in natural history and science have been collected by medical persons, or by those educated in that profession. It seems to me, that a spirit of inquiry into the natural phenomena going on in this globe is congenial with the study of the medical art; and that this disposition may be encouraged and directed in a mode that may lead to important discoveries."

Dr. Hastings could not have brought forward a better example of his position than that afforded us in his own case. We have looked through his book, and are equally surprised and filled with admiration at the profusion of varied knowledge which it contains. Whether we examine it with reference to its statistical information, including the medical history of Worcestershire, or its notices of the zoology of the county, the botany, mineralogy, meteorology, &c., we are bound to declare that it forms one of the most interesting and valuable publications of the kind we have yet met with. We warmly recommend it as a model for kindred productions, which, we have no doubt, will emanate speedily from other new county societies.

The Transactions of the Medical and Surgical Association. Instituted 1832. Vol. II.

Among the papers which we have not yet noticed in our account of this excellent volume, there are two or three the substance of which we may be permitted to lay before our readers. The first of them contains—

Mr. Jennings's Experiments to distinguish the effects of artificial inflation from those of imperfect respiration, in the lungs of infants found dead.

It may probably be recollected that the author, who is surgeon to the Leamington Charitable Institution, contributed a paper to this journal in November last, in which he treated the present subject in a somewhat more compendious form. The experiments now published seem to have been cautiously conducted, are very interesting, and appear to justify the following conclusions:—

"1st.—That artificial inflation will dilate the lungs, so that they will float in water, and crepitate on pressure; and will, at the same time, change their colour from chocolate to bright scarlet.

"2nd.—That this inflation may be effected without the aid of instruments, by merely blowing air into the child's mouth.

"3rd.—Air introduced into the lungs, by artificial inflation, may be expelled by pressure, so that the lungs will sink in water.

"4th.—After respiration, the air cannot be expelled from the lungs, without completely breaking down the structure of every part of the organ. Any part, however small, not thus broken down, will continue to float.

"5th.—Before respiration, the arterial duct is of equal diameter throughout its course, considerably larger than either of the pulmonary branches, and nearly as large as the pulmonary trunk.

"6th.—After respiration has been established, the arterial duct becomes conical, the apex of the cone being towards the aorta; it is also considerably smaller in size than the pulmonary trunk; and one or both of the pulmonary branches will be found, in diameter, nearly equal to, if not exceeding it.

"7th.—If, immediately on opening the chest of an infant, the lungs be found of a bright scarlet colour, filling the cavity of the chest, covering the sides of the pericardium, and crepitating when cut or pressed; if they float freely in water, and portions continue to do so until the disorganization of every part be effected; there can be little doubt that respiration has taken place. If, in addition, the ductus arteriosus be conical, considerably smaller in size than the pulmonary trunk, and but little differing in size from the pulmonary branches, the fact of respiration having taken place may be considered as positively proved.

"Whatever objections may be made against either of the two tests, when employed separately, they are entirely removed when the result of both proves the same thing. Future experiments may prove, either that, where death has

taken place while respiration is very imperfect, or where the infant has been immature, one or both of these tests may fail to indicate the fact of respiration having existed. Still such cases can in no way affect the validity of the test, as proving respiration to have existed, where the results are such as I have pointed out. Future experiments may, and I trust will, disclose means by which the most imperfect respiration may be distinguished from artificial inflation; though, nevertheless, I am of opinion that, for practical application to the ends of justice, the above means will be found amply sufficient."

Dr. Ogier Ward's Observations on Cholera.

The observations given in this paper are founded on the personal experience of the author, engaged in the treatment of the disease, as it appeared in Wolverhampton and its neighbourhood. His remarks on the origin, causes, progress, characteristic symptoms, and the remedies found to be most useful, are judicious, and possessed of more of the interest of novelty than might be supposed after all that had been said on the subject for a year or two previously. The author's opinions are strongly affirmative of the contagious spread of the disease at Wolverhampton—the contagion, of course, being greatly and manifestly favoured by the ill-regulated habits and filth of those most exposed to it.

"A striking instance of the effects of bad food and dirty habits, was shewn in a family of eight persons. The father was in the habit of receiving from the butchers, offal and unsaleable meat, which was hung up in the house for the family to help themselves from till it was consumed, or too putrid to be eaten. The habits of the family were also dissolute and dirty, and, consequently, the house was a nuisance to the neighbours. The wife was the first taken ill, and she died; and it was with great difficulty her funeral was hastened, from the opposition of the husband, who swore most positively that she had not died of cholera, and should not be buried before the usual time; and by way of bravado, he went and lay down by the body. Two days afterwards he was seized with the disease, and taken to the hospital, and the family was dispersed about the town; nevertheless, they all had cholera in a few days, and only one, besides the father, survived."

The following paragraph, also, contains some curious matter for reflection.

"The general state of mind was very remarkable, for, however alarmed the patients might be before the attack, I never observed a person, suffering from cholera, in the slightest degree influenced by the fear of death, by grief at the idea of parting from their relatives and friends, or by the hope of recovery. On the contrary, they seemed in a state of perfect indifference on all these subjects, though their friends were weeping round their beds. Even the children did not cry, but were only restless and troublesome. The lighting up of the mental faculties, described by Sir H. Halford as occurring just before death in certain diseases, has been ascribed, by a late writer, to the intoxicating effect, as it were, of the carbonized blood stagnating in the cerebral capillaries. Again, Magendie and others have adduced the integrity of the mental powers to the last in cholera, as an anomaly, or, rather, as an argument against Bichat's opinion of the poisonous properties of venous blood. As far as my observation extends, however, I have remarked that, besides indifference, there has been, before death, more or less stupor, attended with deafness, and alternating, in some cases, with jactitation and muttering. The conclusion I draw is, that black or venous blood has not only no power to augment, or even maintain, the sensorial faculties, but that it has a direct effect to lessen and oppress them."

A table is given of the results of the various methods of treatment adopted in 356 cases: from which it appears that the deaths amounted to 106, and that the saline remedies were most efficacious,—while the cold-water system proved just the reverse. Croton oil the author considers as superior to every other medicine in the stage of collapse. By the way, "castor oil" is printed three times in page 380, instead of "croton oil," by some error of the press; but the context serves sufficiently to set it right.

Mr. Middlemore's Report of the Birmingham Eye Infirmary,

Contains several valuable cases, with remarks. On the curious subject of

dislocation of the lens, we find the following among other observations:—

"A blow upon the eye may displace the lens and its capsule, and, at the same time, rupture the hyaloid membrane; or it may inflict such injury upon these parts, that the septa of the hyaloid membrane may become absorbed, and the vitreous humour acquire a morbid fluidity, so that the lens will sink backwards and rest upon the retina, and, by its compressive effect, cause total amaurosis. Now, in such instances, an examination of the eye, with the head reclining backwards, will detect a vacillating iris, and a slightly green-coloured body, (which is the most common change of colour that the lens undergoes under such circumstances,) at the lower and posterior part of the globe; and, whilst in this position, the vision of such an eye will be entirely destroyed. If the patient move the eyeball freely and rapidly, the lens, or a greenish or amber-coloured body, will be seen to roll about in the eye. If the patient lean the head forward, the lens will rise into its proper place, or perhaps its upper margin may be seen, if the pupil is somewhat large, and he will be able to see in this position, not perfectly, but sufficiently well to find his way about, even in walking in a strange road, or in a place to which he has not been accustomed. Now it may happen, that, in some of these cases, the lens may not be wholly detached, a slight connexion still existing at its lower margin; and then, of course, it will not float about so freely in the vitreous humour, and will more readily come forwards if the head be inclined in such a direction; in other instances, the lens and its capsule appear to be wholly detached from the hyaloid membrane, and, of course, when this takes place, the crystalline will, in the erect position of the body, sink downwards, as an effect of its weight, and, by resting upon the retina induce complete amaurosis.

"I have seen many of these cases recently, and shall briefly refer to one or two of them; but, I must confess, I had never noticed this form of injury until my worthy and enlightened friend, Mr. Smith, of Southam, obligingly sent to me a man, in whom the lens passed freely into either chamber of the eye, in accordance with the position of the head; sometimes coming in front of the iris, and pressing that membrane (which was in a state of vaccillation) towards the retina, and on leaning the head backwards, it would pass the pupil and resume its proper position; so that, I apprehend, in this instance, the hyaloid membrane was entire. It may be desirable to state, that the lens preserved its transparency."

We must here close our analytical remarks on the contents of the volume. The work itself is well worthy the perusal of professional men, and we trust it will meet with that support and encouragement which it so well deserves.

MEDICAL GAZETTE.

Saturday, July 19, 1834.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

THE PRIVY COUNCIL AND THE GOWER-STREET CHARTER.

SEVERAL of our correspondents have expressed much curiosity to know what has become of the London *University* Charter, since the pleadings before the Privy Council? In answer to which we have to inform them, that nothing has been heard on the subject since the time alluded to. The parties asked for a charter, and have received no answer. But, so far is silence, in this case, from giving consent, that just the opposite inference is to be deduced. The applicants have not got a charter, nor are they likely ever to succeed in procuring one of the kind they seek, while they retain their present constitution. On the other hand, it is the assumption of a name to which they were not entitled — it is the impudence and dishonesty of trying to impose themselves as a *University*—which has been the sole obstacle to their obtaining an act of incorporation; for this would at once have been conceded to them, as it has

been to King's College, and many other scientific institutions. Either they must cease to be a joint-stock company, or they will never become a university in any thing but the name; nor do we believe that even then they would obtain the exclusive privileges which they so much covet. Yet we have no doubt that as the opening of another session draws near, and as the season for throwing out their baits and setting their springes for the unwary arrives, we shall once again witness a repetition of the announcement, that the *University* intends to confer degrees! "What," asked the Lord Chancellor of Mr. Bickersteth, during the late pleadings before the Privy Council,—"what is there to prevent the London University from conferring degrees now, and without a charter, if they please?"—"Nothing, my Lord," replied the learned barrister, "but the ridicule and contempt with which such degrees would be received." This undoubtedly, and this alone, it is which restrains them; this is the secret of their anxiety about a charter; because they know and feel that if they attempt to confer a degree of any kind—even their hybrid abortion of "Master in Medicine and Surgery"—that moment would the other schools also proceed, in self-defence, to grant diplomas; for, whatever right the establishment in Gower-Street has to do so, precisely the same right have all the other schools. Thus would the very name of a London diploma become a mockery and a scorn. Oh! (say the shareholders) but WE have the means of teaching the collateral sciences; and this cry has been repeated, till the public—that is, the unreflecting part of the public—imagine there is some reason in the argument. But what, we ask, may those collateral sciences consist of, a knowledge of which can be obtained in Gower-Street, but which might not be equally obtained at the other schools? Let but the collateral sciences be the passport to degrees, and all the great schools in London would very soon possess the means of teaching them. We presume that by "collateral sciences," as connected with medical degrees, we are to understand natural philosophy, natural history, &c. It might be sufficient, on this head, to appeal to the numbers who do—or rather to the numbers who do *not*—attend these classes in Gower-Street; but we prefer calling attention to another fact, and one which clearly proves that nothing which is made a pretext on which to found superior claims will long remain the exclusive property of any school;—we mean, that at St. Bartholomew's, the lectureships alluded to are already announced as having been added to their list. In short, the time has gone by for monopolies; and the attempt to establish a new one for the sale of degrees has signally failed. But again, as to the collateral sciences, which have been put forth so confidently, as constituting the superior claims of our friends in Gower-Street, what value do they themselves attach to them, as connected with the medical degree? What stress is laid upon them? What regulations have been made regarding them? What "collateral" courses are enjoined?—Much have the learned solicitors and eloquent advocates of the *University* dwelt upon this claim, as one which was to bear down all opposition; but the "Faculty" were wiser in their generation; they knew that if the extra courses and additional qualifications blazoned forth were really required, no one would ever dream of seeking *their* degree; they felt that being worth less, it must be had for less than the degree of a genuine University, else would all competition be in vain. Accordingly, we were very can-

didly informed by Dr. Grant, in his introductory leeture last year, that "*in this institution, the curriculum unanimously proposed by the medical Faculty for the diploma of Master in Medicine and Surgery, comprehends only subjects taught by the Professors of that Faculty.*" Now this is going to the point at once—this is doing business—this is being honest; here is no beating about the bush, no circumlocution, no coquetry, but a plain straight-forward announcement—"we are to give the degrees, and we shall give them only to those who attend our lectures; but if they do this, we shall require them to attend those of no other person, either here or elsewhere!"

We earnestly wish that the "Faculty," instead of always promising and postponing — instead of tantalizing the young aspirants with honours still receding as they attempt to approach them—instead of subjecting them to the sickness of hope deferred, would fairly dub a few—make a score or two of "Masters," and send them out into the world to try their fortune. Why should they hesitate? An individual has done what, in their collective strength, they seem afraid to attempt. The late Dr. Walker used to confer diplomas in vaccination! he granted degrees, and yet he had no charter! It is the easiest thing in the world to do so; as Hamlet says,

"'Tis as easy as lying."

Get a large piece of parchment — —the larger the better; have it well emblazoned with black letter, mixed with a little gilding or tinsel; set forth the morality, the ingenuousness, and the learning of the possessor; confer on him a name (and "Master," the one proposed, is sufficiently *imposing*); authorize him to kill or cure, over all the civilized globe—"*ubique gentium*," as our Edinburgh friends have it;—append to this a large seal and a tin case, and the DEGREE is complete. Now all this has been done, and can be done again, without going to the Privy Council;—besides, why ask permission to do that which can be done without the leave of any one? Therefore, we advise the Joint-stock Company in Gower-Street, either to give up their aspirations after exclusive privileges, and just to manufacture degrees on their own bottom, or let them at length be shamed into modesty, and take their place with other great schools, content to share with them the good and evil of their calling, without attempting to obtain exclusive privileges; for exclusion is contrary to the spirit of the age, and they may rest assured that whatever privileges they obtain will be equally given to all the other schools. We have been telling them this for these five years past, and have always been answered that they had "at last" succeeded. Another "at last" is added to the list, and they are no nearer the accomplishment of their wishes than before: nay, the very *Times* has turned against them, and, on a recent occasion, very truly remarked—

"*The non-existence of any thing like endowment, or characteristic of permanency, in the London University, and its necessary, though hitherto unsuccessful, adjustment* TO THE VULGAR OBJECTS OF OTHER JOINT-STOCK SPECULATIONS, *has been the great impediment to the grant of that charter which its directors have so eagerly desired.*" With the *Times* (their own especial journal), then, we say—that no joint-stock company ever has, or ever can, receive a charter conferring the power of giving degrees; and if ever there be a real London University, it will consist of some independent body, not concerned in teaching, and will extend its benefits equally to all the efficient establishments.

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CLINICAL OBSERVATIONS

ON THE GENERAL

TREATMENT OF FRACTURES OF THE EXTREMITIES.

By Baron Dupuytren.

From the "Leçons Orales," published periodically, under the Baron's inspection.

BANDAGES AND OTHER APPARATUS.

There are few positions in which the surgeon can be placed, that require of him a greater share of acquaintance with the details of his profession, than the treatment of fractures in general. It is by no means enough that he exercise a correct diagnosis, and employ those means which form the general basis of his management; he must further be familiar with the rules of conduct required for the different complications he has to deal with, and the several circumstances connected with each particular lesion. Nay, if he be really accomplished, he will not disdain to stoop to those minute particulars which some people may think of little importance, yet the neglect of which may lead to serious consequences. Thus the precautions to be taken in stripping a patient of his clothes, and in transferring him from one place to another, so as to avoid cruel sufferings, and the aggravation of injuries already done to the soft parts—the situation in which the patient should be placed—the form and degree of softness or hardness proper for his bed during the treatment—the manner in which the surgeon should proceed to dress his patient, and apply the necessary bandages—the means of ascertaining the consolidation of the callus—and the advice to be given to the patient at this stage of the treatment,—all these are points which experience shews must be attended to, and that with no inconsiderable share of diligence. In the observations which I have at present to offer, I shall call your attention, in the first place, to the kind of bandages which I usually employ in fractures of the extremities.

When the fracture to be treated has its seat in one of the thoracic extremities, and is not accompanied by any wound, the roller bandage is to be preferred. Some compresses are placed across the limb, where the fractured point is prominent, and over the splints, whether they be of metal, pasteboard, or wood. If it be the humerus which is fractured, the patient is seated on his bed; one, two, or three compresses are applied, over which some turns of the bandage are made; other compresses are then introduced, both above and below; after which splints are put on the four sides of the member, taking care that they shall not press on the osseous prominences of the joints. A general bandage is then applied over all.

In the case of the fracture of a fore-arm, the apparatus required are—a bandage four or five ells long, graduated compresses, two splints of the length of the fore-arm, or a little longer,—at all events broader.

The patient being placed in a sitting posture, or lying down, the four fingers of the hand are grasped by an assistant; another takes hold of the lower part of the arm; and the fore-arm being now reflected a little towards the humerus, extension is attempted. The surgeon, by means of well-directed pressure on the anterior and posterior aspect of the member, collects the muscular substance of the flexors and extensors into the inter-osseous space, to which he thus restores its natural dimensions—the fragments of the radius being thus separated from those of the ulna. In the next place, the fore-fingers and metacarpal bones are rolled in a bandage up to the wrist. This part is now committed to an assistant; and the graduated compresses, previously steeped in Goulard's lotion, are next applied to the dorsal and palmar aspects of the part, encroaching somewhat on the wrist and the tuberosities of the humerus. The two splints are applied above these, the bandage at the wrist is resumed from the hands of the assistant, and the roller is continued from the wrist to the elbow. The antero-posterior diameter is thus enlarged, and the interosseous space necessary for the movements of rotation, is thus preserved. If the fracture should be complicated with a wound, Scultetus's bandage, or some other on that principle, must be employed.

In fracture of the radius, I am in the habit of using, in addition to the ordinary apparel, a splint, which I call the cubital splint It consists of an iron plate, curved at its lower extremity, and in its concavity having several buttons. The upper extremity of this metallic plate is laid along the ulnar margin of the fore-arm; and a compress, folded several times, being placed between the inner side of the wrist and the convexity of the splint, the hand is drawn towards the metal, and made to grasp the radial margin of the first, by means of a handle formed with a compress, which is placed between the thumb and fore-finger, and the two ends of which, being furnished with two strings, are connected to the splint by means of one of the buttons which are upon it.

When the olecranon is fractured, I prefer the bandage which is used for the

union of wounds transversely, rather than that in common use; and as in extension of the limb, the upper fragment, or the olecranon portion, is the only part that has a tendency to be wrongly placed, I only put compresses above that process. I also employ the anterior splint; but the one I use is straight.

Scultetus's bandage is the one I almost exclusively use in fractures of the leg and thigh, where the roller cannot be kept adjusted, but is displaced every moment. It is composed of the following pieces: 1, several pads — 2, several folds of cloth —3, the ties—4, a streamer—5, a bandage with tails *(bandelettes)*—6, transverse compresses—7, longitudinal compresses—8, graduated compresses, for those cases in which there is some prominence of bone—9, a perforated piece of linen, with cerate on it, or charpie, or diachylon, in case there should be a wound—10, cushions stuffed with bran—11, immediate splints —12, mediate splints—13, a body roller—and, 14, a support for the thigh and for the foot. Let us examine each of these parts separately.

The pads or cushions are for receiving the fractured limb; and while they support it, they tend to prevent the inconveniences of discharges, which would otherwise perhaps saturate the mattress. The folds of cloth, frequently doubled, are laid on the cushions, in order that the latter may not be defiled with blood or pus: this precaution is above all things necessary in hospitals, where, if neglected, the foul cushions become nests of infection. Over the cloths come the ties, which are intended for keeping the several pieces in their proper position, and to incorporate the whole: there are three of them for the leg, and three for the thigh. The streamer ought to be equal in size to the length of the limb, and folded double; it is settled crossways over the ties, and destined to receive the splints on the extremities, and to support them. Above the streamer are placed the *bandelettes*, which constitute the eighteen-tail bandage, and they are either united or separate. If there be a wound, and pus or blood be discharged, they ought to be separate, so that they may be changed occasionally when soiled. If there be no wound, the tails need not be separate—even ten or twelve of them may be connected by overlapping: that is to say, the first should be half covered by the second, the second by the third, and so on. They are applied, beginning from the lowest, otherwise there would be many wrinkles and inequalities, which ought to be avoided. The tails should be long enough to go nearly twice round the limb. The transverse compresses are placed the lowest at the most elevated part; they must be merely a double fold, for if triple their application would be more difficult, and they ought to be as long as the *bandelettes*. If there be a wound present, the compresses ought to be changed without disturbing the rest of the apparel. It is in cases of this kind that we can appreciate the value of the longitudinal compress, which may be withdrawn when foul. Compresses of various forms, but generally square, and two, three, or four in number, are next applied to the limb; nor is it a practice to be neglected to put on graduated compresses along the length or breadth of the extremity. If it be the tibia, for example, which is fractured, and the fragments project outwards, the compresses are to be applied along the sides of the leg; but crossways, if the upper fragment projects in front.

It is also in cases of this description that we have need of the splints to which I have given the epithet *immediate;* for they act immediately on the fragments, from which they are separated by the graduated compresses alone. These splints are made of pasteboard or light wood,—some flexible material,—for otherwise they would injure the parts; and we must not forget, in applying them, always to interpose one or two graduated compresses between them and the fragments.

Every thing being thus settled so far, we avail ourselves of the transverse compresses, and then of the *bandelettes*, with which we envelop the limb, taking care always to direct them a little obliquely forwards, and so that they may partially cover each other. Next come the mediate splints, between which and the member we place the bran cushions; their length ought a little to exceed that of the limb; their breadth to be about five or six inches; and thickness, an inch and a half or two inches: they ought to be modelled according to the shape of the part, by diminishing their thickness where there is convexity of the limb; and, on the other hand, increasing it where it should correspond with a concavity.

The apparel is now to be combined into one piece by the ties which are connected to the external splint: the knots should be very simple, and placed on that side next the operator. A stirrup made of a long compress, or a leather sole with strings to it, and which may be attached to the sides of the streamer, will serve to support the foot in a steady position.

Lastly, some hoops should be put round the extremity, so as to protect it from the weight of the bed-clothes. Nor is this all. In order thoroughly to prevent the movement of the limb, a cloth, folded like a

cravat, is fastened to one side of the bed, then passed over the leg or thigh (whichever is fractured), and the other end tied at the other side. If it be a fracture of the thigh, there is moreover to be placed round the pelvis a body-roller, encompassing the haunches and the upper extremity of the outer splint. This last is a precaution of so much importance, that, in case of fracture at the upper part of the neck of the femur, the patient cannot get well without a curvature of the bone.

An apparel, arranged in the manner which we have now described, is of exceeding firmness, and not liable to disturbance, even should the patient be delirious.

I will still further add to these details, that the bed of the patient ought not to be very soft; it must be even,—not convex or concave; a firm pallet or mattress of horsehair, (furnished with laths), both for the comfort of the patient, and to secure his cleanliness. Lastly, there should be neither an elevation at the upper part, nor should the feet be raised.

Preliminary arrangements.—Having thus given you an account of the several parts of which the bandages we employ consist, and the mode of applying them, I shall now beg leave to call your attention to the care which the patient requires immediately after the accident has befallen him. If he be carried on a litter, you need be in no hurry to remove him from it: he ought to be stripped on it, while his bed is being prepared and the requisite apparel got ready. His boots and stockings ought to be slit up, and not drawn off; for every shock and painful dragging should be avoided. The limb should be washed, that it may not be necessary to spoil the bed afterwards with the process. All this being done, an assistant must take the patient round the body, another by the two extremities, while the operator, or some other careful person in his absence, must take charge of the fractured member. In this way the patient is carried to his bed, and deposited there: his pillow should have little or no elevation, so as to avoid slipping downwards in the bed; a very slight elevation, however, may be advisable, in order to support the head, and not to favour the occurrence of sanguineous congestions.

When putting on the bandages, the operator is placed at the outer side of the fractured limb; an assistant on the inner side is exclusively charged with the business of supplying each piece of the apparel in order. Another assistant holds the foot with his left hand in front and over the instep, and with his right holding the back part just above the heel. A third assistant, at the knee or haunch, according to the place of the fracture, holds the sides of the condyles of the femur or tibia, taking care not to press on the vessels or popliteal nerves; for if there be a wound, the pressure may occasion a hæmorrhage. After this, compresses steeped in goulard-water, or some other resolvent, are held by two of their angles by the operator, while the first assistant holds the the other two; they are then laid on the limb smoothly. If there be a wound, it is either dressed with diachylon, or a compress spread with cerate, and perforated; and over this charpie is laid. Lastly, the several pieces of the apparel are applied in the manner just now described.

If there be no wound, we should proceed on the following day to a fresh dressing, with the removal of the bandages; for it is not unusual to find, after four-and-twenty hours, that considerable swelling, or perhaps gangrene, has ensued. But this being done, it will be enough to visit the patient every fifth or sixth day, unless he feel pains, when it will be necessary to attend to him oftener.

Duration of Treatment. — With regard to the length of time during which the treatment may last, in general the apparel may be kept on for twenty-eight or thirty days in the case of children; forty days for adults, and much longer for aged people. It ought not to be removed until we have ascertained that the consolidation is complete. In order to be sure of this, the operator lays hold of the two fragments, and cautiously tries if he can produce any motion between them. If the callus yield, the apparel must be re-applied immediately; if it do not, the bandages need not be put on again, but they should be left in readiness by the side of the limb for three or four days. At this period, it will not be safe to allow the patient to walk immediately, for the callus may give way to the weight of the body or the action of the muscles: he must keep quiet in bed for ten or twelve, or fifteen days longer. He may then sit up in his bed, or in an arm-chair, with his foot laid on a pillow, and the whole limb wrapped in a roller for about three weeks. Crutches may now be given him, and they should be tipped with cloth, lest they should slip on the floor. The patient ought, if possible, to be kept in a chamber on the ground-floor, and he should avoid walking on uneven pavement—smooth sanded walks being much preferable.

I have thought it thus necessary, gentlemen, to dwell at length on these minute and apparently trifling details, because experience has taught me how very important they are for the practitioner, and, at the same time, how generally they are misunderstood, and frequently, also, how improperly they are practised.

NAPOLEON'S HEAD.

Dr. Antomarchi has prepared a cast of Napoleon's head, from a mould taken at St. Helena a few hours after the ex-emperor's death. It differs in several respects, and very essentially, from the aspect of the ordinary busts, more especially in being much less in size. Artists and poets have alike combined to impress the world with the idea of the mighty mind of the deceased having had a case of corresponding dimensions. Canova's bust of Napoleon—of which the face alone was taken from nature—gives a very exaggerated notion of the craniological developments; and it has not a little disappointed the phrenologists that these have all shrunk to the ordinary dimensions in the matter-of-fact cast made from the cranium itself by Antomarchi. The circumference is twenty inches, ten lines, French measure, which is by no means different from the heads of common men: neither is it remarkable in any other respect, unless that it strikes the eye as under the ordinary size. What, then, becomes of Victor Hugo's apostrophe—

"Ce front prodigieux, ce crane fait au moule
Du globe imperiale" ———

We have no doubt, however, that the poetical busts of Canova and Chaudet will be preferred to the less striking reality; and yet we remember very well a print of General Buonaparte, in which the head is by no means so large as that which afterwards wore the diadem of the Emperor Napoleon.

TRANSPOSITION of the HEART.

Andrew Demlevic, aged 53, is at present attending Dr. Duncan. with chronic bronchitis and asthma, but without any signs of effusion into the chest. *The heart beats on the right side*; the chief impulse being felt between the fourth and fifth ribs, immediately below the nipple. The pulse is regular, and of about equal strength, in both arms; the impulse on the right side of the chest is natural, and none is felt on the left side. He was for thirteen years in the army; served in the Peninsula and Canada, and has always enjoyed good health till lately. He does not remember ever to have felt pulsation on the left side. —*Liverpool Medical Journal.*

ROYAL COLLEGE OF SURGEONS.

At the recent annual election, Mr. White was appointed President, and Mr. Andrews Vice-President, for the year ensuing.

We perceive, by a late *Gazette*, that the honour of a baronetcy has been conferred on Stephen Love Hammick, Esq. surgeon, of Cavendish-Square.

NEW MEDICAL WORKS.

An Investigation into the remarkable Medicinal Effects resulting from the External Application of Veratria. By Alex. Turnbull. 2d edit. with an Appendix, on the External Application of Delphinia, and on the Internal Administration of the Salts of Veratria and Delphinia. 8vo. 5s. bds.

IN THE PRESS.

The Dublin Practice of Midwifery. By Henry Maunsell, M.D. 1 vol. 12mo.

WEEKLY ACCOUNT of BURIALS,

From Bills of Mortality, *July* 15, 1834.

Abscess	4	Hæmorrhage	2
Age and Debility	20	Heart, diseased	1
Apoplexy	9	Hernia	1
Asthma	4	Hooping-Cough	13
Cancer	1	Inflammation	26
Childbirth	5	Bowels & Stomach	9
Cholera	1	Brain	4
Consumption	63	Lungs and Pleura	5
Constipation of the Bowels	1	Insanity	8
Convulsions	35	Jaundice	1
Croup	4	Liver, diseased	4
Dentition or Teething	11	Measles	7
Diarrhœa	1	Mortification	6
Dropsy	13	Paralysis	3
Dropsy on the Brain	7	Rheumatism	1
Erysipelas	2	Small-Pox	4
Fever	14	Spasms	2
Fever, Intermittent, or Ague	3	Stone and Gravel	1
Fever, Scarlet	5	Thrush	3
Gout	1	Venereal	1
		Stillborn	23

Increase of Burials, as compared with the preceding week . . . 52

METEOROLOGICAL JOURNAL.

July, 1834.	Thermometer.	Barometer.
Thursday . 3	from 53 to 79	30·00 to 30 02
Friday . . 4	54 81	29 99 Stat.
Saturday . 5	51 82	29·87 29 93
Sunday . . 6	49 70	29·21 29·92
Monday . . 7	51 78	29 89 29·87
Tuesday . . 8	54 74	29·89 29 96
Wednesday 9	49 69	30·01 30·03

Wind variable, S.W. prevailing.

Except the 4th, generally cloudy, rain at times; thunder frequent during the 6th, with heavy showers.

Rain fallen, ·575 of an inch.

Thursday . 10	from 51 to 73	29·93 to 29·93
Friday. . . 11	56 79	29·91 29 99
Saturday . 12	57 81	29·98 29·90
Sunday . . 13	50 72	29·85 29·88
Monday . . 14	53 76	29·90 29 97
Tuesday . 15	50 81	30·05 30·15
Wednesday 16	53 85	30·18 Stat.

Prevailing wind, S.W.

The 12th and 13th cloudy; a few drops of rain in the evening of the 13th; otherwise generally clear.

Erratum.—Appended to the Birmingham Protest, at p. 480, the name of Dr. Male is erroneously printed Hale.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JULY 26, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XLII.

PRETERNATURAL LABOUR.

SHOULDER PRESENTATIONS.

Turning.—We have made ourselves acquainted with the history of transverse presentations; we have learned how they would terminate if left solely and entirely to nature; we have laid it down as a principle that the interference of art is necessary; and we have also ascertained the most favourable time for the performance of the operation of turning.

Advantages in employing the left hand in turning.—I have recommended that the operation should be performed with the left hand in preference to the right; and this recommendation is grounded upon, what I consider, four very valid reasons. *First*, when the tips of the fingers are brought nearly together, so that the whole organ resembles a cone, or forms somewhat of a wedge, the woman lying on her left side, the left hand enters the vagina more easily than the right, passing upwards, in the direction of the axis of the external parts. *Secondly*, when it is lodged within the vagina, fully occupying the pelvis, the knuckles of the left hand adapt themselves completely to the cavity of the sacrum, and the hand itself is carried up to the brim, in the direction of the axis of the brim, following the curve of the canal. *Thirdly*, when it is passing the brim, while dilating the mouth of the uterus, and entering its cavity, it then again takes the direction of the axis of the uterus, which you are aware is not perpendicular to the horizon when the woman is in the upright posture, but placed so that the fundus is thrown somewhat forward, and the os uteri back, looking towards the centre of the sacral cavity. *Fourthly*, when the left hand is in the uterus, an opportunity is afforded of steadying the uterine tumor externally by the right, carried between the woman's thighs, and placed on the abdomen. The consent between the two hands affords an infinitely greater facility in action than could be attained by the aid of any assistant. An act of volition is all that is required, in order to a co-operation of the two hands: the mind begets the thought, and at the same moment the limb performs the deed required; but if we have to give directions to a party standing by, time is lost, the progress of our proceedings is interrupted, and the probability is that our orders will be obeyed in an imperfect and faulty manner. If you take the position that I recommend, and attempt to pass the right hand into the uterus, you find the knuckles opposed to the under surface of the symphysis pubis, the tips of the fingers rub against the cavity of the sacrum, and the wrist must be bent far backwards, before the hand can enter the uterine cavity. To introduce the right hand fully into that cavity, we must place ourselves behind the patient, which is an awkward posture, and not always to be accomplished. Many practitioners who have observed me use my left hand in preference to my right, have remarked that they could not perform any operation with the left, having such little command over it in all acts requiring nicety of manipulation. You must be aware that this command over the two hands is almost entirely the effect of habit; and I would recommend you in all minute operations, both of surgery and midwifery, to accustom yourselves to use each indiscriminately. For myself, I confess that I am more dexterous with my left hand—to use

an anachronism—than with my right, in all the operations connected with midwifery, though not in the ordinary affairs of life; and this superiority is entirely produced by custom. I became, from early instruction, prejudiced in favour of using the left; my own reasoning on the subject has confirmed that prejudice; and habit has now rendered my left hand more useful to me than the right.

So far with regard to the operation of turning previously to the rupture of the membranes. If the external parts be relaxed—if the vagina be distensible—if the os uteri be pretty well open—it is an operation of comparatively little difficulty; but the case is widely different when the liquor amnii has drained off, and the uterus is closely contracted round the body of the child.

Turning, when the uterus is contracted around the child.—We will next give our attention to the operation of turning under a contracted uterus. It will not happen to any of you, I hope, to meet with a very aggravated case of this kind in your own practice; because I trust you will be able to discriminate the position of the child previously to the rupture of the membranes; and that you will seize for the performance of the operation the favourable opportunity which I have already sufficiently dwelt on. But it is not improbable, especially in the country, that you may be called to the assistance of a midwife—or, it may be, a brother practitioner—who, biassed by the feelings almost universally prevalent till within the last two or three years, and excusing his ignorance, perhaps, on the too-often repeated quotation, "Thank God, I know nothing of midwifery"—has considered this subject of secondary importance in his studies, and has failed to make himself acquainted even with its elementary principles. Under these circumstances, the membranes having been ruptured some hours, the child's body being strongly embraced by the uterine parietes, the question naturally arises whether we shall leave the case, in the hope and expectation that *spontaneous evolution* will take place, or rather that the child will pass double; or whether we shall attempt to relieve the unfortunate situation of the fœtus, under which we believe nature to be incompetent to perfect her object. As it would be unsafe to trust the delivery to nature, we must accomplish it artificially; and we may either proceed to its completion immediately, or prepare the patient previously, by attempting to lessen the powerful action of the uterus. On this question the opinion of practical men is divided—some advising us not to delay a minute, others declaring that we should run the risk of injuring the uterus if we attempted to pass the hand during its contracted state; and that therefore we ought to stay our endeavours until we have obtained that truce which is so desirable, and, indeed, necessary:—and the instructions of each party are perhaps applicable to certain peculiar cases, and therefore occasionally to be followed. But I am myself an advocate for the operation being performed as early as is compatible with the woman's safety; and I have almost invariably found that if due caution be observed, it can be accomplished without the necessity of placing the patient under that severe and rigid system of treatment deemed requisite by some.

The advocates for the system of delay in those cases where the fœtal body is strongly compressed by the powerfully-contracted uterus (and I particularly allude to one very eminent obstetrical authority in this town), instruct us to abstract twenty or thirty ounces of blood, so as to occasion syncope; and to take advantage of the state of relaxation thus induced for the fulfilment of our intention. If, notwithstanding the supervention of faintness, we cannot succeed in introducing the hand, to employ that agent which is supposed to rank second in effect in relaxing uterine contractions, softening rigid fibre, and overcoming clonic spasm—opium: we are taught to exhibit 80 or 100 drops of laudanum immediately; and, waiting fifteen or twenty minutes for the operation of the drug, to make another attempt. If, after the lapse of this time, we are still unable to insert the hand, we are recommended to exhibit twenty or thirty drops more every quarter of an hour, till we find that relaxation has taken place. The use of poppy fomentations to the vulva is advised; and it is also suggested, that in hospital cases we might try the relaxing power of the warm bath. The tobacco enema is spoken of as being one of the most likely means to produce relaxation; although dreaded as a highly dangerous remedy. Failing, however to effect our object by these various means, it is then recommended that we should wait till the uterus is worn out by its own powerful contractions, and that we should take advantage of the quietude induced by exhaustion.

Notwithstanding the high names by which these means come recommended to our notice, from most of them I entirely dissent, and think it my duty strongly to deprecate their use; because it is not so much the immediate delivery of the patient that we have in view, as her ultimate safety. If, then, by such means as bleeding, opium, tobacco, or any other depressing or narcotic agent, we bring the system into such a torpid state as

entirely to remove uterine contraction, we deprive the woman of that very power which is to place her in safety after her delivery; and we prevent the closure of the uterine vessels, a patulous state of which must lead to fearful hæmorrhage. I contend that if we deliver the patient either while under syncope from bleeding, or stupefaction from opium or tobacco, we should be emptying the uterus at a time when it could not exert its contractile energies; we should consequently leave it in a flaccid state, and bring the patient into the greatest peril. Besides, I very much doubt the power of these means in ensuring the end proposed; for it is not only the occasional action of the uterus which prevents the introduction of the hand, but the permanent contraction of its fibres, which is induced by, and consequent upon, that occasional action. When the difficulty merely arises from the violence of the labour pains, we may gradually insinuate the hand during the interval of action; but when a permanent decrease in the capacity of the uterine cavity has taken place, through a continuance of that occasional or intermitting action constituting the throes of parturition, a state of tonic contraction is induced, which is constant and unyielding, and which it would be fruitless to endeavour to remove either by bleeding, opiates, or any other antispasmodic power. Of all the means spoken of, bleeding and fomentation are perhaps the only ones which I would be inclined to employ; and that not with the view of taking off uterine contraction, but of subduing inflammation of the structures consequent on pressure. Bleeding and fomentations can only, I conceive, be requisite where violence has been used by a previous attendant; for I trust that, in your hands, no woman in labour will ever be exposed to such rude treatment as would originate inflammatory disease.

Should, then, this reasoning be correct, and should the means so confidently recommended prove of little or no avail, they must be injurious in the same proportion as they depress the system, and it would be unwise to rely upon, or indeed to adopt them; especially as other modes of delivery are placed within our reach,—when judiciously-directed efforts at turning fail,—without subjecting the patient to such additional causes of danger.

The observations I have just ventured to lay before you are not founded on speculative reasoning, but are the result of somewhat extended practice. During the last eleven years, I have delivered more than 120 women under transverse presentations, independently of a few cases to which I have been summoned where spontaneous evolution occurred. Many of these cases presented a formidable appearance; for in one, the membranes had been ruptured a whole week; in another, sixty-nine hours; in a third, fifty-eight hours; in another, fifty-five; in another, fifty-three; and in many, more than forty-eight; and as a general principle, we presume that the longer the liquor amnii has been evacuated, the more likely is the uterus to have embraced the fœtal body firmly, and the more difficulty will there be in overcoming the resistance. In none of these cases did I exhibit large doses of opium, and in those few where bleeding was practised, that operation was had recourse to, not for the purpose of relaxing the rigidity of the uterine fibres, but to relieve the inflammation which the soft structures were suffering, and to remove tension. In not one of these instances was any injury inflicted on the uterine structure; nor did any permanent evil arise as a consequence of the operation. In four cases only was the uterus so powerfully contracted as to refuse admittance to the hand, and compel me to adopt the alternative of exviscerating or decapitating the fœtus.

Feeling, then, so strongly on this subject, I think it my duty to advise you, in all cases where the membranes have been broken for some time, to proceed at once to the delivery, provided the vagina and vulva be relaxed, and not swollen from inflammation, and provided the os uteri also be fully open: and if your endeavours, judiciously directed and steadily persevered in, be frustrated, to pause, and consider carefully and dispassionately every circumstance connected with the case; that you may inform yourselves of the particular cause of the extraordinary difficulty you experience. I would only again urgently caution you against using undue exertion—against endeavouring to overcome the resistance by force or sudden jerks, rather than by a continuance of gentle means—and against forming a resolution to "deliver" at all hazards; and I am persuaded, if the recommendations immediately to be submitted to you be followed, you will be able to effect the requisite change in the position of the fœtus, and avert the necessity of having recourse to the means already brought before your notice—provided, indeed, "turning" can be accomplished at all.

Mode of performing the operation. — Although, when speaking of turning the child while the uterus was still distended with the waters of the ovum, I stated it as my opinion that it was not absolutely necessary to acquaint ourselves positively with the peculiar position of the fœtus—both because when the hand is introduced into the uterine cavity it can be carried easily along the body of the child until

one or both feet be felt, and because of the difficulty of detecting the exact mode in which it lies previously to the rupture of the membranes—the case is very different when the uterus is strongly contracted, and it then behoves us to ascertain most distinctly its situation before we attempt to remedy it: and this information it is not difficult to gain, by observing which hand protrudes, and attending to the direction of the palm. We know the right hand by the thumb being opposed to our own when we place the palms together; and we know that the palm of the child's hand must be looking in the same direction as the abdomen, unless the arm be twisted. Thus, then, if the right hand be external, with the palm directed anteriorly, the head must be lying on the right ilium, and the face must be looking forwards: if the right hand be down, with the palm towards the anus, the head must be placed on the left ilium, and the face turned towards the spine: if the left hand be protruded, with the palm forwards, the head must be on the left ilium, and the face looking to the abdominal muscles; and if the left palm be directed backwards, the head must be on the right ilium, with the face towards the spine. Having learned, then, on which side the head lies, we know that on the opposite we shall find the breech; and having ascertained whether the face is situated anteriorly or posteriorly, we know also that towards the same part of the uterus we shall encounter the legs; and thus, so far as guiding our hand immediately to the child's feet is concerned, our operation is much simplified.

But it is possible that neither of the fœtal hands may be protruded externally, although the membranes may have been ruptured for some hours, and the shoulder may be fully occupying the pelvic brim;—the elbow may be situated in the vagina doubled. Under this state, it might be difficult to distinguish the right from the left extremity, by making our observations on the arm itself alone; and it is much better quietly and tenderly to unbend the limb, so as to bring the hand down, than commence the operation in ignorance of such an important point.

You may, perhaps, be inclined to ask, why should we bring down the arm, and impede the subsequent steps of the operation by filling up the pelvis with the limb? I would answer, that it is not the arm which prevents us making the evolution; it is the shoulder and the body of the child blocking up the pelvic brim, together with the strength of the uterine contraction: it is, indeed, of little consequence whether the hand is external, or whether the arm is doubled, the elbow presenting in the vagina. If we have any doubt as to whether the presenting limb be the right or the left arm, by all means let us gently bring it fully down.

Some practitioners advise us, that if the palm be looking towards the mons veneris—inasmuch as the feet must then be placed anteriorly in the uterus—the right hand will be more conveniently used than the left; because that hand, traversing the forepart of the uterine cavity, passes up at once to the very spot where the feet lie. If the right hand, indeed, adapted itself as well to the axis of the external parts, to the curve of the sacrum, and to the axis of the uterus, as the left does, it would, no doubt, be much preferable: but as this is not the case, and as, by simply directing the left forwards, through rotation of the wrist, upon its admission within the cavity, we can, without much difficulty, reach the feet, I am, even in this position, inclined to recommend its employment; and in my own practice, I have already said that I almost invariably use it.

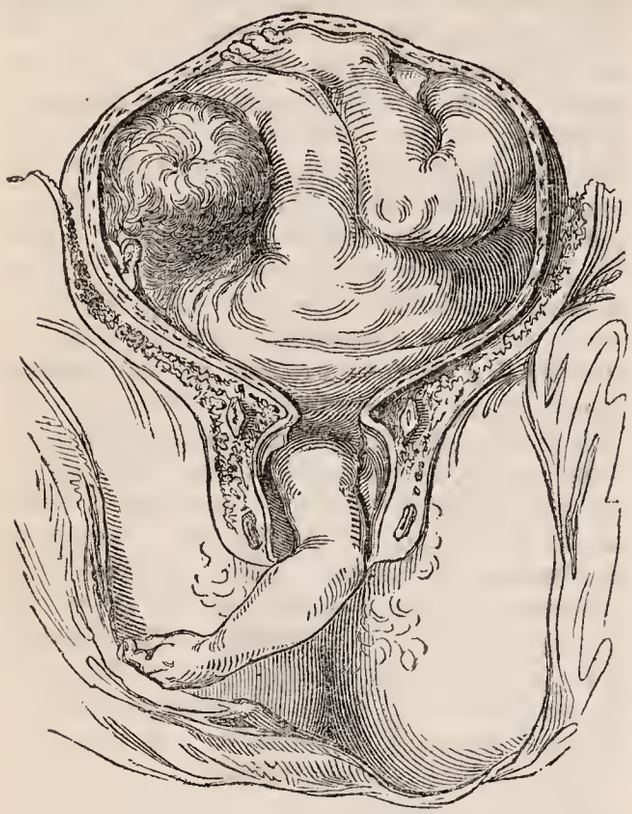
We will suppose, then, we are consulted in a case of this serious difficulty, where the membranes have ruptured some hours, the hand protruded externally, swollen, and somewhat livid; the shoulder and chest occupying the brim of the pelvis, and the uterus contracted powerfully around the child's body. Having ascertained the exact position of the fœtus, by the direction of the different parts of the hand, before proceeding to turn it would be right to make an accurate examination of the abdomen externally; by which we may learn the degree of contraction that the uterus has taken upon itself, and form some opinion of the probable resistance we are likely to encounter. We also make ourselves acquainted with the general magnitude of the ogran, and may judge whether the woman has advanced to near the close of gestation. It is very possible that she may have gone into labour prematurely; and if, on placing the hand on the uterine tumor, we find it so small that it has sunk considerably within the pelvis, we should relinquish the idea of an operation for two reasons; first, because we should be foiled in endeavouring to introduce the hand into so small a space as the uterine cavity possesses; and, secondly, because in all probability the fœtus will pass doubled. Should the patient unhesitatingly inform us that she is persuaded she has not exceeded seven months, we may then generally trust the case to nature; but, beyond that period, an operation will mostly become requisite.

The position and general management of the patient must be such as I have before described. Having taken off our coat,

turned up our shirt sleeve, and anointed the left hand and arm as far as the elbow, avoiding the inside of the fingers and palm, we must kneel by the bedside, and, forming our fingers into the shape of a cone, we must insinuate them cautiously through the external parts, up to the os uteri; then, laying the hand flat upon the child's person, we endeavour to introduce it either anteriorly or posteriorly, according as the feet lie, sliding it upwards along the fœtal body. It may sometimes avail us, in this step of the operation, to raise the shoulder somewhat from its position; but this is generally difficult to accomplish, and not without its dangers. If the uterus be contracted powerfully, and our attempts are made without great care, we may thrust either our own hand or the child's body through the uterine structure, to the almost inevitable destruction of the patient. It is more than probable that the stimulus of our hand may occasion an accession of uterine action, which we are made sensible of both by the complaints of the woman, and the propulsion of the child's body downwards: we must then, for the present, desist from further endeavours, (keeping our hand flatly spread out on the child's body, lest the irregularities of the knuckles might injure the uterus), and resume them in the interval of action. In this way, by little and little—making progress slowly, but steadily, only pressing forward in the absence of pain, laying the hand extended on the child's person during the return of action, preserving all the advantage we have gained, and taking especial care not to withdraw it,—we carry it fully within the uterine cavity to that part in which the feet are placed: grasping them both, if they lie together, or one only, if there is any difficulty in finding the other, we cautiously draw the limb down through the os uteri into the vagina. If the fœtal body be closely embraced by the contracted uterus, and especially if the shoulder be at all wedged in the brim of the pelvis, we shall find that, although the foot descends into the vaginal cavity, still the shoulder does not recede, and the child does not perform the necessary evolution to allow the breech to pass down so as to occupy the pelvis; and the greater exertion we make in drawing down the foot, the more firmly does the shoulder become impacted in the pelvic brim. It is evident, then, unless we can raise the shoulder, that we shall not procure a space into which the breech can descend; and if we endeavour to push the upper part of the child's body out of the way, while we are using no extractive power to bring the breech down, we shall not only not succeed in our endeavours, but the foot will escape back again into the upper part of the uterine cavity. Now, as the vagina is not sufficiently capacious to admit both hands at the same time,—with one of which we might raise the shoulder, and with the other draw down the breech, by means of the leg,—it will much assist us to get a noose of strong tape fixed round the ankle, while still in the vaginal cavity; nor shall we find much difficulty in the adjustment; and when applied, traction downwards, in a line tending towards the coccyx, may be made by it, while a steady pressure upwards is exerted by the extremities of the fingers placed against the axilla or the ribs. By this double effort the shoulder can be raised,—although that was impossible before the leg was brought down,—because room is made for its recession by the descent of the breech, while at the same time a space is formed for the reception of the breech by the ascent of the shoulder. The breech, then, having been brought down, the case is made one of the first order of preternatural cases, and must be managed by the rules before propounded.

I freely confess that sometimes it is most difficult, if not impossible, to introduce the hand so high into the uterus as to arrive at the feet, because they may lie at its very fundus, and it may be dangerous to attempt to overcome the strength of the contraction which the uterus has taken on itself. It has happened to me, in working the hand along the body of the child, to be able to pass it up to the breech, to feel the legs doubled up, but not to be able to reach a foot. In such a case I have not thought it right to make a strenuous effort in order to encompass the feet, but have satisfied myself with hooking a finger in the ham, and making the child revolve by the power that purchase afforded. I must not, however, omit to warn you that this practice is far from being unattended with danger; because you may mistake an arm for a leg,—the bend of the elbow for that of a knee; and I would scarcely recommend your having recourse to it, until your experience enables you with certainty to discriminate the one limb from the other. In one instance, indeed, when the irregular action of the fibres of the fundus prevented my arriving at the foot, after having passed my finger round a ham, not possessing sufficient power to cause the child to revolve, I directed a small blunt hook by the side of my finger, and keeping the point well guarded, made traction by it, and accomplished my purpose; but you must be well aware that such a proceeding must be attended with some risk. In this plate of Smellie, you have a good representation of the uterus strongly contracted

round the fœtal body, under a transverse presentation.



Spontaneous evolution.—It was a remark first prominently set forth by Denman, that occasionally, under a shoulder presentation, after the membranes had been broken some time, the uterus acting with considerable energy, the body was forced down into the pelvis, and a natural termination of the case occurred, the breech being expelled first. To this peculiar change of position, effected by nature, he gave the term "spontaneous evolution;" and he has supplied us with his idea of the mode in which it occurs in the following passage:—"I presume that, after the long-continued action of the uterus, the body of the child is brought into such a compacted state as to receive the full force of every returning action. The body, in its doubled state, being too large to pass through the pelvis, and the uterus pressing upon its inferior extremities, which are the only parts capable of being moved, they are forced gradually lower, making room, as they are pressed down, for the reception of some other part into the cavity of the uterus, which they have evacuated, till the body, turning, as it were, upon its own axis, the breech of the child is expelled, as in an original presentation of that part." He afterwards says,—"Premature or very small children have often been expelled in a doubled state, when the pelvis was well formed, or rather more capacious than ordinary; but this is a different case to that which we are now describing." The ideas that Dr. Denman had formed of this unusual occurrence were generally received by the profession as the true explanation, until Dr. Douglas, of Dublin, in a pamphlet first published in 1811, showed clearly that the description was incorrect. He observes, "that it is incompatible with the received ideas of uterine action to suppose that the uterus, when contracting so powerfully as to force down that part of the child which was at its fundus, would at the same moment form a vacuum, into which another portion, already low down in the pelvis, should recede." Dr. Douglas, then, first gave us a true history of the process; and he has proved that the fœtus actually does pass the pelvis in a doubled state, although denied by Denman. He has described it, as indeed I myself have witnessed, to be accomplished in the following manner:—By the continuance of the powerful uterine contractions, the arm is protruded entirely externally, the shoulder and chest being propelled low into the pelvic cavity. The acromion then appears under the symphysis pubis; and as the loins and breech descend into the pelvis at one side, the apex of the shoulder is directed upwards towards the mons veneris. Further room is thus gained for the complete reception of the breech into the cavity of the sacrum, and that part of the child's body is eventually expelled sweeping the sacrum, and distending the perineum to a vast extent. As, during the whole of this process, the head remains above the pelvic brim, it is evident that, the apex of the shoulder being external, the clavicle must be strongly pressed against the under surface of the symphysis pubis: on which point, indeed, the fœtal body partially revolves, as on an axis; the other shoulder and arm, and the head, being expelled last.

We cannot reasonably expect this doubled expulsion to occur, unless the patient possess a larger pelvis than ordinary, or unless the fœtus be preternaturally small or premature; nor, indeed, except under a long continuance of powerful and expulsive pains. But the knowledge of the fact, however rare its occurrence, must be considered of much importance practically; for if we saw good reason to believe that delivery was likely to be effected in the manner just detailed, we might be inclined to leave the case to the unaided efforts of nature, and hope for a fortunate termination.

I have personally known seven cases of this description, in all which my assistance was desired; and I was present at four of them during the expulsion of the fœtus through the outlet of the pelvis. Three of the children were born alive; two of them were twins: all the rest, except one (which was at full time), were prema-

ture, being expelled between the sixth and seventh month.

But another practical advantage has been also gained by observing the phenomena that I have mentioned—viz. the institution of an operation in which the process is imitated, when it has been found impossible to deliver by the more ordinary method of turning; and this consists in diminishing the bulk of the fœtal body by the removal of the viscera; giving it thus an opportunity to collapse, so that it may be extracted without much difficulty.

Evisceration.—You must not imagine that the operation I am about to describe is intended to supersede the practice of turning under transverse presentations. It is only to be had recourse to as a last resource, when many hours have elapsed since the rupture of the membranes—when the fœtus is so firmly wedged within the pelvis, or at the brim, that the introduction of the hand into the uterus is rendered impossible, or would be evidently attended with most imminent danger. In modern times this operation was first recommended by Dr. Douglas, and in this city first performed by my father and myself. In itself it is not difficult of performance, and requires merely the use of the same instruments employed for perforating and extracting the head. The woman lying on her left side, an assistant should be directed to bring the chest as fully into the pelvis, by traction at the arm, as possible; the perforating scissors, guided by two fingers of the left hand, should be carried against one of the intercostal spaces, and a free opening made. One or more ribs might be divided, if necessary, so that two or three fingers, or the whole hand, might be introduced within the aperture.—Through this incision the contents of the fœtal thorax must be extracted; the diaphragm may be perforated afterwards, and by the same opening the liver and intestines evacuated. The body, thus deprived of the principal part of its contents, will collapse; and if the uterus continues to act with vigour, will be expelled doubled, the breech sweeping the sacrum and perineum. But should the pains have ceased, artificial extraction may be most beneficially made by means of the crotchet fixed within the fœtal ilium; the breech will soon be observed to descend, and the case will be terminated as though nature had expelled the child unaided.

Decapitation.—Another means of delivery under transverse presentations, when turning is impracticable, is afforded by the division of the cervical vertebræ, and the separation of the head from the trunk. This operation I have also had recourse to, and found the difficulty by no means great. The best instrument for its performance is a hook with an internal cutting edge, formed by my father, a specimen of which I now shew you; and the following is the best method of using it.

The finger having been passed around the neck, a large-sized blunt hook must be introduced upon it, and the presenting part must be brought as low into the pelvis as is consistent with the woman's safety. By the side of the blunt hook the decapitator must also be directed over the neck, and a sawing motion given to it by the right hand, while the first finger is kept steadily in contact with its blunt point. It will soon be found that the structures give way, and that the separation is effected. The child's body must then be drawn out by the arm which protruded, and the head extracted by a crotchet or blunt hook introduced into the foramen magnum, or the mouth; nor will its removal generally offer much difficulty, unless the pelvis be contracted in its dimensions. I can scarcely suppose it possible for any case of transverse presentation to occur, which might not be terminated by one or other of these operations, provided turning could not be accomplished; unless, indeed, the pelvis be distorted in an extreme degree, or almost fully filled by a solid tumor. For if the chest be much pressed downwards, occupying a large portion of the pelvic cavity,—although it would be difficult to surround the neck, so as to amputate the head,—perforation of the thorax would be easy, and delivery could be perfected through its means: while if the child presented, as occasionally happens, with the neck directly over the pelvic brim, then there could be little trouble in passing the finger round the neck, as a guide to the cutting hook; although, indeed, to perforate the chest under

such a presentation would be dangerous, if not impracticable.

I can fully appreciate the horrifying feelings with which you may have been listening to the details of two operations apparently of the most barbarous and savage nature — the shudder which you must experience on hearing any person calmly sanctioning and advising the decapitation, or disembowelling, of a fœtus in utero. Surely, however, I need not add, that such means should never be thought of unless the fœtus be dead; nor, indeed, can they ever be necessary until life is extinct; for if the chest be so firmly impacted in the pelvis as to prevent the introduction of the hand into the uterine cavity, the pressure to which the heart itself would be subjected must destroy the existence of the fœtus, independently of the great chance that exists of fatal compression on the umbilical vessels, both in the cord, and after their division upon the placenta; so that our feelings on that account can never be wounded.

In my friend Dr. Ashwell's publication, which appeared in the year 1828, you will find my opinions on the three subjects last spoken of perhaps more fully explained; for at his request I furnished him with a paper, which he embodied in his work, explanatory of my views; and they have undergone no change since that time.

Antiquity of exvisceration and decapitation. —Neither of the last-named operations are of modern invention. I have already brought before your notice a quotation from Celsus, proving the antiquity of decapitation under a transverse position of the fœtus; and Heister mentions it as a peculiar, new, and easy method,—"*quem Hoorneus sæpe laudatus invenit atque descripsit.*" In the works of Ætius we are also advised, under an abdominal presentation, to open the cavity and take out the viscera; after which, he says, the parts collapse, and the position is easily altered. He says also, when the fœtus presents double, it is better to separate the head from the body, than push up the thorax and deliver by the feet.

Impropriety of removing the protruded arm.— The same author, or rather transcriber of the opinions and writings of others, recommends, when the arm is protruded externally, that it should be amputated at the shoulder-joint; and the practice has been pursued in late days; of which, indeed, instances have come within my own knowledge. From such a recommendation, however, I most strenuously dissent; because the removal of the arm cannot, in the slightest degree, avail us in furthering delivery, and because it has been followed by the most distressing consequences. Thus Chapman gives an instance, in which the attendant, supposing the child dead, amputated its arm while in utero; it was afterwards born alive, and grew up to manhood: and a case of even more aggravated nature occurred in France in the year 1829. A practitioner, at a town in the department of Orne (I am in possession of his name, but I suppose that, in these days of tender regard for broken characters, to mention it publicly would be deemed libellous) removed both the arms of a child which were protruded together, swollen, and livid, after making vain attempts to turn: no blood flowed, but the infant was soon born alive; and surviving the mutilations, the wounds speedily healed. The father brought an action against the attendant; and the tribunal, before which the case was tried, referred the matter to the Royal Academy of Medicine for their opinion. A committee of five gentlemen were appointed, of which MM. Desormeaux and Gardien were two: they drew up a report, strongly censuring the defendant; but it met with much opposition when discussed in the Academy, and was returned to the committee for reconsideration. A second time their answer was of the same tenor, though not conveyed in such strong language. I am not aware of the punishment awarded in this particular instance, or whether the tribunal considered the operator justified, but that is of no consequence in our argument.

While, then, there is a possibility of the infant surviving such serious mutilation, it would be in the highest degree injudicious, not to say criminal, to practise it: but another equally valid reason would induce us to banish the operation; viz. that the dismemberment of the child, in the manner described, can seldom be of the least service in facilitating the delivery of the patient; for I have before proved to you that it is not the arm, partially occupying the vagina, which prevents the entrance of the hand into the womb, but the shoulder impacted in the pelvic brim, and the uterine parietes strongly embracing the child's body. Besides, when the arm has been separated, such a confusion of parts is produced as to render it difficult to discriminate between the fœtal and maternal structures; and should it afterwards be found necessary either to perforate the chest, or separate the head from the trunk, we have lost the means of traction which the arm afforded, and which assists us materially in our operation. For all these reasons, then, let us never think of amputating the protruded limb under transverse presentations.

You must already have anticipated that a transverse position of the fœtus may be complicated with a pelvis distorted to such an extent as to preclude the passage of the hand into the uterus for rectifying its

unfortunate situation; or so small as to prevent its extraction, even if the necessary evolution could have been accomplished. In such an extreme case, the Cæsarean section affords the only possible means of delivery.

PHYSIOLOGICAL AND PATHOLOGICAL OBSERVATIONS ON THE BLOOD AND URINE;

Being the Gulstonian Lectures for 1834, *delivered at the Royal College of Physicians.*

By Geo. Burrows, M.D.
Fellow of Caius College, Cambridge.

Lecture III.—*On the power of the mineral acids to coagulate the blood and other albuminous fluids—On the internal use of mineral acids in phosphatic diathesis of the urine and in passive hæmorrhages, diarrhœa, leucorrhœa, and other fluxes: their mode of action—Reasons for believing that alkalies, salts, and acids, taken internally, act upon the albuminous parts of the blood as chemical agents out of the body—Instances of other remedies acting through the blood—Iodine—Ingredients of mineral waters—Mercury—Hunter's opinions on the appearance of poisons in the blood—Case of the effects of lead—Many substances are decomposed by the stomach, and cannot be found in the blood or secretions—Mode of operation of minute quantities of matter taken into the system—Dr. Prout's theory of incidental bodies entering into the products of organization—Observations on Hahnemann's homœopathic doctrines and minute doses.*

Sir,

In a former part of these lectures, I have adverted to the power the mineral acids possess of causing the coagulation of the blood and other albuminous fluids.

When the permanent influence of an acid is required in the constitution, either to correct an alkalescent state of the urine and deposition of the earthy phosphates of lime and magnesia, or to fulfil other indications, a mineral acid is always preferred by physicians. It would appear from the concurrent testimony of all who have written on this subject, that these acids are beyond the control of the digestive process. Those who are not from experience fully aware of the power of the mineral acids to correct an alkalescent condition of the urine and deposition of earthy phosphates, will not refuse their assent to this doctrine after the study of Dr. Prout's chapter on phosphatic diathesis.

There can be little doubt but that in this state of the constitution these acids act through the blood. There are, however, other diseases in which the mineral acids are employed with success, and in these also they must act through the blood.

The astringent properties of the mineral acids, and particularly of the sulphuric, are highly extolled by the best writers on the materia medica. Dr. Paris* informs his readers "that it is a most valuable medicine, especially in weakness and relaxation of the digestive organs, in colliquative sweats, and in internal hæmorrhages: in epistaxis and hæmoptysis, it was Sydenham's favourite remedy."

Murray writes thus in his System of Materia Medica†:—"Dilute sulphuric acid is used as an astringent to check the flow of blood in hæmoptysis and the colliquative sweats in hectic fever,—indications which it fulfils better than any other article of the materia medica."

Dr. A. T. Thomson‡ informs his readers that "dilute sulphuric acid is given with certain benefit in passive hæmorrhages, from whatever part they may arise. Combined with mucilages, it has been beneficially given in passive diarrhœas."

In diseased states of the urine, these mineral acids taken internally exert their chemical powers upon the salts and earths in excess in that secretion. In passive hæmorrhages, in colliquative sweats and diarrhœas, in leucorrhœa, and in other diseases of a similar nature, I cannot refrain from concluding that the mineral acids exert their beneficial influence by acting to a certain extent upon the albuminous part of the blood, causing a tendency to coagulation, as they do when mixed with albuminous fluids out of the body.

Coagulation is the mode nature takes to repress hæmorrhage from a wounded vessel or any divided part, and we by our internal remedies probably induce a tendency to coagulation when we are successful in repressing passive hæmorrhages.

It is the opinion of the best pathologists of the present day, that these passive hæmorrhages may take place from a part without any alteration of structure in that part.

Andral§ expressly says, "In order that a hæmorrhage should take place, it is only necessary that the qualities of the blood should be so modified that its molecules should lose their accustomed force of cohesion: the blood then escapes with the greatest ease, and hæmorrhages take place

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* Pharmacol. vol. ii. p. 39.
† Vol. i. p. 276.
‡ Dispensatory, ed. 1833, p. 698.
§ Précis d'Anat. Pathol. tom. i. p. 338.

at different parts of the body at the same time, without any obvious cause of irritation. This takes place," adds Andral, "in those diseases where there is little doubt but that the nature of the blood is altered."

Dr. Watson, in an excellent lecture on purpura, delivered from this chair, in 1832, took occasion to say, "that of all the solutions that have been offered with the view of accounting for the escape of the blood from its containing vessels in purpura and scurvy, that appears to be the most simple and the most probable which ascribes it to some morbid alteration in the blood itself."

The account which Huxham has given of the state of the blood in some cases of scurvy which fell under his observation, clearly shows us that in that disease "the molecules have lost their accustomed force of cohesion;" his language is most expressive of this fact: "the blood continued half coagulated, and of a bluish livid colour on the top: it was (he adds) most easily divided by the slightest touch, and seemed a purulent sanies rather than blood, with a sooty powder at bottom." In those diseases, therefore, in which passive hæmorrhages take place, and where the mineral acids would be prescribed with the best possible chance of success, it appears that the blood, according to Huxham, is very much softened, and according to Andral, "the molecules have lost their accustomed force of cohesion."

I have exhibited here the effects of alkaline solutions on the blood when fresh drawn from the veins, the destruction of the accustomed force of cohesion of its particles, and its permanent fluidity. I have also detailed the chemical changes produced in the urine, particularly the increased solubility of albumen in that secretion, by the internal administration of alkalies and salts with vegetable acids; and I have drawn your attention to the consequences of a long-continued and excessive use of the alkaline carbonates; namely, hectic fever, with passive hæmorrhages from different parts of the body. The effects, then, on the blood and secretions by the admixture of an alkaline fluid with them out of the body, correspond to a great extent with the changes induced in the blood and secretions by the internal use of these alkaline remedies.

I next proceeded to exhibit the coagulating power of mineral acids upon the blood and other albuminous fluids out of the body: upon the authority of several authors of repute, I stated that these acids, when administered internally, were not decomposed by the stomach, but produced the most striking chemical changes in the urine, and that their power of repressing passive hæmorrhages was acknowledged very generally. So that dilute mineral acids, mixed with the blood and other albuminous fluids out of the body, induce a much readier coagulation of them; and when these same diluted mineral acids are administered internally, their effects are to arrest serous secretions, and to stop passive hæmorrhages. This latter purpose can apparently only be attained by increasing the diminished force of cohesion of the particles of the blood, or by increasing the tendency of the albuminous particles to coagulation.

From a careful and dispassionate survey of the effects of these two classes of remedies upon the blood out of the body, as chemical agents, and of their effects on the secretions and on the animal economy generally, when taken into the stomach, I think it may now be admitted, that in the latter case it is through the blood, and upon the blood, that they exert their specific agencies; and that, in the two instances, the *modus operandi* is identical. There are many other remedies which offer very convincing proofs that they exert their powers on the system through the circulation of the blood.

Of these medicines I will first mention iodine, for the two following reasons:—1. that, in nature, this substance is found associated with soda; one of that class of bodies, the alkalies, to whose action upon the blood and secretions I have, in a former part of these lectures, called your attention; 2. that when taken in very small doses, from its effects on the animal economy, it offers very strong proofs that it enters the circulation, and, indeed, careful analysis discovers it in several of the animal secretions.

Iodine, when taken internally, seems to operate as a stimulant to the capillaries and absorbent system. Although its good effects, in the first instance, were perhaps vaunted above its real utility, still an experience of twenty years has decided that it is a most valuable remedy; and in the hands of M. Lugol, of Paris, it has lately been employed with a success far beyond what practitioners in this country have obtained.

There are few who will deny its efficacy in the treatment of glandular swellings and of scrofulous tumors. It is evident that iodine enters the circulation, for it operates not only on diseased parts, but it causes the absorption of healthy glands.

The most direct proof, however, that iodine enters the circulation is afforded by chemists*, who have detected it in the blood, in the urine, in the saliva, in the

* Christison on Poisons, 2nd edit. p. 14; Thomson's Dispensatory, 1833, p. 396.

sweat, and in the milk of patients, who were using it internally. I have myself seen a patient in whose urine and saliva it was discovered after the internal use of the tincture of iodine.

This substance volatilizes at very low temperatures, especially when moisture is present: some physicians have lately availed themselves of this property of iodine, and have administered it in the form of vapour by inhalation.

Iodine is known to exert its powers when applied locally, as well as when taken into the stomach: this practice of inhalation, therefore, is founded upon just views, when it is employed in cases where there is formation of crude tubercles in the lungs, although the remedy is very difficult of management in this form.

I cannot help suspecting that with the enormous evaporation which takes place from the surface of the ocean, the moisture taken into the air carries with it certain portions of salts of soda and minute quantities of iodine, which exist in marine plants and in the salt water.

The continual inhalation of such an impregnated atmosphere, and the frequent use of warm sea-water baths, may be some of the causes co-operating to render the treatment of scrofulous complaints so much more successful on the sea-coast than in inland situations.

From personal observations I venture to state, that it is my belief that some of the advantages in the treatment of chronic diseases of the abdominal viscera, derived from a long-continued use of the natural mineral waters of Carlsbad and Toëplitz in Bohemia, and of Ems in the duchy of Nassau, arise from similar causes.

In these places the whole animal frame is subjected to the influence of these mineral waters: the mucous membrane of the alimentary canal, from the copious draughts of the waters, and the external surface of the body, as well as the lining membrane of the bronchia, are continually exposed to the action of an atmosphere abounding in moisture from evaporation of the hot springs, and holding some of their salts in solution.

Those who are acquainted with the localities of these famous springs, situated as they are at the bottom of deep ravines, will admit how unfavourable these situations are for the escape of these vapours from between the high mountains around.

The next medicine of which I will speak, which affords strong proofs of its entrance into the blood, is mercury. Administered in very small doses, we find, in particular constitutions, that preparations of mercury cause the most striking effects in the animal economy.

Mercury, like iodine, appears to act upon man in whatever way it is introduced into the system; whether it is swallowed, or inhaled in form of vapour, or simply applied to the surface of the body. There are sometimes differences in its direct effects upon the system, according to its mode of introduction; but the ultimate indirect effects are the same—the affection of the salivary glands.

There is but one common road open for this substance to these glands, in whatever way introduced, and that is through the vessels of the circulation, and the vehicle is the blood. But we have more direct proofs that mercury enters the blood, when administered as a remedy internally. Without enumerating the cases related in older authors, where mercury has been seen exuding in globules from suppurating nodes on the bones, we may refer to those satisfactory cases in modern times where chemical analysis has discovered mercury both in the blood and in some secretions. In the last edition of Professor Christison's work on Poisons, references will be found to several cases where metallic mercury was obtained from the crassamentum of the blood of persons who had been salivated, although no mercury could be discovered in the serum.

Berzelius, however, informs us that the albumen of the serum of the blood will dissolve many metallic oxides: he enumerates the oxide of iron and the oxide of copper, and adds*, "that it is doubtless that the albumen is the menstruum of the oxide of mercury found dissolved in the blood during a course of this metal for the cure of syphilis."

I pass over the less convincing proofs of the appearance of salts of lead and copper in the blood and other parts of the system, after the internal use of them. The testimony of John Hunter†, however, is too valuable to be omitted. It was his opinion "*that the blood can receive and retain extraneous matter capable of destroying the solids, by stimulating to action, so as to destroy them.*" In support of this opinion he relates the case of a house-painter, brought into St. George's Hospital for fracture of the thigh-bone, and who was also suffering from the paralyzing effects of lead upon the arms and legs. The man died, and Mr. Hunter reports that the muscles of the arms had lost their natural colour: they were opaque, resembling exactly in appearance parts steeped in a solution of Goulard's extract. He does not appear to have analyzed the parts to discover salts of lead. It was also his opinion, "that whatever is dissolved in the blood must be only diffused through it,

* Chemical Properties of Animal Fluids; Med.-Chir. Trans. vol. iii. p. 220.

† On the Blood and Inflam. p. 99.

not chemically combined with it, otherwise (he adds) the nature of the blood itself would be altered, and the effect of medicine destroyed." More accurate knowledge now assures us, that many substances administered internally do combine chemically with the salts and albumen of the blood.

The following cases are strong proofs that substances taken internally enter the circulation, although analysis did not detect them there:—

M. Vernière poisoned an animal with the nux vomica, and having taken a portion of its blood, he injected it into the veins of another animal, which perished with the usual effects of that poison*.

Dr. Graves, of the Meath Hospital, at Dublin, made known the case of a woman to whose epigastrium five successive dozens of leeches were applied, and they all died. It is hardly possible to doubt but that there must have been some peculiar state of the blood in this case, and it was well known that this woman had been taking small doses of hydrocyanic acid a short time previous to the application of the leeches†.

Christison has mentioned a nearly similar case of the death of leeches applied to the epigastrium six hours after taking a large dose of oxalic acid.

In animals killed by the indigo bitter, or carbazotic acid, the peculiar yellow colour of that substance is found pervading nearly all the tissues of the body, proving that it must have entered the organs of the circulation‡.

There are many other substances which have powerful effects upon the animal economy, whose action might be proved to be upon the blood, and through the blood upon the various organs.

But the same direct proofs as in the instances of the alkalies and their carbonates, of the mineral acids, of iodine, of mercury, and other metallic preparations, cannot always be satisfactorily demonstrated.

There is one most important cause which I think is hardly ever sufficiently attended to, and which must ever prevent our detecting some of these substances in the blood: this cause is the well-known power which exists in the human stomach, and probably in many other parts of the body, of decomposing the products of organization, and causing a new arrangement of their elements.

This cause will, therefore, probably ever prevent the detection of the greater number of remedies selected from the animal and vegetable kingdom. The same power deprives so many animal poisons of their virulence when taken internally.

This power of the animal economy of decomposing poisons has been ably illustrated by Dr. Christison, in the last edition of his work on Poisons.

Another cause which prevents the detection of various remedies and poisons in the blood is, that those substances have been taken in very small quantities, and that at present our powers of analysis will not detect such infinitessimal molecules diffused throughout so large a mass as the human blood. It would be most unphilosophical to deny that these substances enter the blood because we cannot discover them in that fluid, after they have been taken internally, although the same and many other analogous substances, administered in rather larger doses, are detected by careful analysis. It appears to me that it must be through this channel that such powerful and sometimes sudden effects are produced on distant organs by those very small doses of mercury which have been described by Dr. Wilson Philip, in his late work, and which are known to most physicians of any experience.

There seems no very obvious mode of explanation of the manner in which other remedies, administered for a length of time in still more minute doses, act upon the system, except by supposing that they enter the blood, and are carried in a state of minute subdivision to every part of the frame where the blood circulates.

How these minute quantities operate we do not precisely understand, but probably their operation is analogous to the explanation given by Dr. Prout, of the agency of those particles existing in the products of organization, and which he has designated *incidental bodies**. Dr. Prout supposes that these incidental bodies are interposed among the constituent molecules of substances: and further, that the molecules of these incidental bodies are in a state of strong self-repulsion. "Such being the case," says Dr. Prout, "it is not unreasonable to expect that they may have the power of modifying the arrangement of the constituent molecules of the products of organization, so as to account for the striking differences between sugar and starch, although the essential composition of these substances is nearly the same."

In investigating the operation of substances introduced into the animal economy, there is no necessity of supposing the particles or molecules of these incidental bodies in a state of self-repulsion. Once admitted into the blood, they are then held in solution, or combine with its elements in given proportions, and carried to every part of the animal economy, pervade the

* Roupell, Croon. Lect. 1832.
† Stevens on the Blood, p. 160.
‡ Christison, 2nd edit. p. 690.

* Bridgewater Treatise, p. 425.

minutest capillary vessels, and thus are brought in contact with every molecule of the solids of which our body is composed, and produce effects which we daily witness.

Such, I have attempted to show, is the *modus operandi* of alkalies, neutral salts, mineral acids, of iodine, mercury, and some other medicines.

I may add, in the words of Dr. Prout, that these observations on the effects of remedies artificially introduced into the system, apply to all those elementary substances, such as chlorine, phosphorus, sulphur, iron, &c. entering into the composition of living organized beings. For no one element, when thus assimilated, appears to be in its natural state, or to be capable of exerting precisely those powers which it is known to exert when acting in virtue of its original organic properties."

"In short," says Dr. Prout, and I am proud of this opportunity of adopting his language, "besides the essential molecules constituting the groundwork of a living organized being, and which probably exert on each other, to a certain extent, the ordinary chemical influences of matter, it would seem that there are, at the same time, diffused throughout the whole living mass, in exceedingly minute proportions, various other matters, the molecules of which appear to be in a high state of self-repulsion,"—or, I would say, by means of their solution in the blood, in a very minute state of subdivision.

"By these incidental matters it would further seem, that the ordinary chemical properties of the essential elements of the organized living structure are variously modified; in particular, that the essential elements are hindered from assuming regular crystalline forms.

"Moreover, these incidental matters entering into the composition of a living body, apparently furnish to the organic agent (or, I would say, to the vital principle,) new powers utterly beyond our comprehension; which powers the vital principle has been endowed with the ability to control and direct in any manner that, from the exigencies of the living organized being, may become requisite."

Now the existence of a circulating fluid is as essential to our ideas of a highly-organized being as a circulating medium is indispensable to our conception of a highly-civilized society. The health and integrity of the animal, as well as the body politic, depends on the soundness of the circulating medium. The greatest disorders of the animal, as well as of political economy, are occasioned by the slightest change, the smallest depreciation of this pabulum vitæ —this main-spring of action. It appears, then, probable, that the pre-existence of some of these incidental bodies, as well as the introduction of others, as remedies, into the animal economy, produce the most striking effects.

The presence of the first are ordained by nature for the preservation of health; the introduction of the latter are resorted to by art to control disease, to stimulate the vital powers to new actions, and thus to put a stop to unhealthy operations.

In corroboration of the powerful effects of small quantities of matter introduced into large masses, Dr. Prout has adduced the beautiful experiments of Herschel, who has shewn that an enormous power, not less than 50,000 times the power of gravity, is instantaneously generated by the simple agency of common matters submitted to galvanic influence: as, for example, by the agency of mercury alloyed with a millionth part of its weight of sodium. Such facts as these place beyond all doubt the efficacy of minute quantities of matter in producing the most extraordinary changes in larger masses.

The homœopathic doctrine of Hahnemann has been at once rejected as absurd by practitioners in this country, on account of the improbability of any efficacy in the minute doses recommended by that physician.

After due consideration of the opinions of Dr. Prout, supported by the above-mentioned experiments of Herschel, upon the power of small quantities of matter introduced into large masses, I think the theory of Hahnemann cannot be regarded so visionary as it appears at first sight.

I am no homœopathist myself, having seen that system most extensively employed in the great Military Hospital at Vienna, in the spring of 1828, by Professor Marienzeller, of Prague, one of Hahnemann's earliest and most zealous disciples. The professors of the Josephine Academy of Vienna were ordered by the Emperor of Austria to report upon the efficacy of this plan of treatment, and they came to the conclusion, that in acute diseases it is powerless and useless.

Some chronic complaints certainly are benefited as much under this system as by the ordinary mode of treatment. Perhaps in our country, where the inhabitants indulge in large quantities of animal food, and in copious draughts of fermented liquors, such simple means would not have counteracted the progress even of chronic diseases, but among the poorer classes on the continent such is not the manner of living.

In the clinical wards of Professor Tommasini, of Bologna, I have seen cases of acute pneumonia successfully treated with repeated doses of the tartarized antimony, and the application of a few leeches to the chest, which would have terminated fatally

if a plethoric Englishman had been the patient so treated.

These observations may therefore render it probable, that much smaller doses than practitioners in general are in the habit of employing in this country, when administered judiciously, may produce effects far beyond what is commonly believed.

Thus far I have attempted to point out the correspondence between the effects of some active substances upon the blood as a fluid out of the body, and upon the solids and fluids of the body when administered as internal remedies. I have adduced still further proofs of the action of remedies through the blood, from the fact, that some powerful substances taken in very small quantities into the stomach can be afterwards discovered by their sensible properties and by chemical analysis in the blood and secretions; and I have ventured to offer an explanation of the probable manner in which other remedies, taken in very minute doses, may operate powerfully upon various organs of the body.

[To be concluded in our next No.]

CASE OF MALIGNANT TUMOR, SIMULATING AN ANEURISM;

IN WHICH THE COMMON ILIAC WAS TIED.

As detailed in a Clinical Lecture,

By G. J. Guthrie, Esq.
Surgeon to the Westminster Hospital.

I mentioned, in a clinical lecture, in August last, that I had tied the common iliac artery, and subsequently I stated that the operation had been successful. I did not say any thing more at that time, because there was still a doubt remaining upon my mind as to there being a complication of disease; and I was anxious, therefore, to delay communicating any thing like a result until such time as the cure might be said to be thoroughly completed, by the disappearance of every remaining vestige of disease.

Unfortunately this did not take place. The person died on the 13th April last, and I have now the opportunity of exhibiting the drawings that were made of the diseased parts, and also of giving a complete history of the case.

The lady had laboured for a long time under pain in her hip—a fugitive sort of pain, which she thought was rheumatic. One day she accidentally struck her hip against the corner of the side-board, and hurt herself severely in the fall; and she was led to attribute to that fall the nature of the evil which afterwards took place. Soon after the accident, she observed that there was a swelling at the part—a tumor; first, she says, as large as a hen's egg, which gradually increased to the size of her fist, and that this tumor beat, or pulsated, very strongly (as she expressed herself to me), "like her heart." It went on increasing during the whole of the year, until at last, finding nothing could be done for her—her disease being considered to be a common tumor—she came up to London.

It was then of very considerable size, as large as an adult head, situated upon the right buttock, and so inconvenient as to prevent her lying on that side, or even on her back. When she landed from the steam-boat at Wapping, the tumor was so uncomfortable to her, from its size, that she dreaded coming up to this end of the town in a coach, and I went to see her. I mention this, because, after the operation, the tumor diminished so much, and became so easy, that she was able to get into a coach very well, and suffered no inconvenience from it.

When I saw her at Wapping, I examined the tumor very carefully, and was for some time under considerable doubt whether it was a malignant tumor or really was an aneurism; however, I at last came to the conclusion that it was the latter, firmly bound down by the great gluteus muscle, which gave it a degree of consistence, and at the same time rendered the pulsation less distinct, although it was very manifest in every part; and on putting the ear to it, the buzzing sound attendant upon the flowing of blood into an aneurism could be very distinctly heard, and it was very audible previously to the operation being performed.

Under these circumstances, on seeing that if any thing was to done, it could only be the serious operation of tying the internal iliac artery, I recommended her husband to have a consultation of two or three of the most eminent surgeons in London. He was so good as to say, as he did not know any body, I might please myself. I therefore, as Sir Astley Cooper, Mr. Keate, Mr.

Thomas, and myself, returned together in the evening, from the Court of Examiners, mentioned the case to them, and requested they would see the patient; not, however, in consultation, for as I had some doubts of the nature of the case, I wished them to see her separately—that each should write his opinion as to the nature of the disease, and that mine should be there for them, and they should see it after they had each written his own opinion. This was with a view to prevent any yielding one to another, which might take place in a doubtful matter; and I had some doubts whether the diagnosis was positively correct. These gentlemen saw the patient separately, each wrote his opinion, and at the conclusion there was a difference. Sir Astley Cooper and Mr. Thomas thought it was an aneurism, and that the only operation was that of tying the iliac artery; Mr. Keate had more doubts, and was inclined to think it was a malignant tumor rather than an aneurism; but on finding there was a difference of opinion, he returned the next day, examined the case again, and came to the conclusion that it was an aneurism, and that the operation ought to be done.

The result of this was, that we decided upon placing a ligature on the internal iliac artery, and the lady went into the Ophthalmic Hospital for that purpose--not as a poor woman, but merely for the sake of having her under more particular observation: she was to pay her own expenses, and she gave £10. to the institution. I told her the operation was of a kind so severe and so dangerous that it could not be performed unless she was under proper observation; and she had the upper floor to herself.

On the 24th August I proceeded to the operation, these three gentlemen, as well as two or three others, being present; and I may now say, that she had been seen by a fifth hospital surgeon in this town (which I was not aware of at the time, though I afterwards heard it), and he also pronounced it an aneurismal tumor, and that an operation was the only thing which could be done. I began the operation by placing her on a table, as much on her back as the tumor would allow, and by making an incision upon the fore part of the belly extending, beginning below the inside of the anterior spine of the ilium, about an inch, carrying it upwards and diagonally inwards towards the edge of the rectus muscle above the umbilicus, so that the incision was between six and seven inches long. I may state, that if the incision is made in the side, from the ribs to the ilium, in a straight line, the greatest possible difficulty is experienced in turning over the peritoneum so as to place your finger upon the last vertebra; but if a diagonal inclination be given towards the rectus muscle, not opening its sheath so as to expose it, but carrying the incision fully up to that part, then there is room to turn over the peritoneum, with its contents, so as to get at the artery.

After dividing, then, the skin and the common integuments, the three muscles were of course also divided in layers; the division of the latter, the transversalis, was attended with very considerable difficulty, inasmuch as there was little fascia transversalis, and the peritoneum was remarkably thin,—as thin as the common white silver paper, or nearly so, that is used for ordinary purposes. On attempting to reach the under part, on the inside of the ilium, so as to push the peritoneum over, I found this could not be done; that the tumor had extended inwards; and some bleeding took place from the large veins which surrounded it, giving rise to the caution not to proceed farther in that direction. At this moment, in spite of the greatest possible care that could be taken by Mr. Keate, who protected the peritoneum, a little nick took place in it, and the small intestine made its appearance below. I then tried to gain a greater extent of room upwards; but where the tendon of the transversalis passes directly across to form the sheath from the lower ribs, the peritoneum is usually so exceedingly thin, and so closely attached to it, that it can scarcely be separated but with the greatest difficulty. I knew this from an operation of a similar kind which I performed at the Westminster Hospital the year before, and in spite of all the precaution that I could then take, the peritoneum was on that occasion injured. I was so well aware of the great probability of its being wounded at this part, that I took double precaution; but in spite of all the care that could be taken, this part of the peritoneum was opened, and

the right lobe of the liver made its appearance through it. The opening on the fore part of the belly was not large enough to admit the two hands, and there were two openings in the peritoneum, one above, and the other below, each of them much disposed to increase in size by the moving of the patient; and the operation did not seem to be the most agreeable one that could be performed. The peritoneum, however, being separated a little from the posterior wall of the abdomen from the outside, four fingers of one hand could now be introduced under it, and turned a little over towards the opposite side.

There is a point here of great importance to recolleet, and it is, that the peritoneum must be raised over without the hand being pushed baek towards the posterior wall of the abdomen but as little as can be avoided; for there is some fat usually at that part, if there be any to be found in the body, and behind whieh you are very apt to get in performing the operation, instead of going in the front; and if you do, it leads to the under edge of the psoas musele instead of the upper, and renders the operation mueh more difficult.

The peritoneum being now separated from behind, and earefully turned over, I found I could only get one hand, or a little more, underneath in seareh of the artery, and that no more room could be obtained by increasing the ineision upon the fore part of the belly. Under these eireumstanees it beeame obvious, that to seek in the dark, without being able to have the advantage of sight, for the internal iliac artery, whieh can hardly be found at any time but with difficulty, was not likely to be attended with suceess, and I therefore determined upon plaeing the ligature on the common trunk of the artery. In order to effeet this, I separated the peritoneum, passed my finger across the psoas muscle until it rested upon the fifth lumbar vertebra; and I now thought, of course, that I must feel the common iliae artery; it was not, however, to be felt. I passed my finger up as high as the fourth lumbar vertebra, trusting that I should feel the end of the aorta; but even that could not be felt, from a eircumstanee that I was satisfied had oecurred in the previous operation, which is not known in surgery, but whieh I will now state: it is, that the common iliae artery rises with the peritoneum, which I believe the vein does not. My finger then, resting upon the spine, was beneath the vessel I was searehing for. Mr. Keate endeavoured to raise the peritoneum, to give me an opportunity, if possible, of seeing the vessel; but that was quite out of the question; the ineision, however long, was not suffieient to allow this to be done. However, on raising the peritoneum a little, he felt the pulsation of the external iliac artery; and I now then, passing my finger upwards, found the common iliae adhering to the peritoneum. I separated it carefully with the point of the fore finger of the right hand, with the finger and thumb of the left—for no more room could be given—I passed the aneurismal needle, and plaeed a eommon thread ligature (now on the table) round the artery, which was done without seeing it. In an operation like this, you must have your eyes at the extremities of the fingers, and your head in your hand. I could bring the artery a little forward, by means of the aneurismal needle whieh was underneath it; and in this manner it was brought into view. It appeared to be perfeetly elear; and I ealculated, from feeling the division of the aorta above, that the artery was tied exactly in the middle.

I now requested Sir Astley Cooper to satisfy himself that it was the artery; whieh he did. I asked Mr. Thomas to do the same; but he turned up his eyes, as much as to say, "You are about in as pretty a scrape as any man who has committed legal murder." However, the ligature whieh is before you was applied around the artery, and the pulsation eeased. I then asked Sir Astley Cooper if the pulsation had eeased, and he said, "Perfeetly; not the slightest was to be felt." The ligature was twisted, the peritoneum pushed down to its place, and the parts brought together, eare being taken that the two cut portions of the peritoneum should be pushed well under the skin, so that they might not be in the line of the ineision, that they might be eovered with fresh parts, and might adhere one to the other. Three long sutures, and three or four smaller ones, were put in just through the skin, in order to prevent the possibility of the parts bursting asunder from the movement of the patient. Thus this, the

most formidable operation I ever performed, was accomplished. It was not formidable from the bleeding, but from the circumstance that room could not be got for the introduction of the two hands; and strange as it may appear, the whole safety and ease in doing the operation consist in the incision being so large at the fore part of the belly that the bowels and the peritoneum may be freely turned over by the two expanded hands, so that you can see what is going on underneath. In the former case, the whole parts could be seen in the peritoneum distinctly, and several gentlemen not in the profession saw the iliac artery in its natural situation. I have no hesitation in saying, that, in the manner I have described, the operation of applying a ligature on the aorta might be accomplished without the least difficulty. I could as easily have tied the aorta as the common iliac artery; it was only necessary to have gone an inch higher. If at any future period it is necessary to pass a ligature round the aorta, it is in this way the operation should be performed.

But this operation will lead to another important result; I believe it will prove that it will never be necessary again to put a ligature upon the aorta. It has been twice attempted, and in both cases the patients lost their lives, just as they would have done if the operation had not been performed. If you examine the parts in the dead body, you will find that if you can only apply a ligature three quarters of an inch from the termination of the iliac on one side, that it requires little knowledge to pass one over the iliac artery of the opposite side; so that, when an aneurism has been formed, and you cannot tie the iliac artery of the side in which the disease is situated, you may be enabled to do it in the way I did this operation from the opposite side, and it is not rendered necessary to tie the great trunk of the aorta at all. That is one of the most important results which I think may be deduced from the operation I have described.

Perhaps the most extraordinary part of the history of this case is yet to come. The patient suffered little or nothing from it: she was put to bed after the operation, which was performed on the Saturday; and there was no augmentation of the pulse till Sunday evening, when it arose to 120, attended with pain; but the latter was materially diminished, though not altogether removed, by the abstraction of fourteen ounces of blood. At 11 o'clock at night I left directions with Mr. Hancock, in case of a return of pain and increase of the pulse, that she should be again bled; and at four o'clock in the morning he took fourteen ounces more, making twenty-eight ounces altogether on the second morning after the operation. She had not after that a bad symptom. I took care not to have her bowels moved for the first four days, and then they were gently opened by the exhibition of an enema of sulphate of magnesia, which was accomplished with little difficulty, and was no inconvenience to her whatever.

The temperature of the limb, after the operation, diminished, but not much; and I attribute it to a method which I adopted on the former occasion, and which I shall adopt in every instance in which I may again have to perform the operation. It was that of having two persons constantly with her day and night to rub the limb; and a hot brick, or hot bottles of water, together with flannel, were applied to the feet, of the temperature from 120° to 140°. One rubbed the lower part of the limb, and the other the upper; and they did this continually for three days and nights. I believe it was by this means that the circulation of the limb was in a great measure preserved. The friction was very slight, so as not to injure the cuticle. If an interval of a few minutes elapsed, a hot towel was put upon the limb. The patient occasionally dozed a little, but the same gentle friction was kept up.

She had not afterwards a bad symptom; and on the 19th September the ligature came away. You will observe it was a small thread ligature, and the noose was tied remarkably tight.

Another point deserving of great attention is, that the tumor immediately subsided; for the next morning it became much flatter; it rapidly diminished in size; it became softer in every direction; and at the end of a month it was diminished one-half; and she could now turn over and lie on her back, and also on her side; and there was every fair reason for believing that it was being absorbed, as is the case in aneurism, and every body looked forward to ultimate success. The Chevalier de

Graefe, about this time, saw the case, and he was satisfied that it was going on well, and that the tumor would ultimately disappear. The only observation he made was this, that, instead of rubbing the limb in the manner I had done, he should have applied cold water. He thought that the best way to prevent mortification taking place, under such circumstances, was the application of cold, instead of warmth. I mention this to show the difference of opinion among doctors; and if such an operation comes before you, you may adopt which plan you please.

In the course of two months the wound healed, except at one small point, and she had no farther inconvenience from it. The tumor, towards the middle of December, had diminished more than one half; but she suffered considerable pain; her health became bad in town, and I sent her to Brompton, and there the pain ceased, under the use of quinine and morphia, in large doses; so that she became tolerably well, and was enabled to sit up, to walk about, and to ride in a carriage to the steam-boat, and embark for Scotland.

When she arrived in Scotland the tumor ceased to diminish, the pains increased, and she suffered a great deal; the tumor gradually began again to enlarge, and increased very considerably, until the 30th of April, when she died, exhausted by disease.

I made it my particular business to correspond with the husband, and with a gentleman in Scotland, to have the body opened, in order that we might know the nature of the disease. I had then no doubt that it was a malignant disease, accompanying an aneurismal tumor, which was supposed to exist in the part: however, it was of no consequence to me what it was, so long as we became thoroughly acquainted with the state of the parts afterwards; and, in compliance with my request, the husband yielded against his own wish to an examination of the body; and I will now give you the heads of the voluminous report made on the subject.

When they opened the body it appeared to be an extraordinary spectacle of emaciation and deformity, particularly at the lower part of the trunk, on the right side, by the formation of an immense tumor, which measured from a point, midway between the navel and symphysis pubis and trochanter, nineteen inches; and, from the middle of the lower ribs to the trochanter, thirteen inches. On touching the crista of the ilium, it felt externally as if it were broken or loose. The measurements upon the opposite side, from the same points, were ten inches and three-quarters from the trochanter to the lower ribs.

On opening the abdomen, the colon was observed adhering firmly to the peritoneum (that must have been the part where the cut was made), and the tumor was found to extend up to the kidney. The internal part of the tumor had all the appearance of a large half organized mass; in one part near it the vessels were of a livid colour, but the rest of it was more of a yellow hue, and had the genuine appearance of the tumor called fungus hæmatodes. With respect to the external tumor, no proper idea can be formed of it without regular drawings of the whole body, but the dimensions formerly given will convey an accurate notion of its size. The surface of the skin was of a natural appearance, as it was when I saw it. Upon being cut into, the whole tumor was composed of a substance resembling half putrid brain, but more organized, and gradually putting on more of a brain-like appearance.

With regard to the state of the arteries, the aorta was quite healthy, and divided at the usual place; in fact, all the arteries of the left side were regular and healthy, and presented nothing remarkable, unless that the internal iliac was rather larger than the external; and on the right side the common iliac artery was obliterated nearly in its middle.

The ligature was applied precisely at the spot where you see a partial depression. When the common iliac of one side was compared with that of the other, it was found to be three-eighths of an inch shorter: there is a little difference in the natural state, but not so much as that. The difference between the two was evident, as well as the narrowing in the centre, where the ligature was applied.

A little opening was made in the artery above and below where the ligature was tied, in order to observe the state of the vessel; and it appeared that it was pervious to within the eighth of an inch on each side of it; so that the ligature just cut the artery through, and the two cut edges

united by adhesive inflammation above and below, without forming a coagulum, and without diminishing up to the next collateral branch—a fact which I have said always existed—but which is quite contrary to the received opinions of the present day. The artery, then, carried blood from August to May down to the point of obliteration, and returned it upwards to the same point; and, strange to say, the internal iliac was larger on that side than on the other, and the external iliac quite as large, so that the circulation appeared to be fully established. The principal arteries for effecting this seemed to have been the epigastric and circumflexa ilii, both of which were enlarged; the latter, it was thought, had acquired double its natural size. The arteries of the pelvis must have communicated freely.

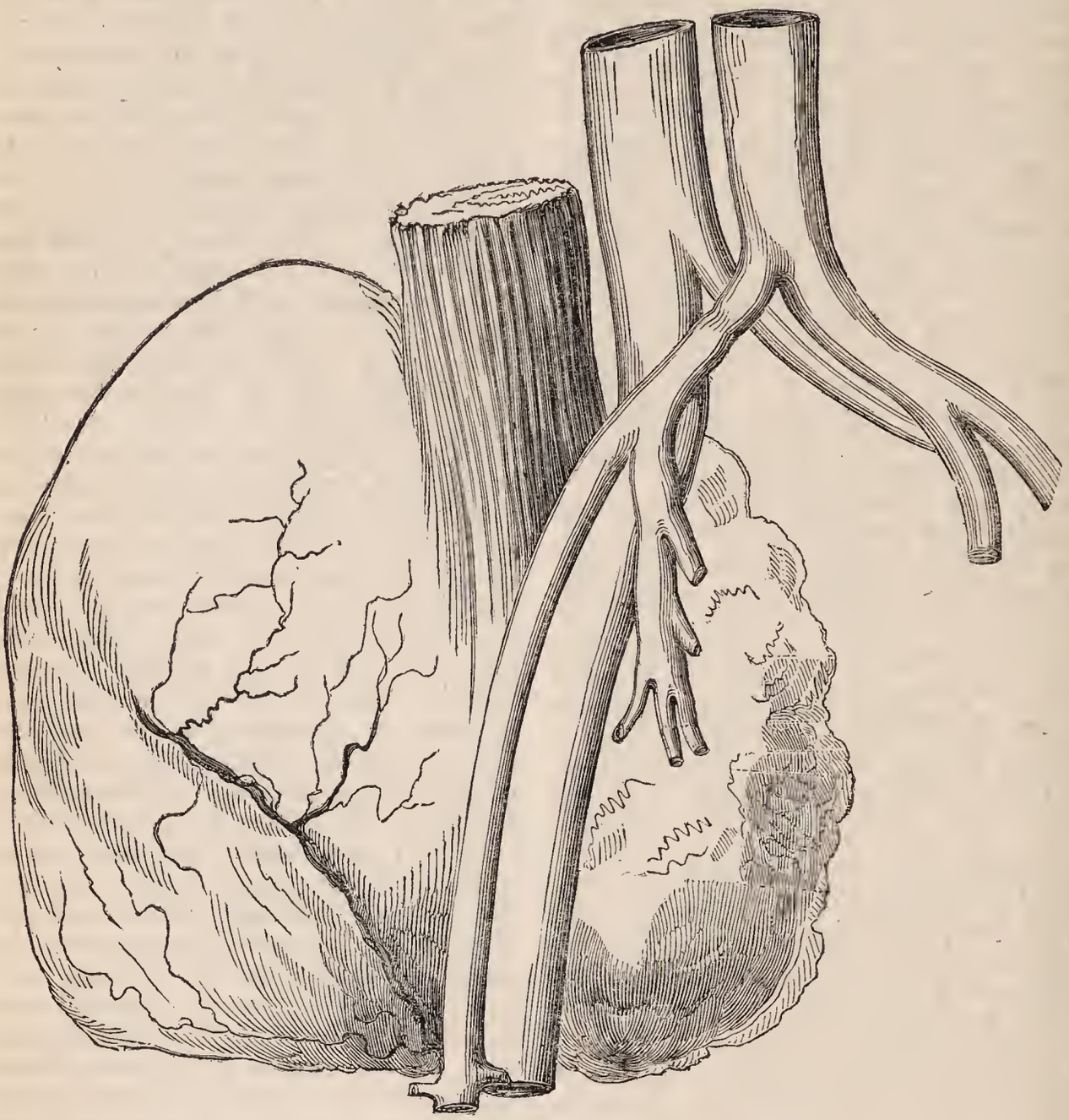
In the first place, then, there was in this case a large tumor, as large as an aneurism, possessing its pulsation, having the peculiar sound which an aneurismal tumor is supposed to have, which we were all guided by, and which proved to be not an aneurism. Now, that a mistake was made is quite clear; but my regret is, that if a similar case were to occur again, I do not know how to remedy the evil. I know we might have done nothing; but the next case attended by these symptoms may be aneurism. Sir Astley Cooper said, "I had a similar case to this some years ago, and I recommended an operation; it was before operations were so common upon the great vessels, and the man did not choose to submit to it. He went into the country; the aneurismal tumor increased inwardly, and burst into the bladder; and he bled to death—he poured out blood through the bladder till he died." Either, then, we must leave patients, under these circumstances, to die, when an operation, which has been successful, might be performed, and would cure the patient, or we must sometimes consent to an operation, even when we are in doubt. I declare, that in this case we all had a doubt as to the matter; we all came to the conclusion, that it was more in favour of aneurism than any thing else; and we all assented to the operation, without the least doubt as to the propriety of it. And, as I before said, I only regret, that if a similar case were to occur to-morrow, I should not be able to point out a diagnostic mark between an aneurismal tumor and one like that of which this lady was the subject. The only thing which could be done in addition, would be to push a grooved needle into the tumor; when some medullary matter, if any, might come away in the groove.

I may add, that the disease destroyed through the bones; and that the head of the femur was found lying in the middle of the tumor itself.

I am sorry to say that a great many efforts have been made by different persons to get drawings of this case. I do not know why gentlemen should have been so anxious respecting it. The doctor who attended the lady in Scotland tells me that he has been applied to by persons in all parts of the country, wanting to get the artery, or a drawing, that they might make the case public. All I can say is, they know little of me to suppose that, occupying the responsible station I do, I should not be the first person to give a case of this kind to the public. It is true there has been a mistake; but to imagine that I should hesitate for a moment to communicate to the professional public that which might be of advantage to them or to mankind, is a sort of thing I do not understand. I only know, that if I had an opportunity of getting a drawing or a preparation illustrating a case of this kind which belonged to any one, instead of asking for it to publish it myself, on the contrary, I should have gone to them and said, "I am delighted that I have procured this, which you could not; it is yours, and you shall be the person to avail yourself of making the public acquainted with it." But, then, different people have different modes of proceeding. I believe, however, there is only one right course—that of unflinching honesty and truth.

Now, so far from having the least desire to conceal any part of the case, I was very desirous that it should be known to all mankind, and therefore I gave myself a great deal of trouble to procure the information, which I have laid before you, and which no one but myself could have obtained. Nothing but my request could have induced the husband to allow the body to be opened. And had I in the most indirect way in the world expressed a wish to the contrary, it could not have been done. But I have gone

farther: I have got not only a drawing, but a lithographic plate, which will enable every person who looks at it to know all about it. I have ordered two hundred copies to be struck off for myself, and then any person who chooses to pay for a copy may have it *.



ON DISLOCATION OF THE ASTRAGALUS; WITH CASES.

To the Editor of the Medical Gazette.

Sir,

You will oblige me by publishing, at your convenience, the accompanying paper on the dislocation of the astragalus.—I am, sir,

Your obedient servant,

B. Phillips.

17, Wimpole-street,
July 21, 1834.

To communicate to the profession any addition to the existing catalogue of diseases by which human nature may be afflicted, is unquestionably a necessary, though not very grateful, task. Happily, however, in the present advanced state of professional knowledge, it is an occupation not often imposed upon us.

The disease to which I am about to allude, from the peculiar violence which is necessary for its production, cannot, happily, be of frequent occurrence.

The two cases which I shall immediately detail, I cannot believe to be so isolated as to be the only examples of

* It is from the front view of the parts, in the plate alluded to, that we have had the above woodcut executed.—E. G.

the kind; although such an inference might be drawn from the absolute silence of authors upon the subject.

The only mode by which this silence can be accounted for, is by assuming that the affection must have been confounded with other affections of the same region, or that it may have been more complicated than in the cases I am about to detail.

The accident to which I allude is, complete dislocation backward of the astragalus, the relative position of other bones having been scarcely disturbed, and no communication being established with the exterior. To demonstrate the difficulty with which this displacement is accomplished, we should take a leg, place the foot (in a wooden shoe) in a vice, screw it there, so that no motion shall be permitted, and then apply any quantity of pressure backwards or forwards. Once arrived at the natural limits of these movements, the pressure still continued, we shall experience a powerful resistance. This, however, may be overcome, and a very loud crackling noise will be produced.

Upon dissecting the articulation which has been the subject of experiment, if the pressure has been forward, by which the leg will have been flexed upon the foot, we shall find the posterior fibres of the lateral ligaments ruptured, but no displacement of the astragalus. We shall obtain the same result if we make the leg the fixed point, and then forcibly extend or flex the foot upon it.

It would seem that such violence ought to produce luxation of the foot backwards or forwards; but we shall almost always find that our attempts in this way to produce such effects will be vain.

The first case on which I was consulted, occurred in the summer of 1832. A gentleman (Mr. G.), aged about 35, was driving himself and two ladies in a phaeton; the horse became unmanageable, and the reins broke. Mr. G. threw himself from the carriage, with the hope of succeeding in stopping the horse, alighted upon his feet, but immediately fell forward to the ground.

He was brought home, and, in about two hours from the occurrence of the accident, I was requested by his medical attendant to consult with him upon the case.

The kind of injury which had occurred was immediately apparent, by the projection which was presented just above the os calcis, and the absence of any other very apparent change in the relative position of the other bones.

The tendo achillis was pressed backwards by the displaced astragalus, so as to describe an angle of about forty degrees; and at one point it had reached so near to the surface, that vesication of the skin was produced directly over it.

Upon more careful examination, there could be no doubt about the nature of the accident. That the projection of the tendo achillis was produced by the astragalus was certain, for there was no fracture of the tibia or the fibula. The tibia was slightly displaced forwards upon the foot, and the os calcis retained its natural position.

The anterior portion of the foot, when compared with the other, appeared a little shortened, and this effect was produced by the action of the lever, which was developed in the following manner. The muscles of the calf were quite rigid —were in spasmodic action. This, then, was the power: the tendon of these muscles was stretched over the astragalus, which was the fulcrum; the resistance being great, the inferior extremity of the leg yielded, and a slight forward displacement was the consequence.

There was very little ecchymosis, and it was not at all evident upon what portion of the foot he had rested in coming to the ground. As he fell forward upon reaching the ground, it appeared probable that the foot was suddenly and forcibly flexed; and this is the more probable, because in that movement the anterior border of the articular surface of the tibia may meet the neck of the astragalus, which may thus arrest the movement of flexion before it has proceeded far enough to produce luxation of the leg. If the place upon which he rested was, as is probable, the side of a bank, the accident would appear to be as much more likely to occur. In that case the axis of the tibia is oblique in relation to the articular surface of the astragalus, in front of which it is precipitated, in obedience to the impulsion which it receives from the weight of the body.

Before the reduction was attempted, the patient was bled to twenty ounces, and nauseated by tartar emetic, the effect of which was maintained for an

hour, during which time the efforts to reduce the bone were uniformly sustained.

The point to which our endeavours were directed was first to produce the greatest possible quantity of flexion of the foot upon the leg, at the same time, by means of pullies, removing those organs as far as possible the one from the other; and by means of pressure upon the astragalus, endeavouring to restore it to its natural situation.

These efforts were unsuccessful, and therefore only one of two modes of proceeding remained for our choice—to leave things in their then state, merely combatting any untoward symptoms which might be developed, or to extract the astragalus.

Having in my recollection, among others, a case which occurred in the practice of Dupuytren, in which the astragalus was displaced forwards, and the long and painful operation which occurred in extracting it, the long convalescence which followed, and the permanent lameness which was the consequence, I declined to accede to this proposition. Leeches in large numbers were applied to the seat of injury; the bowels were carefully regulated; no bad symptom occurred; an artificial articulation was found between the bones of the leg and the os calcis; little inconvenience was occasioned by the astragalus; and when I last heard of him he walked nearly upright.

The second case upon which I was consulted occurred during the last summer. A gentleman, aged 32, was occupied in playing cricket; while fielding, and running very rapidly after the ball, a gutter, which was in his course, was not observed. The toe rested upon the further side of this gutter, while the heel was jammed directly into it, and he fell forward.

He was unable to rest upon the injured leg, and it was evident to the bystanders that a displacement had occurred. I was not summoned till the following day; some inconsiderable attempts at reduction had been made on the previous evening; when the leg presented an appearance very similar to that which I have described in the former case, except that there was more ecchymosis about the external malleolus. He was bled to sixteen ounces, and twenty leeches had been applied to the injured part. In this case there was, when I saw it, considerable tumefaction, and I declined to make any further attempts at reduction, or to remove the displaced bone.

It was necessary, from time to time, during the first three weeks, to apply leeches to the part, and to keep the leg motionless, for the purposes of repressing inflammation, with which he was threatened.

The accident occurred in August. The first week in November he was able, with the aid of a stick, to walk a short distance; and on the 1st of January scarcely any lameness remained. Considerable extent of motion was produced in the new articulation; and the only inconvenience of which he has complained is the necessity (temporary, I believe), which has been imposed upon him, of wearing a shoe, the hinder leather of which shall not reach so far as to come in contact with the projecting astragalus.

In each of these cases "artificial" articulations were formed. Articulations which are thus formed by displaced bones have not been often made subjects of observation, except in the humerus and femur. The former hollows out for itself a new glenoid cavity upon any portion of the surface of the scapula with which it may be in apposition; usually, however, on its anterior surface; the latter bone produces a similar effect upon the external surface of the ilium, the ischium, or the pubes. It is not simply a pressure and wearing away which produce these cavities: the action of the absorbent vessels also contributes to it, by removing a part of the osseous substance, upon which the head of the displaced bone exercises a constant pressure. The supplemental articulations are, however, mainly the work of an inflammation, which scarcely ever terminates in destruction of the part. The head of the displaced bone commonly preserves its diarthrodial cartilage; to this head corresponds a glenoid cavity, covered with a slight stratum of cartilaginiform substance: an albuminous fluid moistens these surfaces: an articular capsule, formed of a dense tissue, and strengthened by fibrous fasciculi, surrounds it. The internal surface of this capsule is smooth, not because it is invested by a synovial membrane, like the natural articulation,

but occasioned by the mechanical action of the head of the bone, which appears to rest here like bullets, which, after a lengthened sojourn, are found inclosed in a cyst, with smooth and humid parietes.

There can be no doubt, however, that there occurs on these parietes a true exhalation, similar to that by which the synovia is furnished in articular capsules.

When a disease is of unfrequent occurrence, it may be of less consequence to establish rules for the guidance of medical men, than in others which are more commonly met with; if, however, it were otherwise, it must be evident that I am not in a condition, from the result of only two cases, to frame any general rules for the guidance of those to whom such an accident may occur.

The appearance of the limb in these two cases was very similar. The astragalus had passed backward from the mortise formed for it by the tibia, the fibula, and the calcis. There was elongation of the heel, immobility of the foot, a projection of the tendo achillis, a shortening of the anterior portion of the foot, an anterior projection of the inferior extremity of the tibia. As the tumefaction lessened, the form of the astragalus was pretty distinctly defined.

That a reduction of this bone, when it has completely escaped, can rarely, if ever, be effected, appears to me certain.

For the purposes of consideration, we may, in this accident, almost compare the astragalus to a wedge, the base of which is presented towards the joint; and I can scarcely believe it possible to separate the os calcis from the tibia and fibula to a sufficient distance to allow of the admission of the astralagus. If this reasoning be correct, it will be useless to torture the patient with futile attempts at reduction.

If, however, the bone be not completely removed from between the os calcis and the tibia and fibula—that is, if only a partial dislocation occur—we ought to employ such means as shall appear best adapted to the purpose, and we shall no doubt succeed in the reduction. All that can remain for us, therefore, in complete dislocation of the bone, is either to expect it making, by this means, a communication between the cavity of the joint and the exterior, or to suffer it to remain in its new resting-place.

Wounds, establishing a communication between the cavities of joints and the exterior, are very serious diseases; and to avoid this communication was the reason of my disinclination to consent to the removal of the astragalus in the two cases which I have detailed. I think it will be generally admitted that it was, at all events, the most prudent course, to avoid every thing which was likely to increase the tendency to inflammation, which we might be unable to control; for in this case, the limb, if not the life, of our patient would be most probably sacrificed.

A CASE IN WHICH

TRANSFUSION WAS SUCCESSFULLY PERFORMED;

With Remarks.

By Robert Bickersteth, Esq.

Surgeon to the Liverpool Infirmary.

On Tuesday evening, the 27th of August last, I was desired to visit Mrs. H. N., æt. 30, who had just completed her eighth month of pregnancy of her fourth child. I found her labouring under violent pain in the uterine region, with intervals of ease, and without any uterine discharge. On examining the state of the os uteri, there was no dilatation, *nor had the pains any effect upon the lower part of the womb.*

I immediately administered a large dose of laudanum, and remained with her two hours, at the end of which period the pain began to lessen. I directed them to inform me if the pains should re-commence, or if there should be any particular change. In two or three hours her husband came to inform me that in getting her up she had felt very faint, but that this feeling had gone off again on replacing her in bed. At 3 A.M., on the 28th, I was again sent for. On my arrival, I found her in strong labour, and in about twenty minutes she was delivered of a female child, which was dead. The placenta came away immediately afterwards, without any effort, and, in fact, appeared to have been separated long before the birth of the child. The expulsion of

the placenta was followed by a very large discharge of coagulated blood and great faintness. Strong pressure was made on the abdomen; and brandy, ammonia, and opium were administered. I watched her most anxiously for two hours, when, not perceiving any improvement, I desired that another surgeon might be called in. Mr. Blackburn was immediately sent for. On his arrival, she was very faint, her extremities were cold and clammy, and although the pulse was perceptible—she was able to answer questions, and appeared quite sensible—she has since declared that she had no recollection of what followed the birth of the child. Mr. B. thought we might safely wait a little longer before resorting to other means. We accordingly continued the use of cordials, warmth, and pressure on the abdomen, for two hours longer, when, as there appeared to be no improvement, and every prospect of dissolution, we proposed the operation of transfusion, which was immediately assented to. Her husband's sister was the individual selected to supply the blood. An opening was made in the median vein of the right arm of the patient, having previously laid it bare, and the tube of the transfusion apparatus was inserted into it. There was sufficient circulation in the vein to occasion its bleeding, and it therefore kept the tube filled with blood until the communication was formed with the pump. Mr. Blackburn, who attended to this part of the operation, then began to pump, the funnel attached to the instrument being kept supplied by my assistant, Mr. Lander, who directed the stream from Miss N.'s arm directly into it. In less than two minutes, and after three or four syringefulls of blood had been injected, the patient opened her eyes—showed signs of sensibility—said that she felt better—that her breathing was easier—that the oppression about the heart was diminished. We continued to inject slowly for eight or ten minutes,—we conjectured ten or twelve ounces of blood,—when the patient expressed herself, and appeared so much better, that we did not think it necessary to go any further. Her breathing was quite good—warmth restored—pulse much improved;—in short, she had no bad symptom remaining, and none returned. She recovered her strength slowly, and the wound in the arm healed without any difficulty.

I wish here to draw the attention of the profession to two circumstances.

1st. As to the cause of the death of the child, the faintness, and hæmorrhage. I believe that the very violent pain which was experienced the day before the delivery, was occasioned by irregular and spasmodic action of the fundus of the uterus—to such a degree as to produce a separation of the placenta long before any action was produced upon the mouth of the womb. This will account for the death of the child, and the absence of flooding until after its expulsion.

2nd. The importance of having recourse to the operation of transfusion, if possible, *before the circulation has ceased in the arm of the patient.* Having had occasion to inject the saline solution in several cases of cholera a year before, and having had much difficulty in effecting this, owing to the blood having coagulated in the veins, I was most agreeably surprised at the facility with which the injection was effected in this instance. The circulation in the patient, feeble as it was, was sufficient to fill the tube with fluid blood, and thus prevent the possibility of injecting any air. I do not mean to say that recovery would have been impossible if the operation had not been performed, but I wish to press its adoption in similar cases, on these grounds only;—1st, the immediate benefit experienced, and the relief of the patient and all her friends from the anxiety attendant upon so distressing a situation—and, 2dly, on the facility of its execution.

The instrument we used was Reid's syringe, which was immersed in a basin of hot water, and the blood was received into a funnel attached to it*.

ON PINK DEPOSITS IN THE URINE.

To the Editor of the Medical Gazette.

Sir,

We shall feel obliged by your inserting in your journal the following remarks on those pink deposits found in the urine of patients labouring under fever and certain visceral diseases.

It is the opinion of Dr. Prout, (and

* Liverpool Medical Journal.

the same opinion is entertained by most, we believe), that the deposit in question consists of the purpurates of ammonia or soda, most commonly the former, resulting from the decomposition of uric acid, by the agency of nitric acid, which last is secreted in some of the urinary organs. Now, although it is manifestly impossible to determine by what *modus agendi* chemical changes of the most complicated nature are on all occasions brought about in the animal economy, still it appears somewhat improbable that sufficient free nitric acid could be eliminated, either in the kidneys or bladder, to produce by its action on the uric acid present, the large quantity of purpuric acid, which, according to this theory, must be present, in order, by its union with bases, to form the copious pink deposits which are so constantly met with. If, therefore, we are unable, by any process of reasoning, to disprove the above theory, it only remains for us to have recourse to comparative experiment, so as to subject the peculiar colouring matter of the urinary deposits and purpurate of ammonia to the action of similar tests. Dr. Prout was confirmed in his opinion by the circumstance of finding nitric acid, combined with a base, in urine yielding the pink deposits; but this may be considered as a rare occurrence; for, out of eleven specimens of urine, each yielding the pink deposits, examined by Wurzer, the German chemist, there was only one which afforded any indication of nitric acid. Vogel, and afterwards Frommherz and Gugert, undertook the examination of the pink deposits, and they came to the conclusion, that the colour was not owing to the presence of a purpurate, inasmuch as it was removed by alcohol. No satisfactory analysis of the whole of the deposit has been, we believe, published; and the action of re-agents on the coloured alcoholic extract, given by the above-mentioned chemists, is incomplete. The following experiments were therefore performed, with a view, 1st, of determining the exact composition of the pink deposit; and, 2dly, in what manner the colouring principle of the same deposit differed from purpurate of ammonia in its behaviour towards re-agents.

About fifty grains of the pink deposit were boiled in alcohol, specific gravity 833, until nearly the whole of the pink colour was removed, the insoluble residue possessing only a slight pink tinge. The alcoholic solution was evaporated to dryness, and a pink residue obtained, which was examined as follows:—

1. Dilute nitric acid dissolved it, forming a nearly colourless solution, yielding a pink colour by careful evaporation. The colour was much heightened by exposing it to the fumes of ammonia.

2. When digested in a cold solution of caustic potass, ammonia was evolved.

3. When exposed to a low red heat in a platinum crucible, with a cover, so as to admit of a sublimate being collected, a very inconsiderable one was obtained. Soluble in water, but not precipitable either by nitrate of silver or chloride of barium, nor evolving ammonia when treated with caustic potass. When the heat was increased so as to drive off all volatile matter, a very inconsiderable white ash was left, exerting an alkaline re-action, and soluble in water. When exposed to the blow-pipe flame, it coloured it yellow.

The matter insoluble in alcohol was boiled in repeated portions of distilled water, and the whole filtered; the solution was evaporated to an inconsiderable bulk, and then examined.

1. Muriatic acid produced, after slight reposure, a white crystalline deposit.

2. Oxalate of ammonia produced a white precipitate.

3. Caustic potass evolved ammonia, but caused no precipitate.

4. Nitrate of silver caused a precipitate not soluble in nitric acid.

5. Chloride of barium, a mere opalescence.

6. When a portion of the solution was evaporated to dryness, and the residue exposed to a low red heat, a white sublimate was obtained, soluble in water, evolving ammonia when treated with caustic potass, and producing a white precipitate with nitrate of silver.

7. The residue from last experiment was exposed to a higher temperature; a white ash was obtained, soluble in water, exerting a strong alkaline reaction, and effervescing upon the addition of an acid; the concentrated aqueous solution was neutralized by a drop or two of dilute muriatic acid, and then mixed with chloride of platinum and alcohol; no precipitate ensued after some time.

The portion soluble in water and alcohol was boiled in a solution of

borate of soda; scarcely a trace of insoluble residue remained. A current of carbonic acid gas was passed through the solution, which became at first opalescent, and afterwards caused a pulpy gelatinous precipitate; this precipitate, when treated with dilute nitric acid, yielded, by evaporation, a pink stain.

The following appeared, therefore, to be the composition of the pink deposit:—

Matters soluble in alcohol, consisting of.....
- Urate of ammonia.
- ——— soda.
- Colouring matter.

Matters soluble in water, consisting of.....
- Muriate of ammonia.
- Urate of ammonia.
- ——— lime.
- ——— soda.
- Alkaline sulphate (a trace.)

Matters soluble in water and alcohol .....
- Free uric acid.
- Probably a little earthy phosphate and animal matter.

Having shewn what the constituents of the pink deposits are, we now proceed to the action of reagents, both on the colouring matter soluble in alcohol, as well as on purpurate of ammonia obtained in the usual way.

Colouring Matter soluble in Alcohol.		Purpurate of Ammonia.
Dilute sulphuric acid..	Produces no alteration of colour in the cold; by boiling, the colour is somewhat diminished.	Entirely destroys the colour.
Sol. of caustic potass..	Produces a greenish-brown solution.	Produces a purple or lilac colour, which is entirely dissipated by heat.
Concentrated mur. acid	Produces no alteration of colour in the cold; by boiling, the colour is somewhat diminished.	Discharges the colour even in the cold.
Sol. caustic ammonia	Produces a greenish-yellow coloured solution.	Causes an increase of the pink tinge, which is not discharged by gentle evaporation.
Sol. carb. of potass ..	Produces a yellow-coloured solution, without any distinct green tinge.	Produces a deeper pink, becoming purple only upon the application of heat.
Sol. proto-muriate of tin	Does not destroy the colour even by ebullition.	Destroys the colour even in the cold.
Hot acetic acid	Does not destroy the colour.	Entirely discharges the colour.

We think these experiments, as far as they go, are sufficient to prove that the peculiar pink colour in urinary deposits is not dependent on the presence of the purpurates.—We are, sir,

Your obedient servants,

R. H. Brett,
Golding Bird.

Guy's Hospital, July 19, 1834.

EFFICACY OF BELLADONNA.

To the Editor of the Medical Gazette.

Sir,

If you consider the following cases of acute conjunctival inflammation, in which the utility of the external appli-

cation of the extract of belladonna was particularly well exemplified, sufficiently interesting to merit a place in your valuable publication, I beg you will give them an early insertion; and am, sir,

Your obedient servant,

JOHN W. WEST,
M.R.C.S.L.

Poole, June 17, 1834.

CASE I.—*Conjunctival and corneal inflammation of both eyes.*

Mrs. King, aged 35, a stout, healthy-looking woman, the mother of several children, was attacked, on the 20th of February, by acute inflammation of the conjunctiva of both eyes, with considerable swelling, in consequence of cold. It was attended with an unusual degree of tumefaction of the upper and under lids of both eyes; so much so, that it was with difficulty the state of the organs could be precisely ascertained. She then applied to a druggist, who merely recommended her an opening draught, which she took. She remained without further advice until the 23d, when I was called to her house, and found her labouring under violent inflammation of the conjunctiva and cornea of both eyes, with considerable opacity of the latter, and the pain so acute that she would scarcely permit me to examine them. I discovered on her neck and face a slight eruption, which I was inclined to suppose was syphilitic, but was unable to collect sufficient information to substantiate the opinion. Her pulse was 120, hard, and full; tongue much furred; bowels constipated. I ordered her to be bled to syncope, and the extract of belladonna to be applied beneath each eye, also the following medicine:—

Haust. Purgans statim sumend.
℞ Liq. Amm. Acet. ℥iij.; Vin. Ant. Tart. ℥vj.; Sulph. Magn. ℥vj.; Aquæ Puræ, ℥iss. fiat mist. cujus s. cochl. duo magna 4tis horis.

24th.—Pain in the eyes not quite so violent, but the redness and tumefaction as much as yesterday. Blood taken yesterday is buffed and cupped. Pulse 110; bowels open.

Rep. V.S. ad ℥xii. Cont. Mist.

The belladonna to be applied this evening as before.

25th.—The right eye is considerably better; the redness and swelling much diminished, and the opacity of the cornea not so great; but the left is not relieved.

Hirudines, vj. sing. temporibus; Admoveatur Emp. Lyttæ nuchæ.

26th.—Both eyes free of pain, and the redness and swelling much less.

Cont. mist. et Belladonna.
℞ Hydr. Subm. gr. ii.; Pulv. Ipecac. Comp. gr. iii.; Conf. Cynost. q. s.; ut fiat Pil. eum sing. dos. Mist. sum.

28th.—The right eye free of inflammation, but the left is more swollen and red.

Hirudines iii. temp. sinist.
The Belladonna to be applied to the left only.

March 1st.—The pain and redness in the left much diminished; gums tender.

Cont. Mist.; Omit. reliq.

8th.—Both eyes quite recovered.

CASE II.—*Conjunctival inflammation—Recovery.*

Mrs. Marsh, aged 24, of a sanguineous temperament, married, and the mother of one child, was attacked with severe conjunctival inflammation in both eyes, for which leeches and blisters were applied. On the 3rd of March I saw her; and, as the symptoms were not diminished, I ordered

Hirudines vj. sing. temporibus; Haust. Purgans quamprimum sumend.
The Belladonna to be used at night.

5th.—The eyes are less turgid and painful this morning; bowels open.

Admoveatur Emp. Lyttæ nuchæ.
To continue the Belladonna.

8th.—Complains this morning of heaviness and pain in the head, but the eyes are free from inflammation.

Hirudines iii. sing. temp.; Haust. Purgans statim.

12th.—No return of pain in the head since the application of the leeches, and the eyes quite restored.

In several other cases of inflammation of the eyes I have used the Belladonna as an external application; and in every instance I found it quite as successful, without producing any unpleasant symptoms. I am aware that it has not hitherto been used generally, except in cases requiring dilatation of the pupil; but it will be found, in all instances of inflammation of the eyes, highly serviceable.

EFFICACY OF THE SECALE CORNUTUM,

IN A CASE OF IRREDUCIBLE PROLAPSUS UTERI.

To the Editor of the Medical Gazette.

Sir,

The medical world has, within the last few years, been treated to numberless details of cases, having for their object the establishment of the remedial powers of the ergot of rye. From the accumulation of many undisputed facts, it is now almost universally allowed that this ingredient *does* exert a specific effect upon the gravid uterus at the period of parturition: other facts lead to a more extensive estimate of its powers, not only upon the uterus in its gravid, but also in its unimpregnated condition; and if the following facts, which fell beneath the observation of the present writer, be confirmed by the experience of others, the valuable agency of the secale cornutum may even be more extensive than many of its warmest eulogists have as yet supposed.

Elizabeth Boden, æt. 36, a washerwoman, applied to the Ardwick and Ancoats Dispensary, on the 17th ult. for the relief of what was termed by the messenger "a severe bearing down." On visiting her, I ascertained the circumstances of her case to be as follows. She had been married sixteen years, and had borne six children; all her labours having been perfectly natural, according to her own account. The birth of her last child occurred about five years ago; and she stated that she then left her bed in about thirty-six hours after the labour, and, in about a week longer, had resumed her ordinary occupation. In about nine months afterwards, she first became conscious of a slight bearing down in the pelvis, accompanied with fluor albus, which last has continued up to the present time. The bearing down continued to increase, evidently in consequence of the harassing nature of her occupation; so that at the end of the second year of its existence, a tumor appeared externally, which disappeared, however, on assuming the horizontal posture. This state of things continued until within a few hours of her application, when a complete prolapse of the uterus took place; and as it did not recede as before, when in the horizontal position, she became alarmed, and sought medical aid.

When I saw her, she was suffering under all the ordinary sensations occasioned by *prolapsus uteri*. On examination, which was in about sixteen hours after the complete prolapse had occurred, the uterus was considerably enlarged; indeed, in a condition decidedly œdematous; and the inverted mucous membrane of the vagina was so completely distended, that, had not anatomical considerations interfered, it might have been mistaken for the peritoneal covering of the uterus.

Attempts at reduction were made by myself and two other medical gentlemen, but without success, plainly referable to its swollen condition; for the size of the tumor was little, if at all, below that of a fœtal head. This enlargement of the uterus had evidently occurred subsequent to its last descent; for it had occurred whilst in bed, and had occasioned no sensation of forcing, whilst, at the time of which I am speaking, its size was such as to have rendered any such easy descent exceedingly improbable: at any rate, had there been no enlargement subsequent to the prolapse, it is not likely that a tumor of the above-mentioned bulk would be readily re-introduced.

Attempts at reduction having failed, we directed the employment of mild aperients and warm fomentations, with an injection; of course, to maintain the horizontal posture. We considered that by gently acting upon the bowels, and soothing the parts, the tumor might be so reduced as to render reduction practicable. We adopted this proceeding for about twenty-four hours, at the expiration of which time we again made the attempt, but with the same result. Under these circumstances, it became necessary for us to think of some other proceeding, whereby we might remedy the condition upon which the irreducibility depended. Well, then, taking into account the whole of her previous history, which included a menorrhagia of some months duration, and observing that there was no material pain upon pressure, with other direct evidence of our conclusion, we inferred that the enlargement was of an asthenic nature, and thought it probable that if we could exert a sthenic influence upon the displaced organ, we might not im-

probably accomplish our object. With this view, we directed the employment of the secale cornutum, to be taken in scruple doses every three hours. After she had taken four doses, we again saw her, and with tears in her eyes. The poor woman informed us that she was much worse, and, as an evidence of the impartial nature of the account she gave, she observed, with the greatest simplicity, (not having been made acquainted with our intentions in the last prescription), that "the powders had not worked her yet," but that she had had a great deal of grasping grinding pain "in that which was down," very much resembling labour; that this had come on in a very short time after taking the first powder, had continued during the night, and been increased after each dose. On making the inquiry, she informed us that the swelling had increased (speaking from her own feelings); but on examination we discovered, to our great satisfaction, that a material diminution had occurred; so much so, that the *rugæ* of the vagina were perfectly manifest; and without any great effort the reduction was effected. We, in the further treatment of this case, acted upon general principles. The woman is now going on remarkably well, so far as prolapsus uteri is concerned, but is still labouring under menorrhagia, for which she is now under treatment.

Should the above circumstances seem to you of sufficient importance to merit a place in an early number of your widely-diffused journal, their publication is at your discretion. The main fact, the obvious action of the ergot of rye, seems to me to be exceedingly interesting; and as I never heard of an exact precedent for the practice which was adopted in the case, and as the remedial agency of the drug we employed is much the subject of discussion at the present moment, I have considered that it would be a dereliction of public duty, had I neglected to communicate the fact to the conductors of some extensively-circulated periodical.—I am, sir,

Your obedient servant,

HENRY W. KER,

One of the Surgeons to the Ardwick and Ancoats Dispensary.

Manchester, July 5, 1834.

ON THE EFFECTS OF OVERDOSES OF ERGOT OF RYE.

By H. MAUNSELL, M.D.

[THE following remarks are appended to an annual report of the Wellesley Female Institution, Dublin, of which the author is physician-accoucheur. The paper is given entire in the Dublin Journal of Med. and Chem. Science.]

Before concluding this report, I am anxious to make a few remarks which, no doubt, must be very imperfect, but which are put forward now as the subject is fresh in my mind, and an opportunity for noticing it may not again occur. I wish to offer to the profession a friendly caution against the *indiscriminate and inordinate* employment of ergot of rye, and in doing so, desire to be understood as in no way retracting the favourable opinion of its prudent use expressed in my former report. What I mean to say at present, is, that extraordinary doses of this drug occasionally produce dangerous effects upon the nervous system, and probably other mischief, and at the same time are less likely to act upon the uterus than smaller quantities.

Mary Redmond, in the fourth month of pregnancy, was seized after a fright and severe exercise with hæmorrhage from the vagina, which continued at intervals for several days. On the 10th March, 1834, at 6, P. M., Dr. Churchill saw her and gave forty-five drops of tincture of opium; at 9, P. M., she was so weak that a plug was introduced. At 7 o'clock on the morning of the 11th, the plug was expelled with considerable hæmorrhage, and Dr. Churchill ordered a grain of opium and two tablespoonsful of acid purgative mixture every second hour. At 8, P. M., of the 11th, hæmorrhage set in again, and Dr. Churchill gave half a drachm of the ergot, and repeated it in half an hour. On the 12th, she had violent headache, and in the course of the day became delirious, and could with difficulty be kept in bed. These symptoms yielded to purgatives, shaving and blistering the head, &c. In a few days afterwards the hæmorrhage returned; ergot was again given, and again followed by delirium, although in a slighter degree.

Mrs. Forrest, two months pregnant, was attacked with hæmorrhage from the vagina on Thursday, the 21st April,

for which she was kept quiet, cold applied to the vulva, and the acid purgative mixture given. On Friday the discharge returned in an alarming degree, and the infusion and substance of half a drachm of ergot was given to her by Dr. Churchill in two doses, with an interval of fifteen minutes. After taking it she fell asleep, and when she awoke the discharge returned; another scruple of ergot was then given, in an hour after which she vomited. On the following day she was in a state of half stupor, with very violent headache, and weak, depressed pulse. Her head being shaved, &c. and purgatives administered, these symptoms gradually gave way.

The occurrence of these two cases attracted my attention to the subject, and upon inquiring among my friends I found that they did not stand alone. Part of the information which I received I shall now briefly recite.

A case was related to me by Dr. Johnson, in which labour of a first child was rendered tedious by the want of uterine action. As the woman had a well-formed pelvis, the head had passed the os uteri, the soft parts were cool and well dilated, and there was no fever, it was thought advisable to give the ergot. Through a mistake of the person in immediate attendance, the infusion and substance of two drachms of powdered ergot was given in two doses. In six hours Dr. J. was again called, and found the patient in a state of incomplete coma, with livid face, and muttering delirium. His first impression was that she had had a convulsion, but finding her pulse reduced to a thread, and only seventy in a minute, he was induced, by his having known similar depression of pulse to follow the administration of ergot, to make further inquiry, and then learned the mistake respecting the double dose. There was no uterine action whatever. She was delivered by the forceps, and remained thirty hours delirious and in a state of partial stupor. She was afterwards threatened with peritonitis, and had a bad recovery. In another case in which ergot was used largely, Dr. Johnson saw complete gangrene of the external parts and death occur six days after labour. In a third he witnessed extensive sloughing of the vagina, without any other probable cause. In neither of these two cases were any instruments used. Attributing such results as these latter to the abuse of ergot may appear to be a mistake of the *post* for the *propter hoc*, but as we know that gangrene is a consequence of its employment as an article of food, they should at least make us cautious. The following extract of a note which I had the pleasure of receiving from Dr. S. Cusack, contains some interesting statements. He says—"Not having notes of the cases in which I used the ergot of rye, I shall briefly state from recollection my experience of it. I was formerly in the habit of giving the infusion and substance of half a drachm, and repeating the same dose at intervals of ten minutes. In one case, supposed at the time to be favourable, the quantity stated above was given three times; the pulse come down from 120 to 90, considerable stupor and epistaxis supervened, but no uterine action whatsoever. The patient was ultimately delivered with the crotchet. In a second case the pulse came down even below the natural standard; a similar tendency to coma existed; and as no uterine action was excited, the child (an acephalous one) was delivered by the forceps. I have since used the ergot in ten grain doses, and I think with more effect."

Upon looking over a short essay upon the use of internal medicines in parturition, by Professor Joerg of Leipzig*, which was sent me lately by my friend Dr. E. Joerg, I find the views referred to above corroborated by the results of experiments with ergot performed upon the stomachs of the Professor himself and several of his pupils, as well as upon animals. When taken in large doses it was always found to produce nausea and vomiting, pains in the abdomen and diarrhœa, weight and pain in the head, and vertigo, with general *malaise*, and depression. Upon animals it produced similar effects, and in the smaller animals, (as doves and cocks,) a comparatively small dose caused death, which was preceeded by coldness and lividity of the extremities, and often accompanied by convulsions. I cannot, however, agree with the learned Professor in condemning its use altogether, nor am I convinced of its inefficacy by the pun in which he affirms that he can see no relation between *mutter-korn* (ergot), and *gebär-mutter* (the womb), except

* "Dass der Gebrauch innerer Reizmittel zur Beförderung der Geburt des Kindes unnothig, fruchtlos, und gesunden Frauen sogar schädlich sei," u. s. w.

the word *mutter*, which is common to both. I shall leave the foregoing facts to speak for themselves; they are not sufficient foundation for a theory, neither is it my wish to erect one upon them; they are merely put forward in the hope that they may act as a drag upon the destructive career into which, I believe, ergot as well as every other new medicine has been forced.

ON

DIVISION OF STRICTURE OF THE RECTUM,

High up in the Gut,

BY A NEW INSTRUMENT.

To the Editor of the Medical Gazette.

SIR,

HAVING recently published in your valuable journal some cases of perforation of stricture of the urethra, I beg to call your attention, as well as that of the profession, to some instruments invented by me, about four or five years ago, for the division of what I have termed the *elastic stricture;* that is to say, a permanent stricture which will admit through it a bougie, but which immediately on its withdrawal, will retract to its former calibre.

These instruments resemble in their structure the lancetted stilettes, excepting that, instead of a lancet being thrust out at the point, a semilunar blade comes out at the extremity on the side, above, below, or laterally, as may be required, and as represented in the engraving. The mode of using them is to pass them through the stricture, and then throw out the blade, and draw the instrument back again through the contracted part until it is divided, when the blade is allowed to retire into its sheath.

This description of instrument is also applicable to the division or slitting of an enlarged middle lobe of the prostate gland, for which they were originally invented,—the division of stricture of the œsophagus,—and the division of stricture of the rectum. To prove its utility in the latter disease, I beg to subjoin a case, and am, sir,

Your obedient, humble servant,

R. A. STAFFORD,

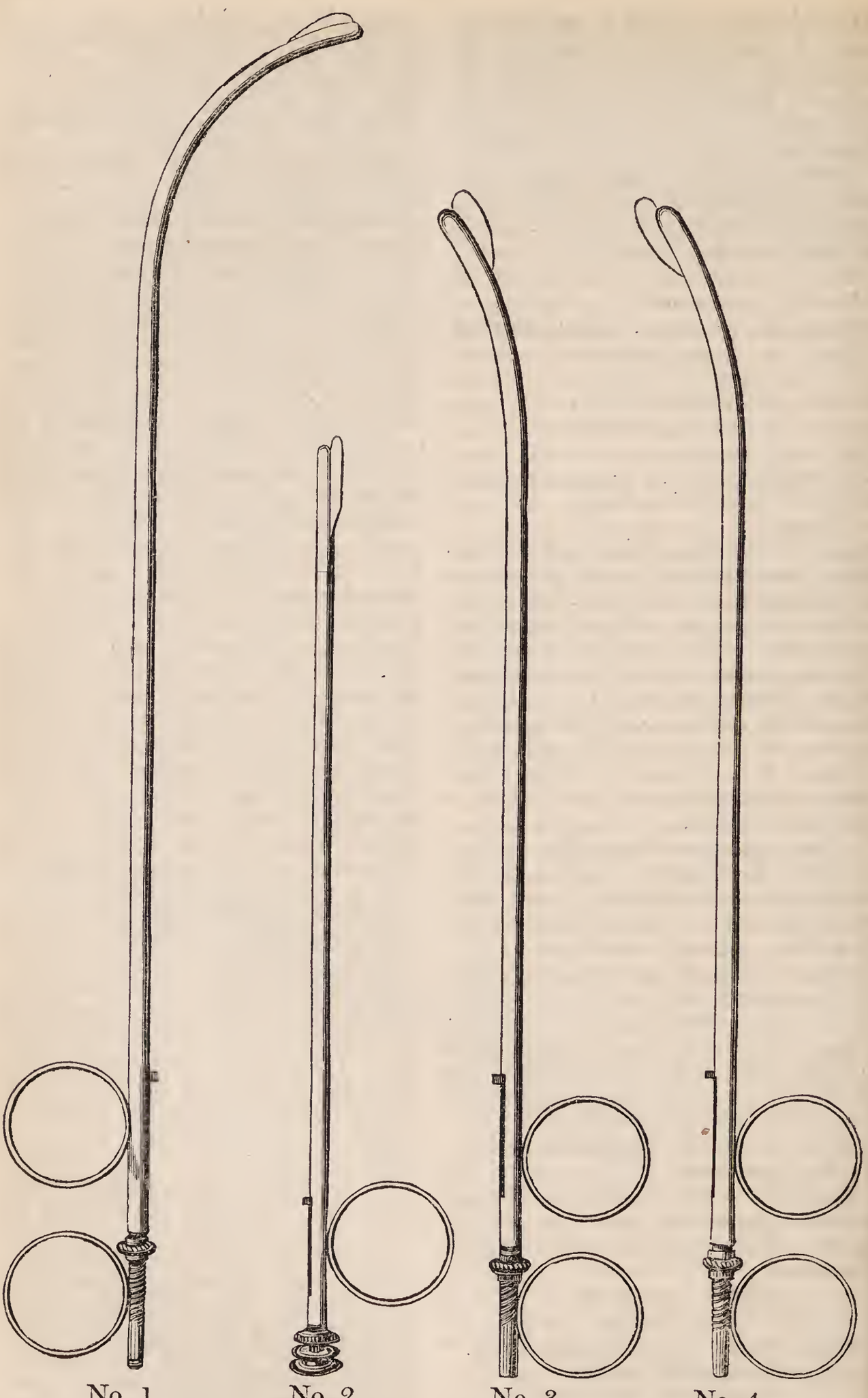
Surgeon to the St. Mary-le-bone Infirmary, and formerly House-Surgeon to St. Bartholomew's Hospital.

28, Old Burlington-Street,
July 21, 1834.

Susan Webb, a delicate, slight-made woman, æt. 31, was admitted into the St. Mary-le-bone Infirmary, Dec. 14, 1831. She states, that about ten years ago she had fistula in ano, which was neglected. A year or two having elapsed, she found she was unable to void her fæces as well as usual, and this difficulty increased until the circumference of the excrement was no larger than the little finger. In the year 1827 she was admitted into St. Bartholomew's Hospital, where bougies and other remedies were employed for three months, when she was discharged, having experienced some relief. From that time, however, her complaint gradually returned; and, as she took no means to prevent it, the stricture became nearly closed up again, and she suffered from it the most distressing symptoms. She was always in pain in the rectum, having a constant inclination to go to stool, without being able to evacuate the contents of the gut. She had frequent tenesmus, accompanied with diarrhœa. There was tenderness in the abdomen on pressure; pain in the back about the region of the sacrum; constant flatulency and sense of fullness; and a continual inclination to vomit. Her pulse was quick (from 120 to 130 in the minute), irritable, and feeble. Her tongue was coated by a brown fur in the centre, and the edges and tip, denoting mucous irritation of the alimentary canal, were of a brilliant red colour. She was greatly emaciated, and so weak that she was obliged to remain in bed.

On examining her, a stricture of the rectum was discovered about two inches and a half from the termination of the gut. It could be distinctly felt, and it gave the sensation to the finger of its being of an indurated structure. It would only admit through it a No. 12 urethral bougie.

Bougies having been passed for more than three months without the least dilitation of the stricture, and other remedies, such as repeated injections, hyoscyamus and opium suppositories, &c. having been employed with no benefit, I proposed, as the symptoms were increasing, and evidently, from the pain and tenderness of the abdomen, chronic peritoneal inflammation was present, that the stricture should be divided. At this time the sufferings of the patient were so great, and the pain and irritation occasioned by the obstruction of the



No. 1. No. 2. No. 3. No. 4.

No. 1.
To divide or slit an enlarged third lobe of the prostate gland.

No. 2.
To divide stricture in the anterior part of the urethra.

No. 3.
To divide strieture of the rectum, or lower part of the urethra.

No. 4.
To divide stricture of the superior or upper part of the urethra.

fæces so distressing, that she was fast sinking. She was constantly on the close-stool, without accomplishing the desired object; she never slept; and was always in the apprehension of immediate death.

1832, Feb. 25th.—An instrument (as in the engraving) about eight inches in length, of a slightly-curved form, and at the side of whose point, on its convex surface, a semilunar blade could be thrust out, was introduced up the rectum, through the stricture. Immediately on having passed the contracted part, the blade was protruded, and drawn back through the stricture, by which it was divided, at that part of the gut immediately in contact with the sacrum. The middle finger could now be easily passed through the contracted part, and a bougie of the same size was introduced and left there for a quarter of an hour. She could not, however, bear it to remain longer, in consequence of the irritation it produced. It was, therefore, withdrawn. The operation caused but little pain, and only a very few drops of blood followed the incision. In the evening she had a motion, which passed with more freedom than for years.

26th.—Had a tolerable night. The fæces had passed freely. She still feels pain in the rectum, as high as where the gut passes over the edge of the sacrum. Has slight tenderness on pressure of the abdomen; pulse 90; tongue red and aphthous in some parts; discharge of pus with the fæces.

Ordered—℞ Liq. Opii Sed. ♏xx.; Mist. Camph. ℥j. M. h. s. s.
Foment. abdom.

27th.—Not worse; if any thing, rather easier.

Contin. foment. Rep. haust.

28th.—Much the same as yesterday. Bowels acted freely.

March 1st.—Still feels pains in the rectum, opposite the upper part of the sacrum. Tenderness on pressure of the abdomen.

Hirud. viij. abdom.
Pulv. Ipec. co. 4tis horis.

2nd. Much relieved; pain nearly gone; tenderness very slight; passed a No. 6 rectum-bougie with great ease; pulse 90; tongue still red; the aphtha diminished. To continue the powders.

3rd and 4th.—Pain quite gone; feels stronger.

5th. — Until to-day, doing well. Bowels confined, not having had a motion for twenty-four hours; pain returned; abdomen tender on pressure.

Haust. Ol. Ric. stat. Hirud. x. abdom.
Foment. abdom.
Vespere—℞ Liq. Opii Sed. ♏xx.; Mist. Camph. ℥j. M. h. s. s.

6th. — Not quite so well; pain increased; bowels freely open.

App. Hirud. vj. abdom.

7th.—Pain in the abdomen lessened; bowels open without difficulty; stool watery, yellow, with some pus; complains of acidity in the stomach.

℞ Magn. Carb. ℈ij.; Dec. Hordei. ℥x. M. statim. s.

8th.—Much better; pain gone; pulse 90; tongue moist, and slightly furred; aphtha gone.

From this time until the 20th she went on well. Her fæces were discharged without difficulty and copiously; the pain of the abdomen was gone; a large bougie was passed with ease, and with but little pain; and the patient felt better in all respects. On the 20th, however, she was seized with erysipelas of the face, which began on the nose, and spread all over the head. In spite of every remedy that could be employed, she went on from bad to worse; and died on the 25th of March, at 1 o'clock in the morning, from the erysipelas.

Examination of the body after death.—Head and face considerably swollen from the erysipelas. The arachnoid membrane was inflamed, and under various parts was effused a serous lymph. The substance of the brain was reddened, with bloody points throughout its whole structure. Effusion of serum in both lateral ventricles, about one ounce in each.

The viscera of the thorax healthy; the liver tuberculated; stomach healthy; adhesions, and a slight blush of red in parts of the peritoneum.

The mucous membrane of the small intestines now tolerably healthy, but evident marks of former disease—inflammation; and here and there the cicatrix of an ulcer which had healed.

Throughout the whole course of the large intestines there were the cicatrices of ulcerations which had healed. In the sigmoid flexure there still existed inflammation of the mucous surface, with some ulcers which had healed, and others which had not.

The gut at the strictured part would admit through it a bougie as large as the thumb. The cicatrix of the wound made by the incision was very perceptible, and it was covered by a membrane analogous to that lining the intestine*.

Remarks.—The dissection in this case proves that had not the patient been attacked by erysipelas of the face and head, she would most probably have recovered. It may be observed that there had been disease throughout the whole course of the mucous membrane both of the small and large intestines. In those of the small, along their whole extent, the ulcers had healed, and the inflammation had subsided. In the large intestine, also, the ulcers had healed, excepting some in the sigmoid flexure. This no doubt arose from the relief given by the division of the stricture. It is more than probable, therefore, the strictured part being so much opened, and consequently the exciting cause being nearly removed, that in progress of time those remaining would have healed also. That such an inference may be drawn is further proved by the pain and tenderness in the abdomen having ceased; showing that the inflammation had diminished, and all the other symptoms abated.

To divide a stricture of the rectum at the termination of the gut, and also the sphincter muscle, is not uncommon; but to divide a stricture so far along the intestine is an operation which has been seldom, if ever, performed. It is not every case which would admit of this treatment, or where, perhaps, it would be desirable. There are those, however, as in the present instance, where the stricture could not be dilated, in which I am convinced it would be attended by the greatest advantage, and be the means of saving the life of the patient. Great caution, of course, must be pursued in the employment of the instrument. Care should be taken not to make too deep an incision, on account of a fear of wounding the internal hæmorrhoid artery or its branches. Such a circumstance could be easily prevented, for the blade of the instrument can be regulated by a screw at its handle: hence the incision made might be of any depth the operator pleased. If, therefore, he chose to cut only the eighth, the fourth, or half of an inch deep, it could be done. The stricture, also, should be divided at the posterior part of the gut, adjoining the sacrum. By operating in this part no injury could be done by a risk of cutting through the coats of the intestine, and thus letting out the fæces. It would be advisable also not to divide a stricture of the rectum beyond that point where the peritoneum covers it, which is about four inches from its termination, and which might possibly give rise to inflammation of that membrane.

Stricture of the œsophagus is comparatively speaking of rare occurrence with other strictures. It happens, however, sufficiently often to point out to the surgeon the small power he has of benefiting the disease. It appears to me that this description of stricture might be divided by an instrument, which can be so well regulated, constructed in a similar manner to those I have now mentioned for the elastic stricture, as I have termed it, of the urethra, and stricture of the rectum; for there is nothing in the anatomy of the œsophagus to prevent such an operation. I have not as yet, however, had a case to confirm my opinion, and therefore do not consider myself warranted to speak positively on the subject.

* The preparation is in the Museum of the College of Surgeons.

MEDICAL GAZETTE.

Saturday, July 26, 1834.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

Cicero.

SOME LATE ACHIEVEMENTS OF QUACKERY.

How very deplorable it must be for the admirers of old English customs to see so many of the national peculiarities daily expiring before their eyes! The spread of knowledge—real or pretended—the more liberal spirit of inquiry, and the more rational experience which prevail, or are said to prevail, of late, play havock with the cherished habits of old times. We were once conspicuous

among the nations of Europe for our prize-fighting, bull-baiting, horse-racing, lotteries, and quackery; but most of these accomplishments and characteristics are no longer peculiar to us: in the last alone have we now any pretensions to be considerable. Notwithstanding the polite patronage and eloquence of Mr. Windham, who argued so successfully, in his day, in favour of old English *sports*, dumb animals have obtained protection from cruelty, while those that speak articulately are by no means honoured with an equal share of indulgence. The glories of bull-baiting and cock-fighting are departed; horse-racing, indeed, maintains a sort of ambiguous reputation, but lotteries are extinct—though they did not expire without a hearty death-struggle. Quackery alone still keeps its ground among us, though not in that high odour which it formerly enjoyed. St. John Long's death has left us trusting to the Morisons, and all those clumsy rogues who fill the columns of the provincial papers; and were it not that we know it has a strong hold in the revenues it pays to government, and in the patronage which it meets with (or did lately) from young Lords and old Dowagers, we should augur ill for the fate of quackery—the last of the heir-looms for which we are so much indebted to the wisdom of our ancestors. In abandoning lotteries, it is true, the government gave up a larger source of revenue than it possesses in the sale of patent medicines; and the favour of lords and ladies is as fickle as the fashions; so we opine there is not much warrant for the brilliant prospects of quackery in these several quarters, after all.

The ignorant empiricism of the present day, besides, is too deadly to be long tolerated. There was a time when a court physician (we cannot help thinking him rather a courtly empiric) could prescribe such articles of materia medica as the balsam of bats—portions of adders, sucking whelps, and earth-worms—the bowels of a mole cut open alive—the mummy of the lungs of a man who died a violent death, &c. &c.; but what was this to the insidious and desperate drugs ventured on by modern nostrum-mongers? St. John Long's lotion was of a nature to kill some patients (as it proved) on whom it was rubbed; while it could be taken into the mouths, or even stomachs, of others with impunity. Morison's pills have dispatched various victims, while they professedly consist only of "vegetable" matter. They consist of various active substances, which, however efficacious when judiciously exhibited, are virulent poisons in their indiscriminate and inordinate use. Every medical man knows this, but the mass of the public know it not; nor would there seem to be any more effectual mode of impressing it on them, than by sad and woful experience—by leaving them to their fate. It is useless to preach to them about the diversity of constitutions, and the difference of diseases in which one and the same nostrum is applied by the empiric: the public will only be taught prudence in such matters by finding the danger at their very doors. The manslaughters and prosecutions of the St. John Long people ought, therefore, to be prominently published whenever they occur; and if this will not serve to warn them, they must be dead indeed to all exercise of reason. But we are satisfied that it has had this effect in more than one instance, of late: the visits to Harley-Street became materially fewer, subsequently to the proceedings in the Old Bailey; and we only hope that the lesson read at York, during the last week, will in like manner not be unattended by its advantages. It is, we presume, pretty generally known, from the public prints, that a vender and dispenser of Morison's pills was convicted

of manslaughter, on Saturday last, at the York assizes; yet a brief abstract of the trial may not be unacceptable to our readers,—if not to refresh their memories, possibly for the purpose of reference hereafter.

George Webb, an innkeeper and dealer in Morison's pills, was put to the bar, charged with the *manslaughter* of Richard Robinson, to whom he had administered large doses of gamboge, aloes, colocynth, and other drugs. The deceased was but twenty years of age, an apprentice to a linen-draper. He was seized with an illness, which proved to be the small-pox, and the prisoner undertook the treatment of him, by administering quantities of Morison's pills, of which he gave, in the course of the illness, as doses, so much as ten, fifteen, and twenty pills, at several times, in one day. The first dose he gave the patient is said to have been twenty of the pills. There were, of course, very copious evacuations immediately; still the pills—the pills—continued to be thrown in,—the prisoner insisting that the patient was doing well, until it being evident to the bystanders that the contrary was the fact, medical aid was sent for, when it was too late—only a couple of hours before the death of the patient. This was on Friday, the 27th of June last, on the eighth or ninth day of the eruption of the small-pox. An inquest was held on the 28th, when a verdict of manslaughter was returned. The only defence the prisoner had to make on the occasion was, that his treatment had been interfered with; he affected to attribute the death of the deceased to the opening of a window on the last day of the illness, to ventilate the small close apartment of the patient.

Mr. West, chemist, of Leeds, was examined at the trial. He had carefully analysed two kinds of Morison's pills, marked No. 1 and No. 2—the remnants of those which had been administered to the patient. The former he found to consist of aloes and colocynth, one grain; gamboge, half a grain; and cream of tartar, three-quarters of a grain; together with a little ginger in each pill. No. 2 were pills of three grains each, consisting of aloes and colocynth, one grain; gamboge, a grain and a half; and cream of tartar, half a grain; with ginger as before. Mr. West was cross-examined at considerable length by the prisoner's counsel, but nothing was elicited to shake his testimony.

The other professional witnesses were two surgeons and two physicians. Mr. James Allen, of York, surgeon, was called in to the deceased on Friday the 27th, two hours before his death. The patient was labouring under confluent small-pox. The body was examined by witness, in presence of Drs. Wake and Balcombe, Mr. Matterson, and others. They found the stomach and bowels much inflamed, the former approaching to gangrene at the cardiac portion. Mr. Allen considered that death had been caused by the small-pox, aggravated by the use of drastic purgatives. These were decidedly improper, as greatly lessening the powers of life, which were already much reduced by the disease. Opening the window had done no injury. Inflammation of the stomach rarely happened in small-pox.

Mr. Matterson, surgeon, deposed that there had been several cases of small-pox at York lately; but none of those that occurred after vaccination, save in the patient's case, had proved fatal. Death was here accelerated by the pills.

The evidence of Dr. Balcombe went to shew, that the probable cause of death in this case was the small-pox, which was aggravated by the pills.

Dr. Baldwin Wake said, that from the appearances which he observed in the body, if he had not known that the

patient had had small-pox, he should have attributed his death to inflammation of the stomach alone. The stomach appeared to have been acted upon by a virulent poison. Gamboge was extremely likely to produce such effects. He had cautioned at different times some of his patients against the use of Morison's pills, as he was aware they contained gamboge.

The mother of the deceased added, that her son had been vaccinated when about a year old.

This closed the case for the prosecution; when the counsel for the prisoner attempted to argue, that there was *no case* to go to the jury, as there was no evidence to shew that there had been any malice on the part of the prisoner, but, on the contrary, that his conduct had been humane, and his treatment well-intentioned. The Lord Chief Baron (Lyndhurst), however, was of a different opinion; he thought it was for the jury to decide whether there had been *manslaughter* in the case or not.

Several witnesses were then called on behalf of the prisoner, to speak to character, and to testify to the virtues of the pills. The son of Morison, and one Moat, a partner in the firm, were examined, and confessed that gamboge did enter into the composition of the pills. His lordship summed up, and the jury, after retiring for a short time, brought in a verdict of *Guilty* *.

Now this we conceive to be a tolerably home case. But how many similar instances of the pernicious effects of quack medicines are there not every day occurring, where the suffering parties will not consent to bring the facts before the public. There is a feeling in the human mind against self-exposure —an antipathy to being held up as the victim of fraud. Lying statements are presented to the eyes of simple and ignorant people in all quarters—but particularly in the columns of the provincial press—all purporting to be accounts of the successful application of certain secret remedies. The readers are easily duped, being incapable of exercising any judgment on the falsehoods set before them, and forgetting that even were these not *fabricated* cases, they must at least have been picked out of hundreds of others, wherein the remedies were either unsuccessful or fatal. The consequence is, that upon feeling some indisposition, real or imaginary, they determine to give the "celebrated pills" a trial—and most likely, in order to justify their imprudence, persevere with them till some serious mischief is done. Should they happen to escape this probable result, they feel bound to speak well of the medicine on which they ventured; or even to ascribe to it virtues which they never knew it to possess: perhaps to recommend it to others, in order to keep themselves in countenance for the hazardous step they had taken. But even though a fatal calamity should be the result, there is generally a reluctance on the part of friends to subject the affair to the cognizance of the public through the medium of a legal investigation; and, unless in very glaring cases, like the preceding, the matter is commonly hushed up.

Thus quackery thrives, and continues to put forth its unblushing falsehoods before the world. Those who could contradict them are silent, from the motives we have assigned; while practitioners, who are perhaps acquainted with damning facts, are also silent—but for a different reason. It is seldom considered in how awkward a position regularly-bred medical men are placed with regard to quacks. Putting out of the question that they have no more privilege or exemption by law than quacks have, they seem to think that

* *Six months' imprisonment* is the *mitigated* sentence since pronounced by his Lordship; for Webb was recommended to mercy by the jury.

proceeding to the exposure of quackery is a course beneath them, as it would place them in that situation in which the police or the common informer should be; while they know that any exposure of quackery proceeding from them would be misconstrued by the mass of the public, as originating in envious or mortified feelings. Occasions, however, do now and then arise, when practitioners cannot suppress their indignation and sense of public justice, One of these instances we have in a case which occurred in Dublin not long since, and in which the same "celebrated pills" did the deed. We will state the chief facts.

"The subject of this case was a gentleman aged about 30. He had caught cold, and sent for Mr. Coppin, a medical practitioner, who found him labouring under the following symptoms: the left side of the face, and the upper lip, were swollen and painful; skin hot and dry; face flushed; tongue white; pulse 132, soft, and small; some thirst. He stated, that previous to Mr. C.'s visit, he had taken *ten of Morison's pills*, in two separate doses.

"A weak solution of the tartarized antimony was prescribed, and a caution given to abstain from taking any more of the 'pills.'

"On the next day the patient was found to be better; but he had several watery stools: he complained chiefly of pain in the hip, which was still much swollen. Six leeches were applied to the hip;—to continue the medicine every second or third hour.

"Mr. Coppin, on the following day, ascertained that the patient had taken five more of the 'Morison's pills:' he again remonstrated with him, and prescribed effervescing draughts, with a pill, every second hour, consisting of a small quantity of blue pill, James's powder, and extract of hyoscyamus. On that evening the patient was worse; he vomited his drink, and passed a watery stool; he complained much of pain, and sickness of stomach; abdomen full, and rather tense. The pills were omitted, and the saline draught continued, though in but half the quantity.

"On the morning of the 8th, Mr. Coppin was preparing to go out, when he received a message, excusing him from further attendance on Mr. ——. *On that day, the patient sent for the agent of this quack medicine*, who saw him, and encouraged him to persevere in the use of the supposed specific. He was assured that the distressing gastric symptoms were the '*proper effects of the remedy;*' that '*the disease would be thus drawn downwards from his head;* and *that by perseverance, he would be soon reinstated.*' As a fit conclusion for this scene of iniquity, *ten pills additional* were administered to the sufferer, by the hands of the agent himself.

"In the course of that day, the unfortunate patient, as it were in a state of desperation, took *thirty-one* of these pills, all his bad symptoms rapidly increasing.

"On the following morning Mr. Coppin again saw him, and found his breathing difficult. He complained of a great pain at the lower part of his chest; pulse feeble and rapid; eyes closed; had been violently purged the whole night. A blister was directed for the belly, and Mr. Coppin requested again that a physician should be called. Dr. Labatt saw the patient for the first time, early in the morning, and found him almost moribund. He was nearly insensible; the extremities cold, and countenance collapsed; the belly swollen, hot, and tender. Some hours after this, he was seen by Drs. Labatt and Stokes in consultation. A degree of reaction had taken place: the patient spoke rationally, and knew all those around him. He declared that *he had taken more than fifty of the pills.* The abdomen was burning hot. The femoral arteries at the groin pulsated strongly; and pressure on the epigastrium produced exquisite pain. Warmth was ordered to the extremities, ice internally, and leeches to be applied to the epigastric region, where he complained of burning pain and heat. The remedies were of no avail; and the patient, who was the sole support of a widowed mother, and large family of sisters, died on the evening of that day.

"His body was examined on the afternoon of the next day.

"Body spare, but muscular; muscles rigid; face pallid, and with a slight tumefaction about the mouth; the integuments of the back externally purple; belly swelled and tympanitic.

"On opening the peritoneal cavity,

the serous membrane appeared of a dark livid hue. The stomach was remarkably contracted, and presented, over the whole of its greater extremity, the most vivid possible scarlet injection, so minute as to totally obliterate all the natural appearance of the mucous membrane. This condition terminated by a well-defined line at the centre of the organ, the remainder of which was a livid slate colour, and the consistence of the whole of the membrane was completely pulpy. The livid colour and softening of the mucous membrane continued through the whole of the small intestines. The cæcum, colon, and rectum, presented a remarkable appearance. In these situations a vast quantity of serous fluid existed, of a brownish colour, and without the smallest portion of fæcal matter. The mucous membrane had totally lost the natural feel and appearance of this tissue, and to the eye and touch greatly resembled a serous membrane. Spleen large and pultaceous; liver small, and apparently healthy; bladder thickened*."

There ought to have been a coroner's inquest on this case. It was either a manslaughter or a suicide: at all events, the unfortunate patient certainly did not die a natural death.

We can see no radical cure for such a state of things, unless it be found in the spread of wholesome information among the several classes of society. There is a disinclination to legislate on the subject,—to prevent people from poisoning themselves after their own fashion: but this surely is not one of those cases in which instinctive wants lead to the procuring of the proper means of supplying them. Among brute animals there are such instincts; some of these, when ill, physic themselves with herbs, which nature indicates for their use; but men have no such instinctive guide; they have reason, if they would but exercise it; if not, (as is usually the case with the multitude), they should be provided for by their rulers, and laws should be made to restrain impostors from playing on the credulity and ignorance of the crowd. Let us not be told that it is impracticable to prevent quackery: a power confided to magistrates would as effectually put down irregular practitioners in medicine and surgery, as it does unlawful dealers in any other kind of business. Patent medicines, bearing the royal arms, and sanctioned by government, should be abolished. It is this which is immediately productive of the mischief; for people cannot be made to recollect, that however serviceable certain pills may possibly be in particular circumstances, they cannot be so in all; and that, as surely as men are endowed with constitutions as diversified as their features and habits, so surely do those quacks lie who offer them one nostrum for all their ailments. Meantime, we think that medical men, and the medical press generally, are quite too squeamish about the exposure of quackery. While the daily prints are filled with lying histories of "*successful* cases," calculated to entrap the grossly ignorant, not a line is inserted to warn the unwary of their danger: the fools are left to grow wise as they may, which they generally do at last, either when too late, or when irreparable injury is already done; and if medical men are called upon to interfere, it is either to repair the havoc of their *rivals*, or perhaps to take off the responsibility from the quack, by being in at the death of *his* patient. Let medical men boldly come forward, and publish all cases in which they find that quack medicine has been productive of mischief. We, for our parts, will lend them all the aid, in this respect, that we can; and we will venture to say, that if, by such means, the malefactors cannot be immediately put in the way of condign punishment, they will be brought, at least, to shame and ignominy at the bar of public

* Abridged from the Dublin Journal, No. XI.

justice, and lose their calling. We earnestly request our correspondents to furnish us with illustrations: it is a duty they owe alike to others and to themselves.

THE UNIVERSAL PILLS.

We can scarcely go into any street in London in which we do not see "Morison's Universal Pills, for the cure of every disease," staring us in large letters in the window of one or more shops. We learn, too, that immense numbers of them are sent not only to every part of this country, but to the Continent, to India, and to America. Now it appears from analysis, that they depend for their medical properties chiefly upon gamboge, a very drastic purgative, long used in medicine, but used by those who understand its action, in very small doses, and with much caution. The gamboge exists in the pills marked No. 2 in the quantity of about a grain and a half; and of these, six, eight, ten, and upwards, are sometimes given at once: thus the patient swallows about fifteen grains of gamboge for a dose!! But besides, each pill contains in addition one grain, which is made up between aloes and coloeynth, two active purgatives, and half a grain of cream of tartar, which is probably added for the purpose of assisting the pulverization, and blending together of the other substances. The pills marked No. 1 contain less gamboge (about a grain), and more cream of tartar; the other constituents being the same. There is also, in both, a trace of ginger. That a mere compound of drastic purgatives should be put forth as capable of curing "every disease," might be deemed the acmé of absurdity, did we not see that the public swallow the imposture and the pills with amazing avidity. And this leads us to remark, that the components are occasionally very imperfectly mixed—probably from large quantities being prepared at a time, and the mass not triturated with sufficient care. We have repeatedly known cases where half a dozen pills have produced no effect, and yet, on taking one or two more, most violent and almost uncontrolable purging has resulted;—a circumstance easily accounted for, by supposing the first dose to have contained little more than cream of tartar and ginger, and the second to have been nearly undiluted gamboge. At this season such doses are eminently calculated to prove injurious; and as the country newspapers too readily admit the "wonderful cures" sent by interested parties, we deem it a duty to caution the public against being deluded by them.

ST. GEORGE'S HOSPITAL.

CLINICAL OBSERVATIONS ON UNUNITED FRACTURE,

By Mr. Brodie.

Mr. Brodie operated to-day (July 16, 1834) upon a case of ununited fracture. The patient, a little boy, of five years old, fractured both bones of the right leg two years ago. The tibia was broken at about its centre, the fibula about an inch below. The case has been treated by continued pressure and setons, and both these methods have failed. Mr. Brodie cut down upon the broken portions of the tibia, removed some soft ligamentous structure, which was found between them, and dressed the incision to the bottom with lint; after which he made the following observations:—

Gentlemen,—I shall not now say all that might be said upon the subject of ununited fracture; because, in the first place, it is one upon which a whole volume might be written; and because, in the next place, a few months since I gave a lecture on these cases, which many of you heard.

This boy fractured his leg two years ago: both bones were broken; the tibia at its centre, the fibula about an inch below. The fracture was treated in the usual way, but did not unite. A surgeon of considerable eminence passed in a seton between the broken ends of the bone. This practice has been of late years very commonly had recourse to. It was first recommended by Dr. Physic, of Philadelphia, and I have tried it in three cases, in two of which it succeeded: but the experience of Dr. Physic would seem to show that this practice rarely succeeds, except in the upper extremity. Amongst the cases in which he adopted it, I have been informed that there was not one case of ununited fracture of the lower extremity in which it did not fail: indeed, I am not aware that there is a single case on record in

which it has succeeded in the lower limb, excepting one which was under my care. The patient was a boy, in this hospital, with an ununited fracture of the thigh. A seton was introduced between the broken ends of the bone, and he recovered. In the present instance, it would be absurd to resort to this plan; since it has, as I just now said, been already tried, and has failed.

There is another method which was, I believe, first suggested by Mr. Amesbury. He recommends you to press together the broken extremities of the bone in opposite directions, so as to keep them constantly and forcibly in contact. Now this plan, which is, at the same time, ingenious and scientific, I have often known to succeed; and I had a patient here last autumn, whose case I then made the subject of a clinical lecture, with whom it succeeded. But in this little boy's case it has been already tried—tried, too, under very favourable circumstances—under the superintendance of Mr. Amesbury himself; but without success.

In former times, it was a common practice to cut down upon the bones engaged in an ununited fracture, and to remove their extremities with a saw: in other words, to make a severe compound fracture. This practice has been, I believe, occasionally successful; but the instances of its success have been very few, compared with those of its failure. I remember conversing with an old surgeon of eminence, who had had frequent opportunities of seeing this practice resorted to, and he told me that he had not himself known a single case in which it had been attended with advantage. But, at any rate, there would be a decided objection to it in this case; for if I were to remove much of the tibia with the saw, the severed ends would not come in contact again, unless I were to perform the same operation on the fibula; and this would be altogether a frightful operation, attended with considerable danger to the patient. What, then, can be done? Sir E. Home was accustomed to mention, in his lectures, delivered in this hospital, a remarkable case which was under the care of Mr. Hunter. A patient had an ununited fracture of the humerus, in which a false joint had formed. Mr. Hunter cut down upon the part, and having introduced a spatula, scraped the fractured ends of the bone; inflammation followed; lymph was effused, and became organized. Bone was deposited in its centre; and in a short time bony anchylosis took place. In this case I have performed a similar operation; and I trust that it may have a similar result.

When a fracture is not united by bone, it generally happens that it is united by ligament. Occasionally a portion of muscle has been found interposed between the broken ends of the bone; but generally they are united by a kind of ligamentous structure: in other words, the constitution is often not strong enough to form new bone, but it is strong enough to form callus or ligament. Sometimes the broken ends of the bone are quite separate from each other, and a complete joint is formed. When this happens, the ends of the bones entering into the fracture are covered over with a ligamentous structure, and are enclosed in a complete capsular ligament, which is lined by a regular synovial membrane. A very interesting dissection of a case of this kind is related by Sir E. Home, in the Transactions of the Society for the Improvement of Medical and Chirurgical Knowledge. Now in this case I thought, from the great mobility of the parts, that there was probably a false joint; but upon cutting down upon the tibia, I found that the two portions of bone were connected by a structure which seemed half ligamentous and half cartilaginous. This substance I cut away, as you saw, with the scalpel, and then scraped the fractured extremities of the bone; after which I dressed the incision down to the very bottom with lint. The lint I shall leave in for three or four days; I shall then remove it, and perhaps put in some more. I shall allow the cavity to fill up with granulations: when this is accomplished, I propose to apply pressure, according to the suggestion of Mr. Amesbury, in order to keep the parts firmly in contact. But it may be asked why I have done nothing for the fibula. In the first place, I think that it is not unlikely that the fibula may unite of itself, if I should succeed in the cure of the tibia. The tibia, if it gets well, will act as a splint constantly applied to the fibula, and will keep it in a state of constant rest. But even if the fractured fibula were not to unite, no great harm or inconvenience would, I apprehend, follow. The principal office of the fibula is to furnish a surface for the attachment of the interosseous ligament and various muscles. It is the tibia which bears the weight of the body; and a leg would be strong enough for walking upon, and for all ordinary purposes, even though there should be a false joint in the fibula. But this is not all. The operation is a very trifling operation for the tibia, but is very far from a trifling operation for the fibula. The tibia is covered anteriorly merely by the integuments, and it is easy to get at once down to the bone, without disturbing any other parts; but the fibula is very differently situated. In order to arrive at it, fascia, and a mass of muscle, must be cut through, and altogether great injury must be inflicted on these various textures, as well as

on nerves and blood-vessels. Abscesses may form—indeed suppuration must be necessary to the cure of such a wound—sinuses may form—matter may burrow amongst the muscles of the leg, and induce a series of symptoms as formidable as those which we often find to attend abscesses burrowing amongst the muscles of the thigh. In fact, such an operation would in all probability produce nothing but one of the worst kinds of compound fracture, and might even terminate fatally. These seem to me to be abundant reasons for not applying to the fibula an operation such as I have performed on the tibia. If the fibula be made the subject of an operation at all, it will be done with more safety after the union of the tibia has been accomplished.

CLINICAL OBSERVATIONS,

By Mr. Cæsar Hawkins,

At St. George's Hospital.

1. *Stricture and Disease of the Kidney.*
2. *Effusion of Urine and Disease of the Kidney.*
3. *Sloughing, with Disease of the Kidney.*
4 & 5. *Two Cases of Lithotomy.*

The true use, gentlemen, of pathology, is not, as many persons seem inclined to believe, the mere knowledge of the alteration of structure which diseased parts undergo, but the connecting these alterations with the symptoms previously observed, so as to enable us to recognise disease in its early stages, before alteration of structure has commenced, or made much progress. It is in this way only that we can hope to make our pathological observations subservient to the cure of our patient. Now I brought under your notice, a few weeks since, some cases of the obscure and interesting diseases to which the kidney is liable*; and as one of these cases has ended fatally within these few days, we will, if you please, terminate our observations of it by a few remarks upon its final result;—I mean the case of John Weighell, with stricture and disease of the kidney. As far as the kidneys were concerned, there was no change since our former lecture upon this subject: the symptoms continued in the same quiet form, and there was no return of suppuration from these organs; but he became gradually pale, and weak, and emaciated, without pain in the back, or other inconvenience referable to the kidney; and about the 20th of June he became further reduced by diarrhœa, with a good deal of pain in the abdomen, which, however, seemed rather to be general than distinctly in the course of the ureters. This returned at intervals, and he became gradually worn out, and died on the 6th of July, having for a day or two complained of intense pain in his head, and having slept a good deal, but without the tendency to stupor, observed on a former occasion.

During this time his urethral complaints remained nearly in the same state, except that the suppuration from the abscess, which I told you I believed to exist in the prostate gland, was lessened; and that about a fortnight before his death the cartilaginous tumour in the perineum a little increased again, though not to the same size as on his first admission into the hospital. During this time I occasionally passed a catheter through the stricture; but for the most part his state of health prevented my trying to do so.

Now let us observe the state of the urinary organs. First, you see this cavity situated chiefly in front of the stricture, in the midst of the perineal tumor; it is now, you observe, a small abscess, communicating with the urethra; but during most of the time it was a small dilatation only of the urethra, and contained a little urine, which could be pressed out of it; and the suppuration was probably during the time it recently enlarged. This cavity never obstructed the bougies, and could readily be avoided. Next you see the broad and firm stricture, dense and white in appearance, and the urethra, turned and twisted at the part. The stricture was, in fact, more on one side than on the other; and you will recollect my showing you sometimes that the end of the instrument had to be moved to the left side before it could be made to go round the point of obstruction—a tolerable-sized catheter used, however, of late, to go through the stricture. Next you see the abscess in the prostate gland, a good deal of the back of which has been absorbed, to form the parietes of the cavity; the abscess may be large enough to contain a table-spoonful of pus, and by its situation in contact with the rectum, may have contributed to the occurrence of the diarrhœa under which he suffered. You see also the opening into the urethra by the side of the verumontanum, in which I told you the point of the catheter was frequently obstructed. Look, in the next place, to the effect upon the bladder of constant straining against the stricture: it is thickened to the extent of above half

* See the present volume of the Medical Gazette, page 426.

an inch, and the large fasciculi of muscular fibres project far into the bladder, the mucous membrane, which is dark-coloured, vascular, and inflamed, being folded into numerous little pouches beneath the network of the inner fibres. Then you observe the ureters, the pelves of the kidneys, and the infundibula, somewhat enlarged, especially the renal pelvis of the left side, and their mucous membrane inflamed. Lastly, you see that kind of alteration of structure in both kidneys which occasioned the symptoms I detailed on a former occasion: the secreting structure in both being firm and condensed, and of a yellow colour; and the left kidney, which is nearly of its natural size, more vascular and brittle than the right, which is diminished to half its natural bulk; and in this part of it, not above a quarter of an inch thick around the dilated infundibula. When speaking to you of this case before, I mentioned the different ways in which pus was secreted in the kidney; and I said, that in consequence of the quantity of blood which preceded the discharge of pus, I rather suspected that it came from a circumscribed abscess. I have looked, however, into this part, and although indented on the surface, and nearly absorbed, I cannot say I see any thing like the remains of an abscess; so that I conclude the pus must have come away in the more usual manner, by secretion from the mucous surface. You might perhaps think that the quantity was too great for this source, but I have often seen many ounces secreted daily from the kidney, as in this case, without any breach of surface. And as it happens that another interesting case, which has ended fatally this week, shows you this fact in a marked degree, I will next bring it before you.

Pollard Tate, aged 33 years, was admitted on Friday, June 27th, in this state. He is a carpenter, of full plethoric habit, and has been subject to stricture, with occasional retention of urine, for the last six years, though he has done nothing for it. On Sunday night, when in bed, he felt a desire to make water, but was unable to do so; only a little dribbling away, with much straining; nor has any water come away since, except by drops. On Monday the penis began to swell; he passed a bad night; and on Tuesday he had several rigors, the swelling now extending to the scrotum. The swelling went on increasing till Friday, the 27th, when he first applied to a medical man, who sent him to the hospital.

On his admission, the penis and scrotum, the perineum, and lower part of the abdomen, were enormously swelled, and of a vivid red colour: he was in very great pain, with some fever, and the bladder was much distended with water.

Now, I told you, some time ago, when speaking of effusion of urine, that I liked, if possible, to pass a catheter into the bladder, and in making the necessary incisions, to let one of them reach the instrument in the urethra, near the usual seat of the stricture. I found, however, here that the catheter would not at first pass, from the tense state of the parts, and from the quantity of sloughs in which the point was obstructed, even near the end of the penis; I therefore made several incisions in the scrotum and penis of considerable depth and length, and another in the perineum, down to the usual situation of the effusion; which last, in fact, gave exit subsequently to some urine, besides that which came through the catheter. I gave him fifty drops of laudanum, and had him placed in a warm bath as soon as the hæmorrhage ceased. Two hours afterwards he was much more comfortable; the swelling was not above half the size it had been previously; and after some little difficulty, the catheter now entered the bladder, and drew off the water which distended it.

When you first see such cases as these, you may think, perhaps, if a stricture is so bad, and produces so serious an impediment to the expulsion of the urine, that the urethra actually gives way behind it; that the introduction of a catheter must be almost impossible; but in practice you will fortunately find that there is seldom much difficulty; for the urine having made its way into the cellular structure, the patient is in great measure relieved from his violent pain and straining, and the spasm of the stricture giving way, it admits an instrument with tolerable facility. This man, for instance, bore a catheter of considerable size, so that but little water passed through the sloughy parts. Probably, indeed, in him the stricture actually sloughed away, at that time, nearly the whole length of the urethra having been destroyed, as you saw at the examination.

This case afforded you an admirable example of the effects of incisions in relieving cellular inflammation, in contrast with the case of Irish, which we considered in the last lecture: in that case the whole of the integuments of the penis, scrotum, and perineum, and even the coats of the the testis, were destroyed by simple cellular inflammation, which had lasted only two days, and in which no opportunity was afforded of making incisions till the parts were almost dead, and yet the examination showed us yesterday that the urethra was perfectly healthy, though the case looked so much like effusion of urine,

when it was first admitted. In this case, on the other hand, notwithstanding the irritation of extensive infiltration of urine, not a single portion of skin died, because the incisions were made in time to save it.

Four or five days after Tate's admission, I took this little calculus, like a grain of coffee in shape and appearance, from one of the incisions on the penis. I look upon this, from his history, as having probably been the consequence of his stricture, and the immediate cause of the effusion, by having been forced out by the bladder against the stricture, and having become so impacted in the already-narrow canal as to block it up completely.

You have watched, I believe, the progress of this case, and saw that it was very favourable for several days, all the sloughs having come away from under the skin of the penis, scrotum, and perineum, the discharge having become healthy, and the sores granulating well; during which time the catheter was kept in the bladder, and the constitutional treatment consisted merely of some occasional purgatives, with saline mixture and ammonia, and of moderately good diet; beef-tea, and so on. In fact, as to the effusion of urine, he had nearly recovered from his danger, and might have recovered but for more serious disease: he had, indeed, besides, some erysipelas, which commenced on the 1st July; but this was mild, and confined to the surface of the skin, and scarcely affected his system, and did not in the least interfere with the cleaning of the sores.

In this case, then, no skin mortified; but, in fact, very extensive destruction, even of the integuments, by effusion of urine, is rapidly recovered from in persons apparently so healthy as this man seemed to be, during the first few days. Indeed, you have only to recollect Irish's case, which I have alluded to, to believe this; for, although more skin was lost than I have seen even in effusion of urine, yet the surface of the testes and spermatic cords, and the other parts around, were healthy; and he, too, might have survived, but for the inflammation, almost amounting to gangrene, which we saw in the mucous membrane of the bowels, and which was indicated even on his admission, by commencing aphthous ulcerations in the mouth, and which was ultimately fatal by diarrhœa.

But, notwithstanding the favourable condition of Tate at first, you saw, on the morning of the 3rd of this month, a sudden change in the expression of his countenance, a complete prostration of strength, and a copious perspiration, with coldness of the hands and feet. This alarming condition continued, and he was scarcely at any time excited by a considerable quantity of wine, and other stimulants, which were given him. On the 6th, our notes still notice the cold clammy sweat, without apparent cause; for the erysipelas, at this time, was scarcely spreading, and was of a better colour, and the local state did not account for it, the wounds being clean, and the skin quite healthy.

To what, then, was this owing? Suppuration somewhere was evident; but where was it? I spoke to you, at one time, of the possibility of the effusion having produced inflammation in the pelvis; but, in general, the inflammation is bounded by the deep fascia of the pelvis, on the outside of which the effusion invariably takes place; he had had no pain about the sacrum, or above the pubes; and this cause did not seem probable, or was there absorption of pus, or other diseased secretions, so as to occasion those secondary abscesses which are but too common in surgical practice. This, on the whole, seemed the most probable supposition; but if so, no local deposit yet made its appearance. On the 6th, indeed, he had slight diarrhœa, and a little tenderness of the abdomen; and on the day of his death pain was complained of in one wrist-joint. He sunk, however, on the 8th July, and the state of the kidneys afforded sufficient ground for his previous condition, though no complaint was made, which could lead to a knowledge of their condition, while he was in the hospital. The bladder was nearly in the same state as Weighell's, much thickened, and reticulated, and inflamed, and contained a good deal of thick bloody urine. The ureters were very much dilated and inflamed, and both of them were twisted and obstructed near the kidney, as you may see, by other preparations in the museum, is often the case in stricture. The pelves of the kidneys, and infundibula, were much more distended than in Weighell; in a high state of inflammation; and the kidneys themselves were coated with thick yellow lymph; and all these parts were on both sides full of a dark, bloody, purulent secretion, quite pulpy in consistence, and which had passed down in great quantity into the bladder. The structure of both organs was inflamed, but not yet condensed or diminished in bulk, as in Weighell. In the right kidney, where most lymph had been deposited, one or two of the infundibula looked like distinct abscesses. Subsequent examination, however, shewed that this was not the case, the cavities all communicating with the pelvis of the kidney by continuous mucous surface.

So many ounces of purulent fluid being thus evidently formed by the mucous surface in this case, it is not surprising that

we found no abscess in the kidney in Weighell. I hope, too, you attended to the appearance of the secretion, as it is exactly like that which has come away during the several attacks of renal disease that another patient, Colston, has had, though in him not yet fatal; that is to say, there is a quantity of blood with the pus, coming away not in small coagula, as it did in some measure in Weighell, but as a sanguineous secretion, without rupture of vessels.

This patient, then, died of renal disease, produced by his neglected stricture; and it shews you the extent to which renal disease may go on without producing complaint; this patient being a remarkably stout healthy-looking person, who had never resorted to medical aid till several days after effusion of urine had taken place; and further, you have seen the sudden and fatal aggravation of the renal disease, notwithstanding its previous slow and insidious progress. Such, too, is often the cause of death after the operation of lithotomy, especially in old persons, the stone producing the same disease of the kidney as in this person was occasioned by the stricture; and just in the same way they suddenly sink, after apparently going on well for a few days; such, too, is the condition occasionally, when you think, during its treatment, that you have nearly cured a stricture.

You will take notice, also, that this is one of the dangers to which patients are equally exposed after the new operation of lithotrity, as well as after the old method of extracting stone. In fact, I cannot help believing that the danger following the operation of breaking down stones in the bladder is as much too lightly regarded as those of lithotomy have been exaggerated in the opinions both of medical men and of the public. It is well known that, in this country at least, a great degree of selection has been observed in the cases in which lithotrity has been to any extent performed, while we, on the other hand, are often obliged to perform the operations of lithotomy against our judgment—in unfavourable as well as favourable cases—in those even in which the other operation has been declined. Tables have formerly been published of the result of many cases of lithrotrity, whence, without so much selection, it would seem that the mortality has been as high as after lithotomy; and even with all the present improvements in the method employed, so many fatal cases have occurred, even in the most experienced hands, that I much doubt whether, in old cases of stone, where there is any suspicion of disease of the kidney, or other serious disorganization, it would not be less dangerous to have the operation of lithotomy performed than that of lithotrity. I am not alluding to a case which you saw last year in this hospital, under Baron Heurteloup, because the occurrence of sanguineous apoplexy, which was there fatal, might have occurred after any operation, and did not detract from the value of the operation; but I am alluding to several other well known cases. Nor, in fact, is it at all surprising that there should be sometimes considerable irritation, when we consider that the stone itself, or its numerous fragments, remain behind to excite irritation, in addition to that produced by perhaps several successive operations with the instruments. The fact is, that if you take away the two dangers—1st, of disease and disorganization of the bladder and kidneys, which is equal in the two cases; and, 2dly, of large stones, by the extraction of which laceration of the soft parts about the neck of the bladder is necessarily occasioned, (but the breaking down of which must also be more or less tedious and hazardous,) then I am satisfied that the operation of lithotomy is by no means so dangerous as is commonly imagined, provided the operation has been so performed as to leave a free and depending wound of the external parts for the urine to escape, and a small wound of the internal parts connected with the prostate.

How trivial the operation is, under these circumstances, you can see, I hope, in two little boys lying in the next beds to one another, on whom I have performed the operation of lithotomy. The first of these, George Stone, a little boy between five and six, admitted on the 4th of last month, had only slight symptoms of stone, though they had commenced before he was a year old. I removed the stone on the 19th of last month, and you saw that he had not even the slightest fever, at least I did not myself observe the least acceleration of pulse. On the 26th, a week afterwards, our notes tell us that some urine began to come through the urethra; and since the 2d July, the twelfth or thirteenth day after the operation, not a drop has come through the wound. July 6th, the wound was almost well; and it would probably have to-day been quite so, but that he is in such spirits as to have rather irritated it, by not being able to keep his legs quiet. I may observe, with regard to the stone, that it had very much the appearance of being partly in a cyst, for it could only be felt in sounding by the point of the instrument turned towards the pubes; and it was quite fixed in this situation during the operation, from which I had to dislodge it, with some little difficulty, by the finger turned quite up above the pubes; and you can see how peculiar the shape of

the stone is, as if this narrow part may have been confined. I do not mean positively to assert that it was so; but if not, it must have been very tightly held by the contracted bladder, both in sounding and in the extraction.

The other case is also a little boy, of about six (James Elmes), admitted on the 2d July. As the operation, however, was only performed yesterday, the 10th, I cannot assert that he is in no danger; but I have no doubt that he will do as well as the other child, having at present not the smallest sign of any bad symptom: indeed he has passed a better night, and seems actually better than before he was operated on *. Nor is it wonderful (on the supposition that the operation of lithotomy is in *itself* not a dangerous one) that he should actually be in a better condition; for notwithstanding the difficulty there has been in detecting the stone, his sufferings from it have been very severe, as, in addition to the usual pain in the penis and bladder, and the occasional stoppage of the water while he is making it, he has had so much irritation as to have experienced several attacks of complete retention of urine, which is by no means a common symptom of stone, requiring the use of the catheter. On his admission, what is more usual, he was unable to retain it a moment after the pain came on; indeed it constantly dribbled away, so that I have scarcely seen any of it, in order to examine it. He was in constant suffering, and had prolapsus ani to a great degree, in consequence of the bowels always acting violently when the pain in the bladder was severe.

This boy was in the hospital in May of last year, when I was unable to detect the stone, and, having scarlet fever, he was taken home again; nor was it perceived either before that time, nor since, although a sound or catheter has often been used by different persons. On sounding him a few days ago, however, the stone was immediately perceived, but you observed, when he was brought into the theatre, that my colleagues could not immediately detect its presence. If you examine the stone, you will perceive the probable reason of this uncertainty; and why, when it was perceived, although as large as a nutmeg, it seemed much smaller than it really was. This was because it is soft and rough on the outside, from an exterior layer, apparently of triple phosphate of ammonia and magnesia, and because it is very light in proportion to its diameter; consequently it neither rung when struck, as a firmer stone does, nor appeared so large when poised in the concave side of the sound; for this stone also, as is often the case in children, was not perceived towards the back of the bladder, but only when the sound was drawn towards the pubes.

You had an opportunity, during the operation, of seeing the curious extent to which intus-susception, or prolapsus of the rectum, may take place, in consequence of the irritation of a stone upon the rectum; for I have seldom seen so large a descent even in an adult. The boy held his breath most resolutely the moment the external incision was begun, so that the bowel was forced down, and, as you saw, it almost baffled the attempts that my colleague made to reduce it and keep it up, and it required some caution not to wound it in completing the incision into the membranous part of the urethra; as it formed, after the reduction, a large tumor within the outlet of the pelvis, which completely filled the space between the sides of the innominata, obliging me to watch for one or two periods, when he was renewing his breath, to be able to push aside the great mass of the rectum, in order to cut with safety. Afterwards, of course, it did not signify, as the finger was then able to depress the bowel out of the way of the knife.

By the by, in speaking of the two operations of lithotomy and breaking down the stone, I had, of course, no choice in either of these two cases; as, on account of the size of the urethra at this early age, it is not safe to trust to the temper of any instruments small enough to enter the bladder for the purpose of breaking the stone. In fact, now that the construction of the necessary instruments has been so much simplified as to make them nearly perfect, and to render the operation of breaking infinitely less dangerous than on its first introduction, and comparatively easy of execution by any one, the great thing seems to be to establish some general rule of distinction as to the classes of cases in which lithotomy should be performed, or in which the old operation of lithotomy is either necessary or preferable to the other.

As I have alluded to the singular case of Irish, in which so much destruction of parts took place from cellular inflammation, without any effusion of urine, I have placed on the table, for your inspection, the bladder and kidneys. You will see the bladder inflamed, and with projections of mucous membrane slightly ulcerated and covered with sabulous matter, though he does not seem to have made any complaint, at any time, of any vesical malady; and in the kidney you may observe numerous small bodies, like cartilage almost in consistence, and of the size of a pea. The function of the organs was probably not interfered with; at least I cannot connect

* July 22d.—Since this lecture was delivered this boy has gone on quite well, and most of the water already passes by the urethra.

them with any particular functional derangement in the account given me of his previous condition. As we have been speaking, however, of renal diseases, you may examine their structure, if you please, simply as a fact in pathology.

ANNIVERSARY MEETING OF THE PROVINCIAL MEDICAL ASSOCIATION.

To the Editor of the Medical Gazette.

SIR,

I AM happy to say that the meeting of the Provincial Medical Association, on Friday last, in this place, fully equalled the most sanguine expectations of its most ardent friends. At the general meeting, upwards of three hundred persons were present. It was, indeed, a pleasing sight to see so large a number of men, many of whom had travelled great distances, thus met together for the advancement of our science. Amongst the members present were to be found the most eminent practitioners in the provinces,—men who, by their talents, had long earned the highest reputation both with their brethren and the public. Where so many men of talent are congregated, it is difficult to particularize; but I noticed with peculiar satisfaction the venerable Sir Chas. Throckmortion, Bart. M.D. and Joseph Crane, Esq. the fathers of the profession. The learned Professor of Medicine in the University of Oxford, Dr. Kidd, was present, and unanimously elected president for the ensuing year. Dr. Carrick, of Bristol, who so ably performed the duties of president last year, again joined us, and though upwards of 70 years of age, presented the happy picture of a powerful mind in full vigour, combined with an activity of body which might almost be called youthful. There were also present Dr. Davies, Dr. Riley, Mr. Hetling, and Mr. Fripp, of Bristol; Dr. Barlow and Mr. Soden, of Bath; Dr. Jeffreys and Mr. Dawson, of Liverpool; Dr. Bardsley, and Messrs. Ransom, Wilson, and Turner, of Manchester; Dr. Jones, of Denbigh; Mr. Griffiths, of Hereford; Dr. Forbes, of Chichester; Dr. Stevens, of Ely; Drs. Hastings, Malden, and Streeten, and Messrs. Sheppard and Hebb, of Worcester; Drs. Conolly and Wake, of Warwick; and Dr. Luard and Mr. Jennings, of Leamington.

The Annual Retrospective Address, delivered by Dr. Conolly, was the most eloquent I ever heard. He considered it the object of this address to mark the circumstances which particularly characterized the present period, and which he conceived to be the great ardour evinced in the pursuit of all sciences calculated to increase the happiness or diminish the sufferings of mankind. In advancing these objects, our own science stood pre-eminent, and the ardour with which anatomy, pathology, medicine, and surgery, were cultivated, he proved by pointing out the advances they had recently made. His biographical sketches were very interesting; but as the address will be published immediately, I shall say no more on this subject.

Dr. Turner presented his report on Anatomy. He took a cursory view of the different epochs which marked the progress of the science, but dwelt more especially on its rapid improvement during the last fifty years. On the subject of general anatomy, he entered more at length, and gave a classification of the tissues, which appeared to please most present.

The report on the Chemical Pathology of the Blood, prepared by Mr. Jennings, of Leamington, was read by Dr. Streeten, of Worcester, in consequence of the illness of the former gentleman. It contained a large collection of facts connected with the altered character of the blood in different diseases; some collected from the observations of previous writers, others the results of his own analyses. This appears to be a subject full of interest, and requiring the continued exertions of many inquirers before we can hope to be masters of it.

Mr. Hebb, of Worcester, read an interesting communication from our foreign correspondent at Amsterdam, Dr. Nieuwenhuys.

The next meeting will take place at OXFORD; DR. KIDD, *President*. Under such auspices a most brilliant meeting may be considered as insured.—I am, sir,

Your obedient servant,

AN ASSOCIATE.

Birmingham, July 23, 1834.

ROYAL COLLEGE OF SURGEONS IN LONDON.

REGULATIONS OF THE COUNCIL RESPECTING THE PROFESSIONAL EDUCATION OF CANDIDATES FOR THE DIPLOMA.

I. Candidates will be required to bring proof:

1. Of being twenty-two years of age.

2. Of having been engaged five years in the acquirement of professional knowledge.

3. Of having studied Anatomy and Physiology, by attendance on Lectures and Demonstrations, and by Dissections, during two anatomical seasons*.

4. Of having attended at least two courses of Lectures on Surgery, delivered in two distinct periods or seasons, each course to comprise not less than sixty Lectures.

* An anatomical season is understood to extend from October to April, inclusive, and to comprise at least 140 Lectures on Anatomy and Physiology,

5. Of having attended lectures on the Practice of Physic, on Chemistry and on Midwifery, during six months; comprising not less than sixty Lectures respectively, and on Botany and Materia Medica during three months.

6. Of having attended, during twelve months, the surgical practice of a recognized hospital in London, Dublin, Edinburgh, Glasgow, or Aberdeen; or for six months in any one of such hospitals, and twelve months in any recognised provincial hospital.

II. Members and Licentiates in Surgery of any legally constituted College of Surgeons in the United Kingdom, and Graduates in Surgery of any University requiring residence to obtain Degrees, will be admitted for examination on producing their Diploma, License, or Degree, together with proofs of being twenty-two years of age, and of having been occupied five years in the acquirement of professional knowledge.

N.B.—Certificates will not be recognized from any hospital unless the surgeons thereto, or a majority of them, be members of one of the legally constituted Colleges of Surgeons in the United Kingdom; nor from any school of Anatomy, Physiology, Surgery, or Midwifery, unless the respective teachers be members of some legally constituted College of Physicians or Surgeons in the United Kingdom.

Certificates will not be received on more than two branches of science from one and the same Lecturer, but Anatomy and Physiology, Demonstrations and Dissections, Materia Medica and Botany, will be respectively considered as one branch of Science.

In the Certificates of attendance on Hospital Practice, and on Lectures, the dates of commencement and termination are to be inserted in words at full length.

All the required Certificates are to be delivered at the College ten days before the Candidate can be admitted to Examination.

(By Order of the Council),

EDMUND BELFOUR, *Sec.*

July 10, 1834.

CIRCULAR TO LECTURERS.

SIR,—In consequence of the occasional irregularities that have taken place in the certificates, transmitted to the Court of Examiners of this College, I am directed by the Council to acquaint you, that it is their anxious wish to have some plan devised and adopted by the lecturers at the various medical schools of the United Kingdom, whereby the regular attendance of the students on the lectures at such schools may be enforced and registered, so as to entitle the students to receive, and to justify the lecturers in giving, certificates of attendance, the accuracy of which may be relied upon by this College, and which the greatest vigilance and circumspection on the part of the lecturers cannot at all times secure under the present system, where there is a want of some such check and registry.

The Council will consider themselves obliged by your attention to their wishes, and will thankfully receive any suggestions you may be pleased to offer on the means which, in the opinion of yourself and your colleagues, may be best adapted to effect this important object.

I have the honour to be, sir,

Your most obedient humble servant,

EDMUND BELFOUR, *Sec.*

July 2, 1834.

occupying not less than one hour each, given on separate days; and at least 100 Demonstrations of the like duration, given in a similar manner; exclusive of Dissections, of which distinct Certificates are required.

METEOROLOGICAL JOURNAL.

July, 1834.	THERMOMETER.	BAROMETER.
Thursday . 17	from 56 to 87	30·16 to 30·09
Friday . . 18	60 75	29·84 29·79
Saturday . 19	50 59	29·61 29·50
Sunday . . 20	49 65	29·83 29·58
Monday . . 21	51 65	29·66 29·72
Tuesday . . 22	53 71	29·81 29·91
Wednesday 23	51 76	29·94 29·97

Wind variable, S.W. prevailing.

Except the 17th and 23rd, cloudy, with frequent rain; a thunder-storm in the evening of the 18th.

Rain fallen, 2 inches and ·2 of an inch.

The wheat, which every where promises abundance, has in some places been cut; and no doubt, but for the rain since the 17th, the harvest would ere this have generally commenced.

The thermometer rose on the 17th to a height not attained since June 28th, 1826, at which time it was 89.

CHARLES HENRY ADAMS.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *July* 22, 1834.

Abscess	3	Hernia	1
Age and Debility	36	Hooping-Cough	14
Apoplexy	7	Inflammation	26
Asthma	15	Bowels & Stomach	9
Cancer	2	Brain	4
Childbirth	5	Lungs and Pleura	12
Cholera	11	Insanity	5
Consumption	76	Liver, diseased	6
Constipation of the Bowels	1	Measles	7
Convulsions	46	Mortification	5
Croup	2	Paralysis	1
Dentition or Teething	12	Rheumatism	1
Dropsy	14	Scrofula	1
Dropsy on the Brain	14	Small-Pox	9
Epilepsy	2	Sore Throat and Quinsey	1
Erysipelas	2	Spasms	3
Fever	9	Stone and Gravel	1
Fever, Scarlet	9	Thrush	4
Fever, Typhus	4	Unknown Causes	10
Gout	1		
Hæmorrhage	1	Stillborn	29

Increase of Burials, as compared with the preceding week . . . 97

W. WILSON, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, AUGUST 2, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XLIII.

COMPLEX LABOURS.

We enter to-day on the consideration of the last class of labours—*complex*, which embraces nine orders; not, however, included under one head because of any analogy that they bear to each other, but merely to prevent the embarrassment which might arise from a multiplication of separate and distinct classes.

These are—*first*, labours complicated with hæmorrhage; *secondly*, with convulsions; *thirdly*, with ruptured uterus; *fourthly*, ruptured bladder; *fifthly*, syncope, independently of hæmorrhage, or any extensive laceration or other lesion; *sixthly*, descent of the funis by the side of the head, or breech; *seventhly*, the descent of the hand by the head or breech; *eighthly*, labours in which monsters are brought forth; and *ninthly*, in which there is a plurality of children.

HÆMORRHAGE DURING LABOUR.

Hæmorrhage is by far the most frequent source of danger to a lying-in woman. We have already spoken of it as occurring under some deranged states of uterus, where no organic disease existed; and we shall hereafter have an opportunity of considering it under pregnancy—as accompanying abortion—and as one of the most common symptoms of change in structure. We shall now confine our remarks to hæmorrhage during labour: and since the occurrence is so frequent, and the result so dangerous, this accident calls for the most anxious and serious attention.

Dependent on a partial detachment of the placenta.—We must remember that all alarming hæmorrhages during parturition, and towards the close of gestation, are to be regarded as originating in a partial detachment of the placenta from the uterine surface, and the consequent opening of a certain number of vascular orifices. For although some physiologists have supposed that the vessels which communicate with the decidua might, when a portion of that membrane became loosened from its uterine connexion, furnish a sufficient quantity of blood to constitute an alarming discharge, I cannot think but that they have overrated the consequence of such an accident; and that, during the latter half of pregnancy at least, we should attribute all dangerous floodings to a partial separation of the placental mass itself.

It is a common observation, that the earlier in gestation hæmorrhage occurs, the less danger does it generally bring with it; so that either towards the full period of pregnancy, or during the progress of labour, an attack of flooding is by far more frequently fatal than in the more early weeks. This is owing, as you well know, to the enormous size which the vessels have acquired towards the close of pregnancy. We have seen in the uterus at different periods, that, as it enlarges in bulk, and its cavity increases in extent, the blood-vessels also undergo a gradual dilatation in their calibre, and that, towards the close of gestation, the arteries have acquired a capacity sufficient to admit the barrel of a goose-quill without any difficulty, and the veins a cylinder of even larger diameter. I have myself, upon more than one occasion, when removing an adherent placenta from the uterus, accidentally and unintentionally passed the extremity of my finger within one of these apertures; so that you can imagine what an immense quantity of

blood may be instantaneously poured out from such vessels when opened.

Passive in character.—It must also be borne in mind, that hæmorrhage from the uterus, under labour, should be viewed as of a passive character; that the blood escapes, not as a consequence of any forcible rupture, produced by the excessive action of the heart and arterial system,—as is generally the case in hæmoptysis,—but merely by being allowed to exude through orifices rendered patulous by the separation of a substance which had previously closed them; and that our treatment must therefore necessarily be directed towards preserving within the body as much of the vital fluid as possible, if the flow be immoderate.

Effect on the system to be regarded more than quantity.—Although a large loss of blood is always greatly to be dreaded, yet in practice we do not so much regard the quantity that flows, as the effect which the loss produces upon the constitution of the patient; because we find that different women vary very remarkably in their capabilities of bearing up against the results of hæmorrhage. It is astonishing how slight a degree of depression will follow an excessive flooding in some women; and how small a discharge, comparatively, will destroy others. I have known two women die from the eruption of scarcely a pint of blood; and I have seen others recover perfectly when they have suffered the loss of some quarts; so that the quantity which would constitute a dangerous hæmorrhage in one constitution, would in another not even produce alarm.

Besides the quantity of blood lost, the danger to the patient depends also on the celerity with which it flows. If a pint escape at one gush, it is usually followed by a state of faintness, and perhaps complete syncope; but a slow draining may go on for a considerable time, until, indeed, in the whole, some quarts may have oozed away, with but little constitutional disturbance; and this difference may depend on two circumstances:—In the first place, the arteries throughout the entire body, by the power of contraction inherent in their structures, accommodate themselves in diameter to the decreased quantity of their contents; and this diminution in calibre they have an opportunity of effecting when the blood flows slowly, but not when it passes out with greater rapidity: and secondly, at the same time that the discharge is going on, fresh blood is also formed by the assimilation of nourishment; and thus the deficiency is in part at least supplied, and a more equal balance is kept up.

But although the immediate effect on the constitution is not so great, still we must look with much anxiety on these continual drainings, for they will in time undermine the most robust habit; and I have remarked, that women usually recover better when a small quantity has suddenly gushed forth in one eruption, than when they have lost a larger quantity more slowly; although, in the latter case, they may have experienced but little comparative distress at the time the blood was flowing. Dropsies, purgings, affections of the chest, and organic diseases of the abdominal viscera, have more frequently followed a draining continued for a length of time, than one sudden gush, notwithstanding that the violence of the shock may have produced a state of syncope that was alarming at the moment. The danger then will be partly produced by the quantity lost, partly by the celerity with which it flows, and must be estimated by the effect on the constitution.

Seldom continues till death.—When a woman floods, it is very seldom that the discharge will continue with the same impetuosity until death supervenes.—I use the expressive term "flooding," understood equally by the female as the medical world, as descriptive of the violence of the eruption. It often, in truth, resembles a deluge, and has been fancifully compared to the sudden bursting of the flood-gates of a river.—It is not usual, then, for the hæmorrhage to proceed uninterrupted until the last vital gasp is drawn; but the patient faints and rallies, and faints again: and at length a perfect syncope will paralyse the senses, deaden the nervous energy, and put a stop for ever to the action of the heart. Occasionally, indeed, one tremendous burst takes place, which so completely depresses the system, that a mortal faint at once occurs. The heart and sanguiferous vessels become so rapidly emptied, that they possess no longer the power of contracting upon their diminished contents, so as to propel them onwards; and thus, after making some vain and futile efforts to keep up the circulation, their action entirely ceases, never to be restored; though this is comparatively rare. Sometimes, again, a slow draining will go on for a length of time, the faintness increasing with the loss of blood, the heart's action never being perfectly suspended during the continuance of the discharge, and the first attack of syncope will be the last.

Means adopted by nature to arrest hæmorrhage.—Since, then, bleedings seldom continue uninterruptedly until death takes place, it is clear that a process is established by nature for the purpose of subduing hæmorrhage. When an artery is

wounded, there are three principal ways by which nature has been supposed to endeavour to put a stop to the immediate flow of blood; and a fourth, by which she renders the safety of the patient permanent. The first philosophical attempt to explain nature's mode of proceeding in suppressing hæmorrhage from divided arteries, was given to the world in 1731 by M. Petit, who accounted for it on the principle of a coagulum formed around, and at the extremity of the bleeding vessel, extending to a considerable distance within the cavity of the canal, lying partly within, and partly externally; and thus offering a barrier to the free flow of blood. This opinion was in a few years attacked by Morand, who was afterwards followed by Poutcau, Kirkland, Gooch, and others.—These physiologists insisted that the chief cause consisted in a change which the artery itself undergoes; that change was described as being a corrugation, or plaiting, of the circular fibres of the vessel, by which its calibre is directly diminished, together with a shortening, a corresponding thickening of its longitudinal fibres, and a retraction of the open mouth, which all indirectly assist in contracting its canal. The third opinion was that of the late Mr. John Bell, who asserts, that "when hæmorrhage stops of its own accord, it is neither from the retraction of the artery, nor the constriction of its fibres, nor the formation of clots, but by the cellular substance which surrounds the artery being injected with blood;" and he supposed the pressure thus occasioned to be the cause of the suppression of the bleeding.

More extended observation has taught us that each of these means contributes its due share towards the object which nature has in view; and we now consider that the flow is temporarily restrained partly by the extremity of the vessel contracting, partly by its retraction within the surrounding cellular substance, partly by blood effused into that cellular substance, and partly by a clot formed at its extremity, and passing to a considerable distance within its canal. But it must be evident that such slender safeguards, even when acting under the most favourable circumstances, can only exert an influence for a limited time, and that there must be great danger of renewed bleeding on the application of many trifling exciting causes. Nature, then, not content with the insufficient security obtained through these means, has instituted another process, by which the perviousness of the canal is permanently destroyed. The divided extremity of the artery inflames, its *vasa propria* pour out lymph, which, adhering to the internal coat of the vessel, fills up the cavity, and eventually obliterates the canal. Its coats also become thickened by a similar process—namely, a deposition of lymph within their structure; by which two conjoint actions the vessel is converted, in process of time, into a ligamentous cord; and this change is usually observed to have taken place as high as the first lateral branch given off above the seat of injury.

It is an established principle of improved surgery, in cases where an artery is pricked, and cannot be secured, to divide it completely across; by which the best chance is afforded it of diminishing its capacity, of burying itself within the surrounding cellular structure, and of becoming plugged at its extremity by the formation of a clot.

From observing these wonderful expedients adopted by nature, and the changes she has instituted, modern art has derived a most valuable lesson, and the surgeon now throws a ligature around the sides of the canal, which at once renders it instantly impervious, in the same manner that nature attempts through the medium of a coagulum of blood; and the process of adhesion and consolidation afterwards advance in even a more rapidly progressive manner.

To apply this to our present purpose, let us examine if there are any means analogous to those just spoken of, by which nature or art can restrain hæmorrhage from the uterus under labour. If the structure of the uterus be such as I have demonstrated it, and especially if the vessels follow their tortuous course as I have described them, not terminating in open mouths, but having circular or oval apertures at their sides—and if these vessels are most, if not all, of them veins—it is impossible that they should be able to retract and bury themselves within the surrounding parts; they might, indeed, be compared to the punctured vessels of a limb. Even if the arteries terminated as we have hitherto always been taught, in open mouths, there is so small a quantity of loose cellular substance in the uterine structure, among which they could bury themselves, that we can hardly expect retraction to assist us in this emergency; and we therefore lay it down as a principle that this expedient will not serve us. I am not prepared to say that coagulation does not occasionally take place at the vascular apertures described; on the contrary, indeed, I have had frequent proofs that it does, and that it is some safeguard against the continuance of hæmorrhage; but still it is a very poor one, and one upon which we are not warranted in relying, provided there are any other means which we can have recourse to. Nor am I exactly prepared to say that consolidation of the vessels by

coagulating lymph, might not perhaps occur; but we should not *à priori* expect it, because the occasion of its deposition, in a divided vessel, is the inflammatory state set up as a consequence of injury. In the separation of the placenta from its uterine attachment, however, there is no solution of the continuity of the vessel itself—there is no injury sustained by it—and we cannot, therefore, with any shew of reason, anticipate the occurrence of adhesive inflammation. An argument against such a supposition will also immediately offer itself to every practical man; for when a woman has once suffered an attack of hæmorrhage towards the close of pregnancy, she will very often, even on the slightest exertion, become the subject of an eruption of blood, during the remainder of gestation, which could not happen if a process of consolidation were going on in the vessels, similar to what takes place when an artery is divided. I know, indeed, that the reverse of this is sometimes observed—that hæmorrhage under pregnancy, although it may return upon more than one occasion, may also be permanently stopped, and that by a natural cause: but this is to be accounted for, not by an obliteration in the vessels themselves, but by a morbid adhesion which the separated portion of the placenta contracts with the uterus, owing to the effusion of coagulable lymph between the two surfaces. But even presuming that it was possible for a consolidation of the bleeding vessel to occur to the fullest extent, still the process must occupy a considerable time; nature cannot effect it at once;—in hæmorrhage under labour, death must occur before even it could be commenced; consequently it cannot be considered as a means of preservation.

But independently of the formation of coagula—independently, if it is even possible, of the consolidation of the trunk of the vessels, and their retraction — nature arrests uterine hæmorrhage under labour by means as sure, as powerful, as effective, nay, even more so than the silken ligature of the surgeon, and almost analogous to it:—I mean the contraction of the uterine fibres. We have demonstrated the blood-vessels circulating in the uterus in a most tortuous manner, intersected and surrounded by the uterine fibres in the form of a complicated net-work. When the fibres of the uterus contract, the vessels are closed by the compression; and it is to this admirable contrivance, to this incomparable system of "living ligatures," that all women owe their safety after delivery. This arrangement of nature possesses a decided advantage over the art of the surgeon; because not only is the trunk of the vessel closed at one point, of a line's breadth, but the compression extends along the whole of its canal. We must ever bear in mind, that it is to uterine contraction alone that we are to look for the ultimate safety of every woman from flooding under labour: if it were not for this contraction, she must inevitably die.

Symptoms.—The symptoms which accompany uterine hæmorrhage under labour, are those of bleeding in general. The pulse becomes quick, small, feeble, indistinct, and fluttering; the breathing becomes hurried and laboured; the respiration is drawn with sobs and sighs; the voice falters; the countenance is pallid; the lips exsanguined; the eyes glassy and lustreless; the extremities cold; and a cold perspiration breaks out on the cheek, neck, and forehead. The pulse, by degrees, becomes more feeble and indistinct, and at last fainting supervenes. During the continuance of this faint, the patient remains motionless, and the pulse at the wrist is not to be felt at all, perhaps, or at the most is beating very languidly. The heart's action is also enfeebled, or possibly suspended for a few strokes. After an uncertain time, the pulse is again to be felt; the breathing is more natural; the lips and cheeks partially regain their beauteous tinge, and the eye a portion of its fire; the extremities and the general surface become warmer. With a return of animation—with a restoration of arterial action—occurs a return of the bleeding; and the patient rallies, only to contend with fresh and increased dangers. Again the pulse flags; again she sobs and sighs; again there appear the ghastly face and sunken eye; again animation is suspended; and usually the second faint is more intense and longer than before. She may recover a second time, with depressed powers; and now possibly restlessness will take the place of quiet. At first she throws her arms about, tosses off the bed-clothes, cries out for fresh air; and then universal jactitation supervenes. No entreaties, and scarcely force, can restrain her in one position, till again she sinks motionless and faint. On recovering again, there will probably be rigors throughout the whole frame; vomiting will come on; there is great anxiety of countenance and mind;—she is sure she is dying, calls for her husband and children; and although seldom her presence of mind and fortitude forsake her, still much dread and solicitude are evident. A sense of constriction of the chest will next appear, as if a cord were tightly drawn around the centre of the body. This suffocating sensation is usually followed by two or three convulsions, and death closes the terrible and agonizing scene.

Although these are the general symp-

toms, and they mostly occur in the order in which I have enumerated them, it is not necessary that they should all shew themselves;—rigors and vomiting, for instance, may be wanting; and the time which may be occupied from the first commencement of fainting till death results, varies exceedingly, according to the constitutional strength and several external circumstances. Sometimes, as before remarked, the first attack of syncope carries off the patient; sometimes there are many faintings, and the vital spirit flutters and hovers around the devoted head, as if unwilling to quit a tenement which it has so long inhabited. You may have observed the flame of a candle dying in its socket; it rises occasionally and flickers, throwing a sudden, a tremulous, and transient flare through the apartment, and as quickly subsides—the momentary glare giving place to darkness and gloom. In the same way the last remaining spark of life brightens and gleams, and flashes for an instant, and when again it sinks, more cheerless is the prospect left, from the destruction of those vain hopes which the temporary restoration had inspired.

Under such a state of distress and danger, the medical man's duty is indeed of the most arduous and harrowing description; to be of service, he must be prompt, persevering, steady, and decisive; and he must continue in the use of his means until the total cessation of the respiratory actions proves that life is entirely extinct.

Treatment.—In the treatment of hæmorrhage our duty is twofold: *first*, to restore the patient from the faint into which she has fallen, provided its intensity, or the length of its duration indicate immediate danger; *secondly*, to prevent a return or increase of the bleeding.

The treatment that we should have recourse to as a general principle, on flooding occurring during labour, is, in fact, much the same as that we should employ under many of the ordinary states of hæmorrhage. There are particular means, indeed, which are applicable to every particular case, and which I shall hereafter mention, when I come to treat of those cases individually, but I shall now speak of our management generally; and, in the first place, let us give our attention to venesection. It was formerly the practice to consider that all hæmorrhages, except they arose from accident, were to be regarded as of an active character; and under this impression, venesection was had recourse to, even in flooding under labour, with a view of putting a stop to the discharge. I have already told you, that we look upon it as a passive hæmorrhage; that it is not produced by the increased action of the heart, causing a rupture of the vascular coats, but is occasioned by the blood being allowed to exude through orifices already rendered patulous; and it must be likened in its character to the bleeding from a punctured wound. If a patient were brought to a surgeon with the radial or ulnar artery, or any other large vessel, divided, would any man in his senses think to stop the flow of blood by syncope induced by bleeding at the arm?—Certainly not; he would put a ligature around the vessel, and endeavour to save the patient from a greater loss. In a case of placental presentation, however, before any means were taken to accomplish delivery, I have known the patient bled, in the hope of restraining the hæmorrhage, which very act, in a great degree, diminished her previous chance of life. I do not mean to assert that in very many cases of hæmorrhage under pregnancy, and especially the early stages, bleeding may not be highly useful; but in labours complicated with flooding, it is scarcely ever—nay, I would say never—serviceable. Let us not think, then, of bleeding from the arm while the blood is gushing from the uterus during labour.

Secondly, let us consider opium. Opium is held up by many very great authorities as a most valuable means for arresting hæmorrhage, particularly after the child is born, and the placenta has been expelled. Opium takes off muscular contraction, by destroying nervous sensibility; and it also removes uterine action—the very power on which alone we are to rely for the full and complete closure of the open vessels. Does it not, then, seem preposterous to use those very means for subduing hæmorrhage which would take away our only source of safety? Few men, even its most strenuous advocates, recommend it at the commencement of labour, or before the child is born; and hereafter an opportunity will be afforded us of canvassing its merits after the birth is perfected.

Thirdly, stimuli. Stimuli are had recourse to for the purpose of rousing the vital energies,—to keep the patient from sinking under the faintness occasioned by the discharge; but their use is dangerous, and they should be avoided, if they can in any way be dispensed with; because, through their agency, the nervous and arterial systems are excited and wound up to a pitch beyond the healthy standard. Thus, as arterial action is increased, the coagulum at the orifices of the vessels, if it have been formed at all, will most likely become dislodged. This plug is to some extent a safeguard, but not one to be depended upon, to the exclusion of other means. It is right, however, to preserve it

as long as it answers the purpose of restraining the discharge in any considerable degree. Another reason why we should not unnecessarily have recourse to stimuli is, because it has been observed, first, I believe, by Kirkland, and since corroborated both by Hewson, Jones, and Thackrah, that the blood coagulates more readily under fainting than when the circulating system is in full vigour. This is a singular and most beautiful provision of nature to sustain endangered life; and if we remove the fainting by suddenly increasing the arterial powers, we prevent the deposition of coagula, as well as run the risk of forcing away the plug even when once formed. Cases, however, are unfortunately occasionally met with, in which we must have recourse to stimuli; where we must rouse the patient from the faint into which she has fallen, lest that particular syncope should prove fatal. We have only the selection of two evils offered us; and we must resort to stimuli, as a matter of necessity, though not of choice. These means are never to be employed, then, except where there is decided present danger.

Other remedial agents have been used besides stimuli. The ergot of rye has been much extolled for preventing hæmorrhage; and various papers have lately appeared, intended to prove that it will restrain the flow of blood from other parts of the body as well as the uterine organ. Sufficient trials have not yet been made to enable me to speak positively on the subject, with regard to other organs of the body, but it certainly has the power of restraining bleeding from the uterus in many varieties. Where the uterine fibres are too relaxed to produce the contraction requisite for the perfect closure of the vessels, we shall find the ergot of rye a most valuable medicine,—much more so, indeed, than opium, whose action is to remove rather than excite uterine contraction. The ergot of rye is particularly beneficial in hæmorrhages preceding the birth of the child, and in those after the entire evacuation of the uterine cavity, when it remains in a state of flaccidity,—when the fibres of the organ, for want of sufficient tone, refuse to take upon themselves that contraction which is indispensable to the well-doing of the patient.

Refrigerant medicines, and particularly the mineral acids, are very largely used under uterine bleeding, as well during labour as in the unimpregnated state. Mineral acids, it is well known, coagulate albumen; and direct experiments, as well as observations made on patients affected with calculous disorders, prove that they are beyond the influence of the digestive powers, and enter the blood unchanged. It is reasonable to infer, then, that they may, in no slight degree, favour coagulation, and to this quality we may attribute their efficacy in most passive hæmorrhages. In their power of repressing the more violent discharges, indeed, I have but little faith, and would certainly not trust solely to their agency to restrain any copious eruption of blood from the uterus of a parturient woman: but, as they are generally grateful to the patient, and within proper moderation not likely to prove injurious, no objection that I am acquainted with can be urged against their use;—provided too much reliance be not placed upon them, to the exclusion of other more efficient means. The acetate of lead, and turpentine, which have each of them been much extolled also, are, in my opinion, inappropriate remedies in those cases of more copious floodings which we sometimes meet with under labour.

Cold is also a valuable agent, when applied generally, to overcome faintness, and topically, for the purpose both of moderating the flow of blood to the uterine vessels, and exciting contraction in the uterine fibres; and when combined with astringents, its powers seem to be augmented. In the commencement of the flow, before faintness supervenes, cold may be had recourse to almost universally, and without restraint; but its advantage, when the system is much depressed, is more than equivocal: for there is a point beyond which the vital energies cannot bear up against the continued application of cold; and at that point it becomes actually injurious. Some discrimination is therefore necessary in the use of this common and effective agent.

Pressure on the uterine tumor by a bandage, or, what is better, by the grasp of the hand, is another means of restraining some varieties of flooding, most powerful, daily had recourse to, very generally employed, and open to few or no objections. And the evacuating the uterus artificially is sometimes necessary, though never to be undertaken without grave occasion.

We have been recommended, and especially by Dr. Stewart, to plug the vagina in some cases of hæmorrhage under labour; but this practice appears to me—except perhaps under placental presentation—likely to be fraught with hazard; for the uterus, at the full period of pregnancy, by reason of its lax condition, allows itself to be easily distended with coagula; and while its cavity is sufficiently capacious to contain a large quantity, the source of danger is concealed by the blood being pent up within; and, as the nature of the case may be overlooked, the peril is likely to be increased. In hæmorrhages under abortion, indeed, when the cavity of the

womb is small, and its parietes incapable of distention to any great extent, the tampon will often be found invaluable.

Puncturing the membranes, and letting off the liquor amnii, is often resorted to in accidental hæmorrhage with the most beneficial effects; but this subject requires a deeper consideration than this summary sketch will admit of, and will be reverted to at our next meeting.

The plan of transfusing blood from the system of another person to that of the patient, for the purpose of rousing the dormant powers and of sustaining life under hæmorrhage, has lately been had recourse to in some cases with success. But this idea is by no means of modern growth, though its application to the specific object we are considering is comparatively new. Thus we find Ovid representing Medea renewing the youth of Œson, by injecting the juice of herbs into his veins; and the transition would be easy and natural from infusion of herbs to fresh-drawn blood. It is more than probable, indeed, that such a fancy originated in a mere flight of poetry, not from any custom that had been adopted by the ancients. The flitting and airy phantoms of the poet's imagination, however, have been embodied into a distinct and substantial form by after ages; and during the extensive reign of the humoral pathology, not only were different medicinal substances injected, by way of experiment, into the veins of brutes, but Dr. Edmund King, about the middle of the seventeenth century, was bold enough to try the effect of transfusing blood from the lower animals into the veins of man. Our French neighbours, with their characteristic enthusiasm, caught at the novel experiments; the most extravagant speculations were entertained of the virtue of the new system; and it was given out that decripitude, age, and disease,—nay, even death itself,—would flee before the all-powerful and all-resuscitating process. Short-lived, however, were these high-wrought expectations; the most dreadful effects were the consequence of the pernicious practice; and in France this method of treating diseases was denounced under severe penalties.

To Dr. Blundell we are indebted for the proof that the system of the inferior animals can be nourished for a length of time merely by injecting, at proper intervals, into their veins, the healthy blood of an animal of the same species: for he preserved a dog alive for three weeks by this means without food, merely allowing it a little water; and in that space the animal was reduced but little more than one-fourth of its whole weight. From this experiment we may conclude, that the same treatment might possibly be found efficacious in the human subject. And as a means of preserving life under uterine flooding during parturition, we shall find that its employment is more likely to be beneficial when the organ is entirely empty and contracted, than at any other period of the labour.

These, then, are the principal means which have been had recourse to under hæmorrhage in labour—bleeding, opium, the ergot, stimuli, refrigerant medicines, cold, pressure, evacuating the uterus either partially or entirely, plugging the vagina, and transfusion of blood; to most of which I shall again have to call your attention, when laying down the principles of treatment for each variety of the accident.

May occur at any period of the labour.—Hæmorrhage may occur at any stage of the labour—before the liquor amnii is evacuated; after the membranes have ruptured, and before the head is born; after the head is born, while the shoulders are in the pelvis; between the birth of the child and the expulsion of the placenta; and even after the placenta is expelled, when the uterus is emptied of its previous contents, and when, in the common acceptation of the terms, we should consider the labour as concluded.

Hæmorrhage previous to the birth of the child. To our countryman, Rigby, we are indebted for a great practical improvement in the treatment of flooding at the commencement of labour. He distinguished hæmorrhage, occurring previously to the birth of the child, into two species—the first of which he called *unavoidable*, and the second *accidental*.

By the first kind—unavoidable—we understand cases in which the placenta offers itself at the os uteri, either blocking up the mouth of the organ, or being partially placed over it; so that dilatation cannot take place, without *necessarily* separating the placenta more or less from its uterine attachment, and without rendering patulous those orifices which were previously covered and closed by the apposition of the mass.

By accidental hæmorrhage we mean those cases where the placenta—being implanted, not over the os uteri or near it, but in its more natural situation, the fundus or body—becomes, to a greater or less extent, disunited from the uterine surface; but where the separation is perfectly accidental, and where it is not necessary that bleeding should occur upon the opening of the os uteri.

PLACENTA PRESENTATION.

It has for a long time been known that the placenta may be found at the os uteri under labour; and this malposition was

noticed by Mauriceau, Peu, and La Motte; all of whom, however, held the opinion, that it was not originally apposed to this part of the uterus by nature, but that, in consequence of some peculiar accidental circumstance, it had become loosened from its attachment above, had fallen down by its own weight, and had thus accidentally placed itself over the uterine orifice. But, inasmuch as not only is the placenta attached to the surface of the uterus, but the chorion is in apposition to that surface throughout the whole extent of the membrane—the decidua, indeed, being interposed—and inasmuch as the membranes are closely united with the placenta, it would follow (provided this idea was correct) either that they must be torn from the placenta all around, or that the whole ovum should partially revolve. We know that neither of these occurrences takes place; we know that there is exactly the same arrangement of the vessels of the cervix uteri, and exactly the same kind of connexion between those vessels and the placenta, as obtains between that organ and the vessels of the other parts of the uterus, when it is placed in a more natural and favourable situation: we have demonstrative proof of it, indeed, before our eyes, in the preparation of placental presentation on the table, in which case the patient died of hæmorrhage, the os uteri being dilated to the size of a crown, and no efforts having been made to deliver her.

Giffard was among the first writers who asserted that the placenta might be placed by nature over the os uteri at its first formation; and his remark has been perfectly borne out by the observation of every practical man since his time.

Under a placental presentation, then, there must necessarily be a greater or less discharge of blood, on the dilatation of the os uteri; and, if the case were left entirely to nature, the bleeding would proceed either as a draining or in gushes, until the successive faintings terminated in a mortal syncope: or—the os uteri dilating rapidly, and the womb acting vigorously—the head of the child bearing forcibly against the placental mass, might expel it first, and itself quickly follow: for it would be impossible for the child to perforate the placenta, and pass through it; and it would also be unlikely that it should escape by its side, provided the mass were implanted centrally over the uterine mouth. A number of cases are on record in which the placenta was expelled before the child, in the manner I have just mentioned. My father, in his work, has recorded three instances which came under his own observation; and three others, communicated to him on authority which he could not doubt. Smellie and Barlow have each given a case of the same kind. In a small obstetric work, published many years ago by Sarah Stones, we have one case in which a similar termination occurred. Another is recorded by Mr. Baillie, in the London Medical Repository. In this case the placenta was expelled as the woman was getting into bed, and was suspended between the thighs by the funis. There was great hæmorrhage. The child was delivered by turning, and restored by the warm bath. Dr. Hamilton also mentions in his lectures having known two instances of the like kind. Some others similar may be scattered through different obstetric treatises; but I believe these are the principal authentic cases that have been put on record. Although there is thus a possibility of a natural termination of the labour, by the placenta passing first, and the child being expelled afterwards, it would be wrong to expect it, or to wait for it; for the probability is that the woman will bleed to death,—as was the case with the patient whose uterus is on the table,—before the os uteri acquires a diameter sufficient to allow the passage of the child's head through it.

Knowing, then, that in the great majority of instances the patient will die if relief be not afforded, it is considered as an obstetric principle, that, under entire placental presentations, delivery should be effected by art as early as is practicable, without incurring the risk of injury. We shall proceed, therefore, to speak of the symptoms, and the mode of treatment.

Symptoms.—We have some symptoms, which are very suspicious, of placental presentations, and others that assure us of the nature of the case. Those which are suspicious appear at the commencement of labour, or even before the accession of uterine action,—during the last weeks of pregnancy.

We have already learned, that during the few first months of utero gestation, until, indeed, between the fifth and sixth, the cervix uteri is not developed; it has not yet been expanded, or taken up to form a portion of the general cavity; but when five months are perfected, or before that period, expansion commences, and this unfolding or developing of the fibres must necessarily produce a separation of the placenta from its previous attachment to the body, at the upper part of the cervix. This separation, then, must in its turn occasion flooding, even before the process of labour is begun; because the orifices of the vessels previously plugged by the placental mass are opened on its partial dislodgment by the gradual expansion of the uterine neck. We find, then, that during the last few weeks of pregnancy, a patient under placental presentation is liable to

sudden gushes of blood, in a greater or less quantity, coming on without any apparent cause, neither the consequence of exertion, nor accident, nor mental agitation; but when she is asleep in bed,—while she is sitting quiet in her chair,—when unemployed in any active duties,—she is unexpectedly seized with a flow of blood from the vagina. Her attention, perhaps, is scarcely drawn to the occurrence before it has subsided, or diminished to such a degree as to give her but little uneasiness: she will probably suppose that labour is coming on, and she may begin to make preparations for its approach. After the lapse of a few days, another gush takes place as unaccountably, and subsides as suddenly as the former; and attacks of the same kind recur at uncertain intervals during the remainder of her pregnancy.

Whether the patient's mind be impressed with much alarm or not at these repeated bleedings, to one acquainted with the physiology of the placenta, and the peculiar connexion between that organ and the uterus, such a history would be fraught with suspicion and anxiety; and she should be watched over with the most assiduous and solicitous attention.

Our suspicions may also be excited at the beginning of labour, if with every pain, with every slight increase of dilatation in the os uteri, there be an increase of discharge, and if the flow of blood be moderated or suspended in the interval of action.

But we can only positively assure ourselves of the nature of the case by an examination per vaginam. To this the patient may be unwilling to submit; she may suppose we can be of no service to her, since the pains are so infrequent and trifling. It is our bounden duty to combat her objections, and to insist on the necessity of the measure proposed; since our practice so much depends on the information we gain. The examination will most advantageously be made with the two first fingers of the left hand, because they pass so much higher within the pelvic cavity, and so much more completely command the os uteri. By this inquiry we learn whether the placenta be implanted over the mouth of the womb or not; and if we discover its presence, we must ascertain whether the orifice be entirely or only partially occupied by its mass; for our practice differs much under the two varieties.

Diagnosis.—We shall know the placenta by the fleshy, fibrous, and lobular sensation which it communicates to the finger, and by its being attached to the cervix uteri within. If we can introduce our finger sufficiently far to pass it round within the orifice, we shall be sensible of this attachment, although the union is very easily separated. There is but little chance of mistaking the placenta for the membranes partially protruded into the vagina; but there is great probability that we may mistake a coagulum, blocking up the os uteri, for the placental mass apposed over it. How, then, shall we discriminate between these two?—The placenta cannot be perforated or broken down; the tenacity of a coagulum may without difficulty be destroyed. The placenta is attached within; a coagulum lies loose. The placenta cannot be removed by the finger; but we can generally bring away a coagulum. If there be any doubt, let us take these means,—let us try whether it is attached,—whether we can break down its structure, and whether we can remove it from its position; but these attempts must be made with the greatest possible care. In some cases I have for a moment been in doubt whether it were the placenta presenting, or a coagulum formed at the uterine mouth; but in all instances in which it was merely a coagulum I have been enabled either to break down its structure or remove it altogether.

PHYSIOLOGICAL AND PATHOLOGICAL OBSERVATIONS ON THE BLOOD AND URINE;

Being the Gulstonian Lectures for 1834, delivered at the Royal College of Physicians.

By Geo. Burrows, M.D.
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[Concluded from p. 590.]

Lecture III. concluded.—*Morbid secretions and malignant structures arise from an altered state of the blood—Effects of natural secretions introduced into the blood, bile, milk, semen—Of morbid secretions, as pus: this fluid sometimes arises from the decomposition of the blood itself: appearance of malignant matter in the blood-vessels—Two cases of this kind—This matter is not absorbed—John Hunter's laws of the animal economy applied to their formation.*

Andral's opinions of primitive diseases of the blood advocated.

Effects of the introduction of small quantities of organic matter into the animal economy.

Effects of the introduction of an organized being among a whole class of organized beings.

Effects of the introduction of one living being into the system of another living organized being.

Inferences from these considerations.

Conclusion.

No one will deny that it is from the blood that the various parts of our bodies are formed, and the various secretions eli-

minated: if direct proofs cannot be adduced that morbid changes of structure or secretion are owing to alterations in the circulating fluid, still, having admitted that the healthy processes of nutrition and secretion are carried on by the blood, it is difficult to deny that the morbid processes are equally due to an altered state of that fluid. Thus we find that when individuals are long exposed to an insufficient supply of food, to an impure atmosphere, and to deprivation of the influence of the sun's light—causes contributing to deteriorate the qualities of the blood, this fluid circulates less freely and less highly coloured through the capillary system of the skin, and at the same time there is a tendency to various excessive discharges of the serous parts of the blood, tubercles often form, and even entozoa are developed in various parts of the body.

There are, however, more direct proofs that morbid secretions and malignant structures are the consequents of an altered state of the blood. We sometimes find these morbid fluids actually circulating in the system, and there are cases on record where the blood itself has degenerated or been converted into morbid growths, and even into the most malignant structures which infest the human body.

This idea would not appear so strange if we regarded the blood in the light it was considered by John Hunter, and is now contemplated by that able pathologist, Andral. John Hunter said*, "that it appeared to him to be evident, that the fluidity of the blood was only intended for its motion, and its motion was only to convey life and living materials to every part of the body. The materials when carried, become solid; so that solidity is the ultimate end of the blood, as blood."

He maintained "that the blood had as much the *materies vitæ* as the solids, which kept up that harmony between them: and as every part endued with this principle has a sympathetic affection on simple contact, so as to affect each other, so the blood and the solids are capable of affecting and being affected by each other, which accounts for that reciprocal influence which each has on the other."

Andral† reasons thus upon this interesting point of pathology:—"On the one hand, since the blood nourishes the solids—since without its presence they cannot live, how is it possible that the state of the solids should not be influenced by the state of the blood? On the other hand, if the solids of the body be examined in their relations to the blood, they may be divided into two classes, the one class employed in making blood, and the other in consuming blood.

"No solid whatever, then, can undergo the least modification without causing a disturbance in the nature or in the quantity of the materials destined to form the blood, or to be separated from it. Physiology, then, compels us to admit, that in consequence of any alteration in the solids there must be alteration of the blood; and in like manner, in consequence of any change in the blood, there must be a corresponding change in the solids."

These opinions appear to be quite in harmony with each other, and perhaps the latter is drawn from the former.

The natural secretions of the body which should pass off by the excretory ducts sometimes enter the circulation, and produce the most deleterious effects upon distant organs: the effects of bile carried into the vessels of the circulation, and of urea in cases of suppression of urine, and of milk in puerperal women, are well known. It was the opinion of Blumenbach, that the ferocious disposition of male animals in their wild state, during their time of heat or rutting, was owing to the absorption of the seminal fluid into the circulation.

Other fluids, however, the products of morbid actions, are found in the blood and in the vessels of the circulation, causing the most violent and distressing symptoms.

Pus has now been so frequently found in the veins, in the cavities of the heart, and sometimes in the interior of clots of blood at different points of the circulation, that it is unnecessary to detail individual cases in proof of this assertion. It generally occurs after phlebitis following phlebotomy or parturition, after amputations or operations of any kind, and very frequently after the matter is let out from a chronic abscess. It rarely makes its appearance in parts of the circulation distant from the diseased part, unless there be some external opening.

It was the opinion of John Hunter* that "true suppuration was the consequence only of inflammation," and that the large collections of matter found in chronic abscesses were of a scrofulous nature, and not the result of common inflammation. For, says Hunter, "the very surfaces which formed the above matter, immediately produce true matter, when inflammation comes on, which it always does whenever they are opened."

But the most interesting and serious consequences of this inflammation of the surfaces of a chronic abscess are, the absorption of pus into the vessels of the cir-

* On the Blood, p. 92.
† Précis, &c. vol. i. pp. 527-8.

* On the Blood, pp. 390—392.

culation, and its deposition in distant organs. These collections of pus are then found in many organs of the same body, sometimes in nearly all the joints, and at other times deposited in the cellular tissue between muscles. In those parts where the pus is found, there is no appearance of inflammation, nor of irritation.

In such cases, pus is sometimes likewise found in the cavities of the heart, in the veins and lymphatics; and we can hardly refrain from concluding, that pus has circulated with the blood.

At other times, pus has been found in small quantities in the middle of clots of fibrin, which had collected in one of the cavities of the heart: there was no suppuration going forward in other parts of the body, and nothing tended to prove that this pus had been brought there by absorption from other parts. With respect to these rare cases*, we can only draw this inference from them, that pus may form either in a solid or in a liquid solidified, as the coagulated blood, without our being able to discover any irritation or alteration of structure. It arises, probably, from the decomposition of the blood itself.

I once had an opportunity of observing this change of the blood into a fluid resembling pus under the following circumstances:—I had severed the two lower limbs of a full-grown fœtus, which had died in parturition, and immersed them in running water, in order to ascertain the manner in which these limbs would be converted into adipocire. After a few days, when the peculiar decomposition had arrived at a certain stage, upon compressing the limbs there exuded from the divided femoral vessels, a fluid exactly like pus. It appeared to me that this fluid was formed by the decomposition of the blood in the femoral vessels.

But many other products of diseased actions are found in the blood-vessels; and it is extremely difficult to decide whether these morbid matters have obtained admission by absorption of parts affected with similar disorganization, or whether they arise from the decomposition of the blood. Andral says, from whatever source they may be derived, they produce a complete change in the healthy physical characters of the blood. He has, in his valuable work on Pathology†, recited cases of this nature which have come under his own observation, as well as those published by other authors; but this subject is too extensive for me to enter upon in this part of my lectures. I will, however, shortly detail two cases of the kind which have recently come before me.

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* Andral, Précis, &c. tom. i. p. 405.

† Précis, &c. p. 537.

Case I.—Mary Jones, æt. 50, was admitted into St. Bartholomew's Hospital, in January 1834, under the care of my friend, Dr. Latham. She was suffering from ascites, with considerable œdema of the left lower extremity: there was some pain on pressure in the left iliac fossa, and also of the left thigh; there was great prostration of strength; and without any alteration in her complaints, she died a fortnight after her admission. Upon the examination of her body, the sigmoid flexure of the colon was found adhering to the parts around, and upon separating these adhesions a purulent fluid escaped: the source of this was ascertained to be the left Fallopian tube, which, as well as the ovary, were converted into cysts of considerable size, filled with a purulent fluid: the lining of these cysts was covered with a deposit almost resembling crude tuberculous matter, and the vessels of these parts were congested. The mouth of the uterus, as well as its cavity, contained a clot of blood firmly adherent to the lining membrane. The veins of the uterus and its appendages were examined, and found free from disease.

The left femoral vein was obstructed by firmly coagulated blood: this clot extended nearly as high as the junction of the external and internal iliac veins: the common iliac was lined by a layer of lymph, and the interior of the vein was red: this common iliac was filled partly with a fibrous clot, and partly with a softer substance, very much resembling that found in the ovary and Fallopian tube. Upon this part of the vein the cysts had rested, causing inflammation terminating in this peculiar product, and obstructing the venous circulation of the left lower limb.

Case II.—In February last a woman was admitted into St. Bartholomew's Hospital with pain in the epigastrium, nausea, vomiting, and tympanitic abdomen. She remained in this state for several days, and then died.

Upon the examination of her body, there was found a large carcinomatous ulceration, extending from the cardia almost to the pylorus. The edges were elevated, everted, and in some parts softened, the matter there resembling medullary sarcoma.

The vena porta was cut through about an inch above the juncture of the splenic and mesenteric veins, and the vein was found distended and obstructed by a mass resembling fœtal brain. This mass was traced down into the mesenteric vein, which was apparently lined with lymph: the whole splenic vein, from the spleen to the vena porta, was filled with firm coagulum of blood, which became paler and

more like the matter obstructing the vena porta as it approached that vessel.

All the small veins entering the splenic and portal trunks were filled with firm coagula.

Both of these cases are of the kind detailed by Andral, and are highly interesting to pathologists, whether we regard the peculiar matter obstructing the veins as identical with morbid matter in neighbouring organs, and absorbed from them into the circulation, or whether we regard these morbid masses as coagulated blood, degenerated or converted into these peculiar substances by some morbid actions of the vasa vasorum of these great veins. I am inclined to adopt the latter opinion; for had it been absorbed matter, it is probable that the vessels leading from the diseased organs would have contained similar matter, which they did not; and besides, there is no such direct communication from the absorbent to the venous system, in man at least, which could have conveyed the absorbed matter into these large veins.

I rather consider these cases as exemplifications of a principle of the human blood, which John Hunter first made known to our profession.

"Motion," said John Hunter*, "is essential to the fluidity of *healthy* blood;" fluidity essential to its power of conveying life; the continuance of life, or support of the body, is owing to the coagulation of the blood—to its becoming solid. "*It then changes into this or that particular kind of substance, according to the stimulus of surrounding parts, which excites this coagulum into action, and makes it form within itself blood-vessels, nerves, &c.*"

This is the law of health presiding over nutrition: the law regulating the formation of morbid products is probably analogous.

In the cases I have detailed to you, the circulation was impeded through large veins; motion, essential to fluidity of the blood, was destroyed; clots formed in these vessels: the blood had then become solid; and, as Hunter said, "it then changes into this or that particular kind of substance, according to the stimulus of surrounding parts."

The stimulus of the surrounding parts in these cases was in the one to the formation of scrofulous or puriform matter, and in the other to the formation of encephaloid matter—a modification of carcinoma.

I never heard of pus being found in the vessels coming from an organ infiltrated with melanosis, nor melanotic matter found in the veins and cavities of the heart after the puncture of a chronic abscess, nor encephaloid matter in the veins of an organ loaded with scrofulous tubercles. If the circulation be arrested, the coagulum is converted into a peculiar kind of substance, according to the stimulus of surrounding parts.

But I must leave the further development of this great principle of the blood, announced by John Hunter, to future occasions; and I conclude these pathological observations in the words of Andral:—

"Je viens de retracer les faits, les raisonnemens, qui, dans l'état actuel de la science, doivent conduire à reconnaître dans le sang l'existence d'un certain nombre d'altérations. Ce que j'ai dit à cet égard suffirait déjà, à mon avis, pour démontrer que non seulement ces altérations sont réelles, mais qu'elles sont souvent *primitives, qu'elles précèdent celles des solides*, et que dans le sang, par conséquent, se trouve le point de départ de plus d'une maladie. S'il est vrai que la masse du sang peut être, dans certains cas, primitivement alterée, il s'ensuit que l'existence des maladies générales n'est point une chimère: lorsqu'en effet tous les tissus viennent ainsi à recevoir un sang altéré, n'est-il pas physiologique d'admettre que leur mode normal de vitalité, de nutrition, de sécrétion, doit être plus ou moins profondement modifié? Ou il faut accepter cette conséquence, ou il faut nier l'influence que, de l'aveu de tous les physiologistes, le sang exerce sur tout solide."

I have pointed out and attempted to illustrate the effects of the introduction of minute quantities of *inorganic* bodies into our system by the observations of Dr. Prout, and by the experiments of Herschel, who called into action an enormous power by the alloy of a mass of mercury with a millionth part of its weight of sodium.

When small quantities of *organic* matter, or of the products of *organization*, are introduced into the *animal economy*, effects are produced very analogous to those described by Herschel upon the introduction of a small quantity of *inorganic* matter into larger matters *of the same*.

Let us regard first the vegetable kingdom. How small a quantity of woorara, how minute a dose of prussic acid, is capable of annihilating all the vital powers; so suddenly, that our senses have hardly time to follow the series of physiological phenomena, and so effectually, that antidotes are almost useless! Let us ascend the scale of organization, and the intensity of its products are not less striking. How minute a quantity of poison from the reptile class introduced into the blood will produce sudden decomposition of it, and almost inevitable death!

The smallest particle of saliva adhering to the enamelled surface of the tooth of a

* On the Blood, p. 86.

rabid dog, when left in the wound, seems maliciously to rest there for a time, but only the more effectually to destroy with all the horrors of hydrophobia.

Duhamel* relates that a butcher, while slaughtering an overdriven ox, merely put the knife with which he killed the animal between his lips, and in a few hours he was attacked with swelling of the tongue, difficulty of breathing, black pustules all over his body, and death ensued in four days. Another person scratched his finger with the end of one of the bones; the wound mortified, and he died.

The smallest particle of poisons from the human body, either dead or alive, are no less virulent—no less powerful.

The dreadful constitutional disturbance following pricks made in the dissection of human bodies, and which is so often fatal to members of our profession, and had so nearly deprived us of one of our valued colleagues at the present time, is familiar to you all.

Poisons imbibed from the living human body, in the smallest quantity, are capable of exciting equally extraordinary effects in the animal economy.

How minute a particle of matter from the pustule of small-pox, merely introduced beneath the cuticle, is capable of inducing a precisely similar disease in the inoculated person! This remark applies to the morbid matter of other diseases, and it is not a little wonderful that such infinitessimal molecules should be capable of deranging the whole animal economy, and of exciting it to the production of pints of matter identical in all its properties with the original molecule.

A little leaven has indeed leavened the whole lump.

I have now been speaking of the introduction of the products of organization, in minute quantities, into the body of an organized being—man. But what are the effects of the introduction of an *organized being* among a *whole class* of *organized beings*?

All agriculturists and gardeners are familiar with the fact, that when weeds introduce themselves into a space appropriated to cultivated species, the latter are starved in their growth, or completely stifled. One noxious weed will thus destroy all trace of a beautiful variegated carpet of flowers.

I must here borrow some ideas from the philosophical work of Mr. Lyall, on Geology, in illustration of the effects of the introduction of a new species of animals into a particular district of the earth.

Let us cast our thoughts back to the period when the polar bears first reached Iceland, probably on icebergs floated from the opposite coast of Greenland. Several of these animals might effect a landing at the same time, and the havoc they would make among species previously settled in the island would be terrific. The deer, the foxes, the seals, and even the birds, on which these animals sometimes prey, would be thinned down, and some species perhaps exterminated. "But," observes Mr. Lyall, "this would be a part only, and probably an insignificant portion, of the aggregate amount of change brought about by the new invader.

"The plants on which the deer fed being less consumed in consequence of the lessened numbers of that herbivorous species, would soon supply more food to several insects, and probably to some terrestrial testacea; so that the latter would gain ground. The increase of these would furnish other insects and birds with food, so that the numbers of these last would be augmented.

"The diminuation of the seals would afford a respite to some fish which they had persecuted; and these fish would then multiply, and press upon their peculiar prey.

"Many water-fowls, the eggs and young of which are devoured by foxes, would increase when the foxes were thinned down by the bears; and the fish on which the water-fowls subsisted, would then, in their turn, be less numerous. Thus the proportions of a great number of the inhabitants, both of the land and the sea, might be permanently altered by the introduction of a new species into a region; and the changes caused indirectly might ramify through all classes of the living creation, and be almost endless*." Such, then, are the surprising effects of the introduction of one new species into a mass of living organized beings.

What may be the effect of the introduction of *one organized being* into the system of *another living being*?

From the time of Lewenhoeck up to the present, physiologists have admitted the existence of animalculæ, or of molecules, endowed with the power of spontaneous motion, in the seminal fluid of animals. Indeed, without admitting their existence, it would be extremely difficult to offer any explanation of impregnation, and more especially in cases of extra-uterine conception.

The appearance of entozoa in different organs of animals is another point which has offered many difficulties to physiologists. The idea of the spontaneous generation of these animalculæ has been rejected by the soundest physiologists, and Cuvier rather adopted the explanation

* Andral, Précis, &c.

* Vol. ii. p. 150.

that their ova passed from the alimentary canal, through the organs of the circulation, into distant organs.

The solar microscope has now magnified a drop of water upwards of 500,000 times: each drop contains hundreds of animalculæ, and if each of these little beings is endowed with a fecundity at all proportionate to the other inhabitants of the same element, the salmon for instance, how wonderfully, how inconceivably minute, must be the ovum of one of these animalculæ! Who will then say that the idea of Cuvier is improbable or impossible?

I have addueed these two facts from physiology to show, that if it pleased Him who has caused the principle of life equally to reside in such an infinitessimal molecule as the ovum of one of these animalculæ, and in the mammoth or the whale—that the motion of the corpuseules in the seminal fluid should eease—or that the ova of the animalculæ of a drop of water should increase and multiply within the several organs of our bodies;—then, in the first ease, the present generation would be the last, and the whole of mankind would moulder into kindred dust;—and, in the latter case, the race of human beings would be suddenly annihilated, and would vanish from the face of the earth like a morning eloud.

Let me venture to draw your minds yet higher, from the erust of our globe to the expanse of the heavens—a provinee of nature into which the true *physician* ought to extend his observations.

Modern astronomers have proved from the phenomena attendant upon the return of eomets, a high degree of probability, nay almost a certainty, that there exists throughout all space a subtle, refined, resisting medium.

This medium, however delicate it may be—however slight its powers of retardation, must eause a diminution of the earth's velocity in her orbit, and henee the centrifugal force will be less. The earth will, therefore, slowly, but certainly, in process of ages, be attracted to the sun.

The symmetry of this universe will then be destroyed; "the heavens will then pass away with a great noise, and the elements shall melt with fervent heat; the earth, also, and the works that are therein, shall be burned up*." How awful the event—how simple the means!

So, then, if there be any truth in the facts and observations which I have brought forward in these lectures, we must eonelude that it is not the quantity or apparent power of the agents we employ in the treatment of diseases, which will aeeomplish the wished-for object—their cure, but rather the right adaptation of means to the end; and further, we may learn from the phenomena of nature, that the simplicity of the agents employed by our Creator, is not less wonderful than the magnitude of the results brought about by his will.

* 2 Pet. iii. 10.

TINCTURE OF LYTTA IN RETENTION OF URINE.

To the Editor of the Medical Gazette.

Sir,

As I do not remember to have met with a case of retention of urine from atony of the muscular power of the bladder treated by the tincture of lytta, I am induced to forward the following successful one to your widely-circulated journal. Should you deem the case of sufficient interest, an early insertion will oblige

Your obedient servant,

Jesse Leach,
Clinical Assistant.

Westminster Hospital,
July 21, 1834.

I am aware that several objections might be urged against the internal administration of lytta, more especially on account of its producing stranguary; however, the present is a good instance of its beneficial effects without that unpleasant concomitant. Cases have come under my notice where stranguary was produced before the muscular action returned. The acetate of morphia and camphor mixture were given in this case, with a view of allaying the spasmodic hysterical paroxysms, which answered the expected result.

It would be trespassing too much upon your space to enter into a detailed history of a victim to hysteria; I shall therefore content myself with a brief summary of her case.

Ann Gough, æt. 22, of a short stature, and a sallow, bloated appearance, had been for some years, periodically, a martyr to hysteria and scanty menstruation. About a year previous to the present attack, after suffering from an hysterical paroxysm, she was not able to void her urine without the aid of the catheter for four months; was admitted into Queen's Ward, on June the 25th, under the care of Dr. Bright,

labouring under a violent hysterical fit, which readily yielded to the usual treatment, leaving, as on the previous occasion, a retention of urine, preceded by pain in the back, aggravated by percussion over the third and fourth lumbar vertebræ. The warm bath, leeches, cupping, and ultimately the ointment of ant. tart., were administered, which benefited the pain and tenderness, but had no effect on the muscular efforts of the bladder. On June the 29th, she could not pass her urine without the aid of the catheter twice a-day. On July the 8th it was determined to give internally the following mixture, in order to stimulate the dormant muscular energy, and allay the violent hysterieal fits which were, about this period, of frequent recurrence.

℞ Tinct. Lyttæ, ℥ij.; Morphiæ Acet. gr. j.; Mist. Camph. ℥viij.; Cap. ℥j. ter die.

July 10th.—The hysterical paroxysms have not recurred so frequently, but the lytta has produced no effect.

11th.—A burning sensation along the course of the left ureter and in the hypogastrie region, but no attempts have, as yet, been made to void her urine without the catheter.

13th.—During the morning, she made several unsuceessful attempts to void her urine, but did not sueceed till the evening; when the urine possessed a strong ammoniacal smell, and was free from blood.

14th.—Urine passed without any diffieulty, exeept a slight scalding.

21st.—The woman is perfeetly convalescent, and thinks of leaving the hospital in a few days. She has had no relapse of her hysterical paroxysms since the 13th.

CASE IN WHICH A FEMALE CATHETER ACCIDENTALLY SLIPPED INTO THE BLADDER.

To the Editor of the Medical Gazette.

Sir,

In your journal for September last* I related the history of a case in whieh a female cathcter had aceidentally slipped into the bladder, where it remained fifteen days, without oecasioning much irritation, when it was removed by dilating the urethra with a sponge tent. It is rather singular that another case of the same kind should so soon have fallen under my observation.

Elizabeth Creed, aged twenty, married about four months, had retention of urine, and was relieved several times by the catheter, when, in passing it one day, it unfortunately slipped from the surgeon's fingers into the bladder. No attempt was made to extract it, and about a fortnight afterwards she was brought, from a distance of thirty miles, to the Bridgewater Infirmary. Having ascertained the presence of the instrument, I introduced a piece of sponge tent, in the evening, into the bladder, and allowed it to remain during the night. On the following morning I withdrew the sponge, and passed my finger, with some diffieulty, into the bladder, and felt the eatheter resting on the pubes; from whence it was removed into the urethra, and readily extracted. In this case the instrument remained in the bladder seventeen days, and created considerable irritation. There was much greater difficulty in dilating the urethra in this case than in the former one, which oeeurred after delivery; in the latter, the urine passed involuntarily until the fourth day, but in the former the urethra regained its power immediately.

Two such cases having been brought to my notice in so short a period, it would appear that this accident is not of such rare oecurrenee as is generally imagined; and with the instruments usually employed, may frequently happen from sudden shrinking, or change of the patient's posture. It may not, therefore, be amiss to eaution my professional brethren against using the eommon silver catheters, whieh are rarely provided with guards. Of late I have preferred a long flexible cathcter, whieh is more convenient, partieularly in all cases where it is neecssary to empty the bladder during labour.—I am, sir,

Your obedient servant,

Jonathan Toogood,
Senior Surgeon to the Bridgewater Infirmary.

Bridgewater, July 21, 1834.

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* Vol. xii. p. 837.

EFFICACY OF OXIDE OF BISMUTH IN PULMONARY COMPLAINTS.

To the Editor of the Medical Gazette.

SIR,

I HAVE recently taken the liberty of addressing a letter to you, respecting the use of the oxide of bismuth in diseases of the lungs*; and I did so from having experienced very decided advantage in some severe cases of pulmonary affection by the use of this medicine, and under the impression that sufficient attention had not been paid to the immediate sympathy which subsists between the stomach and the lungs. In our ordinary treatment of these diseasess, we advert very generally to the connexion which the skin has with that important organ, the lungs; and counter-irritants on the external skin have been very universally applied, to correct the disordered functions of the organ of respiration; and as there does, beyond a doubt, exist a very strong sympathy between the stomach and the lungs, as well as with other important and more distant parts of the body, I wish to call the attention of the profession to another mode of counter-irritation, for altering the actions of the stomach, with a view to the alleviation of diseases of the lungs. The oxide of bismuth has not appeared to me to quicken the circulation, or in any way to excite fever; but it induces new actions in the stomach, which, in the experience I have had, seemed to influence very decidedly the nature of the expectoration, so formidable a symptom in these cases.

I write thus shortly, conceiving that the readers of your excellent publication will not require a more elaborate dissertation to enable them fully to comprehend the practical fact, which I consider myself authorized to communicate, but which in so intractable a disease, must require more extended experience to establish firmly.

I have the honour to remain, sir,
Your obedient servant,
GEO. S. GIBBES, M.D. F.R.S.
Fellow of the Royal College of Physicians.

Weymouth, July 27, 1834.

* Med. Gaz. p. 567, *ante*.

RETIRED MEDICAL OFFICERS OF THE ARMY AND NAVY.

To the Editor of the Medical Gazette.

SIR,

As the pages of your widely-circulated journal appear to be not only the most proper, but also the most efficient, medium for the following observations, I trust you will deem them worthy of insertion.

In this, our day, "reform" seems wholly to engross the minds of men, and at this moment a Committee of the House of Commons is sitting on the subject of what is called "medical reform." What may be the result of their inquiries, or what the legislature may decide upon, I cannot foresee; but I would beg leave to call their attention to a class of medical men who should not be overlooked, nor treated with indifference;—I mean the retired medical officers of the army and navy. To become qualified for these services, an examination at the Royal College of Surgeons, and subsequently an examination on the various branches of medicine, (equal, at least, to the examination for a medical degree at some of our universities,) is required, and must be undergone by the candidates at the respective Boards. Surely, then, it would be a hardship if "medical reform" were to extend to the depriving this class of medical men of a right to practise the profession in which they have been properly educated, examined, and found efficient members. I would therefore suggest, that the heads of those medical departments should be privileged to grant certificates of competency to practice to those gentlemen who have been in the service, and are practising, or such as may hereafter retire from the service. Would it not be a hardship of the first magnitude that a man who has spent the prime of his life (and perhaps has lost his health) in the service, should be doomed to starve, or else become a school-boy, as it were, by resuming the required course of studies, to enable him to take out his Doctor's degree?

The Society of Apothecaries, in the most liberal and handsome manner, granted to the medical officers of both army and navy, the privilege of practising as apothecaries; and it would be

creditable to the College of Physicians to follow this example.

I trust, Mr. Editor, that the foregoing remarks will call forth your energies, and also the attention of Parliament, to this class of meritorious practitioners; and that the protection will be afforded which they so justly deserve.

I am, sir,
Your very obedient servant,
D. J. H.

London, July 7, 1834.

URINARY SEDIMENTS.

To the Editor of the Medical Gazette.

Sir,

I am anxious to correct a mistake into which your correspondents, Messrs. Brett and Bird, have fallen, with respect to the composition of urinary sediments. I have never asserted, nor was it ever my opinion, that pink urinary sediments consist of the alkaline purpurates. My opinion is, and always has been, that these sediments consist (usually and essentially) of the lithate of ammonia; that this lithate of ammonia is *merely tinged, or dyed*, with two kinds of colouring matter; the *yellow colouring* matter of the urine, which possesses peculiar properties; and the *crimson colouring* matter, derived from some alkaline purpurate. When the yellow colouring matter predominates, the sediments are yellow; when the crimson exists alone, or predominates, (which is very rare, as I have only seen two or three well-marked instances in twenty years,) the sediments are of a pink colour. When the two exist together, which is the usual state of things, the sediments are more or less of a red or lateritious (not pink or crimson) colour. That the crimson portion of the colour is derived from an alkaline purpurate in all instances, I have no doubt; that this purpurate is often formed by nitric acid, I think is very probable; but that it is *always* so formed, I do not believe; as the lithic acid can be converted into a purpurate in many other ways besides the agency of nitric acid.

The above has been most explicitly stated in my volume on Urinary Diseases; but notwithstanding this, I have reason to believe that many have fallen into the same error as Messrs. Brett and Bird; and that the subject is by no means so well understood as it ought to be by medical men in general. I am happy to see the increased attention paid to animal chemistry; without a knowledge of which, in many diseases, the physician is likely to do more harm than good. It only remains to conclude with a piece of advice to my young opponents, which, as coming from an *old* chemist, I hope they will excuse—viz. always to obtain a clear and accurate knowledge of the points under discussion, whatever these may be, before they begin to make experiments; a practice which, from long experience, I can assure them will save much trouble.—I am, sir,

Your obedient servant,
Wm. Prout.

Sackville-Street,
July 29, 1834.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'Alembert.

On the Certainty and Safety with which the Operation for the Extraction of a Cataract from the Human Eye may be performed; and on the Means by which it is to be accomplished. By G. J. Guthrie, F.R.S. Surgeon to the Westminster Hospital, and to the Royal Westminster Ophthalmic Hospital, &c. &c. &c.

The object of this little work is so specifically set forth in the title page, that we need not say a word more in its explanation; and we shall, therefore, limit ourselves to an account of the arguments and observations with which the author supports and illustrates his views. Mr. Guthrie is of opinion that the directions given, even by the best practical writers, with respect to the mode of performing the operation of extraction, are not sufficiently precise. This particular expedient, we are told, ought not to be had recourse to except in cases of hard cataract, which may in general be distinguished from others with sufficient ease. When this has been done, and the operation determined on, it has been considered necessary to have recourse to a good deal of preparation. Mr. Guthrie, however, states,

that he regards the same precautions only to be necessary as those we employ when any other important surgical operation is required, particularly about the head—viz. moderate antiphlogistic treatment for a week, and a couple of doses of gentle physic; to which may be added, if the patient be disposed to fulness, the abstraction of twelve ounces of blood from the arm the evening preceding the operation. If gout be impending, it ought to be allowed to pass over; and if there be determination to the head, the patient ought to be cupped. It has been customary to operate on only one eye at a time; but Mr. Guthrie prefers performing the extraction on both at once. He says—

"There is a difference of opinion among surgeons, whether both eyes should be operated upon at once, or not. I am now decidedly for doing both operations at the same time, in all favourable cases. Formerly, I preferred doing first one, and when the patient had quite recovered, the other; and I recommended this proceeding from the common honesty which all surgeons should have, believing it to be for the advantage of the patient; but then I was not so certain of success as I am at present, and consequently wished to save even the smallest chance. I must confess that I did not then go to the performance of an operation with the same confidence that I do now, for I only expect a total failure as a matter of accident—rather as an untoward circumstance that may occur, than as one which is likely to occur; and as such an untoward accident would scarcely be possible, with respect to both eyes, I recommend that they should both be operated upon at the same time, when one treatment only will be necessary. If an accident should occur, and a very vigorous or prolonged after-treatment be necessary, the patient, whatever may be the result, will not readily submit to a second; and if an elderly person, will not be in a state to submit for a great length of time afterwards; and if that second operation should be unlucky, requiring also a vigorous after-treatment, the loss of health, and ultimately that of life, may be the consequence. Elderly persons can generally bear a vigorous treatment *once* well, and recover from it completely; but they cannot often bear and recover from it *twice*. It is, however, a matter of election which the patient should make himself, after considering it well. I should prefer having both done at once in my own case, if I were so afflicted, provided I had due confidence in the operator."

The directions for performing the operation being, in our author's opinion, defective, he proceeds to give the following minute and perspicuous details upon this point; and as they constitute the very end and aim of the book, we shall give them in his own words:—

"The operation of extraction should always be done, as a general rule, *upwards*—that is, on the upper half of the cornea; and this regulates the position of the patient and surgeon. The patient should be placed opposite a single clear steady light, without sunshine, and a northern light is the best, although it is not of any consequence what light it is, provided it is unaccompanied by the beams of the sun. He should be seated in an arm-chair, the back of which should be low enough to support the head when gently inclined backwards. A night-cap fitted exactly to the head, so that it cannot move, should now be put on; the fore part should be turned up, if it comes too low down on the forehead, and the middle of a light thin spongy kind of linen bandage, two inches and a half wide, should be pinned or sewed to the centre of it behind, ready for use.

"For the operation on the right eye, the surgeon should place himself behind the patient, and he will usually find it necessary to stand on a stool, in order to raise himself to such a height that he may readily lean over, and have his hands at perfect ease; and in that position and distance from his own head or chest which is most convenient to him. The patient's head being a little inclined backwards, and duly, although gently and comfortably, supported by the cushion, or back of the chair, the surgeon leaning over from behind, brings the *two* fore-fingers of the left hand over the forehead gently down on the eyelid, and raises it up slowly and tenderly, so as to fix it ultimately against the upper edge of the orbit; and to be able to retain it there so perfectly with the end of *the* fore-finger only, that the patient cannot lower it, or close the eyelid, by any effort he can make. He should also be able to do this and to make a little pressure on the eye-ball,

in order to fix it at the moment the incision is begun. As soon as the index finger is in this position, the second finger leaves the upper, and lowers the under lid, and fixes it against the edge of the orbit below. The eye is thus completely exposed, and may be almost fixed between the two fingers. To do all this well, requires a certain degree of practice, but which is very easily acquired. It must be done very gently, very tenderly, and without giving pain, or almost uneasiness. The error usually committed, is in using too much force with the extremity of the finger, which gives pain, and makes the patient swerve; and it is an error of such great importance, that the surgeon must practise this part of the operation until he feels that he does it as a matter of art, not of force.

"The left eye may be opened, and fixed in a similar manner; or the surgeon, standing before the patient, raises the upper lid with the side of the fore-finger of the left hand, and depresses the under lid with the thumb, the hand being over the nose. The pressure of the fore-finger tends to fix the eye at the same time, and to render it as immoveable as possible; and this mode of proceeding I generally adopt in preference, for the left eye."

The object, or at least one of the great objects, accomplished by retaining the eye-lids asunder, is that of depriving the organ itself of its sensibility; for Mr. Guthrie goes on to show, by various arguments, that it is only while the act of grasping on the part of the eye-lids is performed that sprain is liable to be produced by the contact of a foreign body. This being done, and the eye fixed as directed, the sensation is so dull that nothing beyond uneasiness is experienced. The knife recommended is spear-pointed, and blunt on the back, and it ought to be as wide as the perfection of the point will admit; but after all, it is "essentially a narrow knife." Great attention must be had to the point of the instrument, which the surgeon ought to try upon a piece of fine leather; through this it ought to pass without the slightest sound; if any, even the least, be heard, the knife is unfit for use.

Another circumstance worthy of attention is, that the author strongly recommends the application of belladonna before the operation; and therefore entirely dissents from those who object to it, meeting their arguments by various counter-statements. These observations having been premised, we now proceed with the operation thus:—

"The pupil being fully dilated (and it is of no trifling importance to know that it ought to be dilated), the eyelids being separated, the eye fixed, and the knife ready, the surgeon commences the incision of the cornea, by introducing it near its edge, or junction with the sclerotica; some say one quarter of a line, some half a line, others a line. The intention of all is the same, viz., that it may be near to the edge of the cornea, in order to admit of the opening in it being as large as possible; it ought not, nevertheless, to touch the sclerotica, as parts of the same kind unite more readily than those which are of a dissimilar nature; and it should also be so much above the plane of the iris, that it will pass readily across without touching it. The upper half of the cornea must be completely divided, and if any thing rather more than less, so that the point of the knife must be entered at nearly the least possible distance below the horizontal, or as it is sometimes pedantically called, the equatorial line of the eye.

"The manner of entering the point of the knife is disputed. The cornea being composed of several layers, constituting a substance of a certain thickness, there is some danger of passing the knife between the layers, and not across the anterior chamber of the eye in front of the iris, if care be not taken that the cornea is fairly penetrated in the very first entering of the knife; which accident may happen, if the anterior chamber is small, and the iris is close to the cornea. In order to avoid this error, modern authors (I believe, without an exception), recommend that the knife should be entered in the direction of the iris, as if the point were to be carried directly against it; but that as soon as the cornea is penetrated, and the point is in the anterior chamber, the handle of the knife is to undergo a sort of almost imperceptible depression towards the temple, by means of which the blade is to be placed with its flat surfaces parallel to the iris, across the anterior chamber; and it is said that the more quickly this is done, the less chance is there

of the escape of the aqueous humour. To all this I object in the most decided manner: the gentlemen who think they do as they say, are, I believe, mistaken; and when they do, they do nothing but mischief. The very turning, or attempting to turn the knife, generally leads to the escape of the aqueous humour, the very thing they want to preserve, and to all the evils it is most desirable to avoid. The knife should, in my opinion, be held flat, and the point is to be introduced steadily in the same direction at the proper place; and if the operator can neither see nor feel when it has penetrated the cornea, and is in the anterior chamber, the sooner he abandons operating the better. If a man has eyes, he can see when the point of the knife has entered the anterior chamber; and if he has fingers, he will feel when the resistance offered by the cornea is overcome. The young surgeon, instead of practising these manœuvres, which are worse than useless, should practise on sheep's eyes, until he has acquired that tact which will enable him to know when his knife is in the right place."

All those who have ever seen this operation, or that of couching, performed, must have observed, that at the moment the knife has penetrated the cornea, and is about to be carried onwards, the patient will often move the eye involuntarily. For this the surgeon must be prepared, and he must wait till the spasmodic action of the muscles has ceased, after which he must fix the eye as much as possible with the fingers.

"If the turn takes place just after the point of the knife begins to advance across the anterior chamber, none but a calm, deliberate, and experienced operator will be able to finish the operation successfully. If he hesitates, the aqueous humour will escape, the point of the knife must be entangled in the iris, which immediately falls forward against the cornea, and the surgeon has nothing to do but to withdraw his knife, let the wound heal, and try again a fortnight or three weeks afterwards. If, on the contrary, the turn inwards of the eye only takes place as the point approaches the opposite side of the cornea, the case is different, and a steady operator will readily complete this part of the operation by carrying the knife onwards, even if the point should be out of sight; and if he can carry the point through the cornea, which is called completing the punctuation, he is safe. If the accident should not occur, and the eye remains steady, the knife will in all probability go safely across the anterior chamber; but if the hand of the surgeon wavers for an instant, or the patient endeavours, however unwillingly, to retract the eye, on feeling that it is wounded, the pressure caused by this effort of the muscles forces out the aqueous humour, and the point of the knife in either case may be even enveloped by the iris. If all these evils should be avoided, and the knife has swiftly, as well as steadily, crossed the anterior chamber, it may not be able to penetrate the cornea on the opposite side. This entirely depends on the perfection of the point of the knife, and therefore on it the success of the operation rests. The inner layer of the cornea, or membrane of the aqueous humour, as it is improperly called, is much more dense and firm than the outer one, and the knife, which will easily enter, will not always be able to come out on the opposite side. I have, in some cases, been obliged to use such a degree of force, (and have even then not succeeded), that I have almost felt assured that the knife, when it did perforate the part, would inevitably go on, and stick into the nose. This greater degree of denseness of the cornea must always be reckoned upon, and in every case a proportionate degree of power must be applied, as the point of the knife touches the inside of the cornea, that it may go through without any delay; at which moment the aqueous humour frequently escapes in part or in totality. In order to prevent this, and the evil consequences which may ensue, there must be no hesitation in this part of the operation. From the moment the knife enters on the one side, it should be carried swiftly, but not hastily on, with a steady undeviating progressive motion of the thumb and fore-finger, or two fore-fingers, until it passes through the opposite side, and the centre of the blade is in the middle of the anterior chamber, exactly opposite the pupil. This is the acme of perfection in operating, and must succeed: there is scarcely any thing can prevent such a case from turning out well. I am aware that it cannot always be done, and that it suffices to have the knife fairly through on the

opposite side, but then the after-steps are not so certain. If the width of the knife be compared with the width of the upper half of the cornea, it will be seen that they are nearly equal; and as the flat sides of the knife lie one against the cornea, the other against the iris, the latter part is kept back, and can scarcely get before the edge of the broad part of the knife; indeed, it cannot do so; and the knife is either carried on with the same motion, so as to cut its way out; or, if the angle of the eye-lids or the nose, or any accidental motion of the eye upwards, prevent it, the knife must be made to cut its way out, not by one forcible effort, but by gently acting with the blade first towards the point, and then with the heel, so that the inner portion of the upper part is divided by the blade near the point, the outer by the heel; for the cornea is often very tough, and requires this sort of zig-zag or sawing motion to be made before it will yield. The section of the cornea, thus completed, should be one perfect half, and a little more, rather than any thing less; and the incision should be at an equal distance from the sclerotica, or from the edge of the cornea, all round; but this is not often accomplished, and is of little consequence, as the cicatrix is out of sight."

The chief difficulty which presents itself in this part of the operation occurs from the escape of the aqueous humour; for by this the iris is apt to fall forward against the cornea, and of course against the knife. If the escape of the fluid does not take place till after the knife has crossed the anterior chamber, but without penetrating the cornea, it must be carried on, and the object be completed, for the iris is not liable to be cut by this continued progressive motion of the instrument. But, suppose the contrary to happen, according to our author the evil has been much overrated. His words are—

"There is now another secret to be disclosed, of yet greater importance to the young operator; it is, that the effect of an injury to the iris is very greatly overrated, and that if the operation cannot be completed without injuring it, the injury must be committed. The real truth is, that *no good operators* in this great city of London do otherwise. When the iris bulges much over the edge of the knife, it is often not possible to get it quite clear, by any effort, exertion, or dexterity, on the part of the operator. If any man says otherwise, I do not hesitate to say that he is in error, and that his own operations, if he has ever done any, will show it."

After each part of the operation, the upper eye-lid should be allowed to drop, and the patient to rest for a little. After the cornea has been divided, the capsule of the lens is to be attached, by introducing a hook under the flap until it reaches the pupil. The end of the hook forms a right angle with the shaft, and is to be introduced on its side with the point backwards, so that it cannot catch any thing until it has arrived opposite the centre of the pupil; the point is then to be turned towards it, and several slight scratches made into the capsule, and then withdrawn. Sometimes, however, this is not sufficient; and we are told that—

"When the lens does not begin to move from its situation, after the capsule has been torn, the surgeon must satisfy himself that the incision in the cornea is sufficiently large; or if it is not, he must enlarge it. He should be equally certain that the capsule is sufficiently torn through, by repeating that part of the operation; when, if there be nothing to impede the advance of the lens, it will be seen to rise from its situation, its upper edge gradually passing through the pupil and sliding over the iris, unless the external incision is not sufficiently large, when it may protrude the iris before it; at which moment it should be assisted by the hook, or curette, and on its expulsion the lid should be allowed to fall. If the flap made in the cornea should by accident be turned downwards, it must be replaced in its situation by the end of the curette, when the lid is again raised.

"The lens does not, however, always come out so easily and regularly. It sometimes happens that, instead of rising up by its upper edge, the vitreous humour, which appears black because it is transparent, and allows the black pigment beneath to be seen through, is perceived pushing forwards between it and the iris. Nothing can prevent a portion of this vitreous humour being protruded or expelled; and no attention need be paid in order to obviate it, for it cannot be done. But attention must be paid to the fact, that its expul-

sion under pressure will not in general be accompanied by that of the lens, which, having lost its support, will sink down towards the bottom of the eye, and must infallibly cause its destruction by inflammation, if not removed. The surgeon, aware of this circumstance, and knowing that pressure of any kind, that even the mere action of the muscles, will cause the expulsion of the vitreous humour without the lens, passes either the large or the small hook under the edge of the lens, through the vitreous humour, which begins to escape when its inclosing membrane is pierced by the hook, the point of which is to be fairly stuck into the under part of the lens, and thus drawn out. A portion of vitreous humour must of course escape; it cannot be prevented; it was inevitable from the first; but the great object, the extraction of the lens, has been attained. If the surgeon hesitates, and does not calmly and steadily introduce his instrument, and hook the lens at once, the vitreous humour begins to escape, the lens sinks, and the eye will be lost, if he does not instantly pass the hook through the pupil, and hook the lens, as he would catch a fish with a landing hook. There is no alternative; it must be done, or the eye will be destroyed."

Sometimes, by a spasmodic action of the muscles, not only the lens, but a great portion of the vitreous humour is expelled. Mr. Guthrie once saw this occur when the whole of the vitreous humour appeared to have been lost, yet the patient recovered a tolerable degree of sight. "The loss of the vitreous humour has been a surgical bugbear in doing this operation."

The last important point in the performance of this operation is to bring the edges of the cornea into nice apposition; and this ought to be very carefully done.

In the directions for dressing the eye, excluding light, and subduing subsequent inflammation, we find nothing particular to extract; and, indeed, we have already exceeded the limits we intended, in order that we might do the greater justice to this subject. The brochure is an interesting one, and will amply repay the time spent in its perusal.

MEDICAL GAZETTE.

Saturday, August 2, 1834.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

HINTS REGARDING CHOLERA.

THE present is the third season during which the metropolis, and various other parts of England, have received an annual visit from cholera; and every point connected with the history of this singular epidemic continues to be viewed with an interest commensurate with the importance of the subject. It is one, indeed, which involves the public health and excites the public interest to a much greater extent than any malady which has prevailed within our times: nor has any thing even approaching to it been experienced since the frightful epidemic of Northern Europe, more particularly of Dantzic, in the early part of the last century. Hitherto no one has been successful in the attempt to reduce to any thing like order, the multifarious,—the capricious,—and often apparently contradictory phenomena of cholera; and M. Villermé, in his late valuable work on epidemics, makes an especial exception in the application of his observations with regard both to the "black plague" of the fourteenth century, and the "blue plague" of the last few years. Still, there can be no doubt of the propriety of noting and calling attention from time to time to the circumstances connected with it, as, by the gradual accumulation of facts, we may at length be able to detect and turn to account some of the laws which, though hitherto undetected, doubtless exist and regulate the march of the disease.

There seems sufficient reason to believe that cholera, however it may at its first invasion appear to disregard cli-

mate and temperature, is yet, in its subsequent visitations, not irrespective of seasons. In 1829-30, it flourished in the northern parts of Europe despite the rigour of a Russian winter. It is on the 21st December of the latter year that we first find "cholera" in our bills of mortality; yet so little progress did the disease make for some time after, that during the whole of the year 1831 the total number of deaths from this new addition to the catalogue of diseases, only amounted to 48! It was during the following winter that the disease in its more malignant, and, in some essential particulars, its new form, was introduced; and though the progress it then made was such as we have no record of the "cholera morbus" having ever before made among us at the same season of the year, still the influence, whatever it may be, which was, as it were, engrafted upon this our portion of the globe, was not fully developed till it was fostered by the heat of summer.

In 1831 we have seen how small was the number of deaths which occurred during the six warmest months; but in 1832 not fewer than 1006 fatal cases of cholera are recorded in the Bills of Mortality for July alone—380 of these having occurred in one week. In August they fell to 777, after which they progressively diminished till the end of September, when they became very rapidly extinct*. The temperature at this time was not particularly elevated, averaging about 65 degrees, but the vicissitudes were frequent and considerable, and in one instance very remarkable, viz. at the end of the first week in July, and just before the epidemic underwent a marked exacerbation, the thermometer ranged within twenty-four hours from 40° to 71°, a difference of not less than thirty-one degrees. The prevalent state of atmosphere during the above period was cloudy, with frequent rain, and a good deal of thunder.

In 1833 the disease again shewed itself in July, during which month there occurred 181 cases; while we have 853 in August, the epidemic diminishing in September, and disappearing more rapidly than the preceding year. The weather during its prevalence was very similar to that of the preceding year.

In 1834, the month of July once more gives us the "blue plague," but only 37 cases; the weather having been warmer, and much drier than during the preceding years; and the disease increasing on the atmosphere becoming damp.

Thus it would appear, so far as our present experience goes, that cholera in its worst forms has a disposition to shew itself towards the months of July and August; and this corresponds with the account given by Sydenham of the old English cholera, which he says comes at the end of summer and in autumn, as regularly as vegetation comes in spring. But though we are distinctly of opinion that something has lately been added to those causes which have heretofore produced those bilious fluxes to which Sydenham especially alludes, yet we cannot deny, nor do we wish to do so, that the phenomena of the "blue" cholera exhibit a closer affinity between the two forms of disease than they at first displayed, or than we had anticipated would be found to be the case. Not only does the same season, and the same state of atmosphere, appear equally favourable to both—supposing them specifically distinct diseases—but the same immediate exciting causes would seem also to give rise to both. This last is by far the most important circumstance to which we have to allude; for while the other points are speculative only, this becomes practical. It has on former oc-

* This and the following can only be considered as an approximation, owing to the manner in which the Bills are made out, and to their not comprehending the whole town.

casions been observed, and certainly was never more conspicuous than during the present season, that scarcely any instance of the disease in its malignant form appears which is not traceable to some exciting cause, of a nature which for the most part might be avoided. Thus it will generally be found that the party attacked has laboured under more or less looseness of the bowels for several days. Many attach no kind of importance to an apparently slight attack of this kind; and the fact is, that those preliminary warnings differ in nothing, which the individual can distinguish, from similar affections which at other times pass off without inconvenience. Misled by this analogy, and deluded into a false security, they think merely that something has disagreed with them, and that it will "work itself off;" or perhaps they assist this supposed salutary result by taking a dose of tincture of rhubarb, or a few of Morison's pills. Experience, however, has fully proved, that at this season of the year, particularly since the introduction of cholera, laxatives, even of the mildest kind, must be given with caution, and that any degree of diarrhœa requires to be speedily and efficiently checked. This is the practice now generally adopted by intelligent persons; and while we venture to say that bowel complaints have not, at any period for several years back, been more rife than during the last fortnight, it is at the same time undoubted, that never since the first visit of the cholera, have so few, as yet, passed into that disease.

But in some instances the symptoms take on a character of urgency, without any preliminary warning, hurrying the patient, within a few hours, into an almost hopeless condition. According to our own observations, when this has been the case, one of two circumstances has occurred. Either the individual has applied some internal, or been exposed to some external cause, superadded to the general atmospheric influence. By far the most common exciting cause of this nature, is some error in diet—an imprudence which proves equally pernicious among the high and low; for its effects differ but little, whether they be produced by white-bait, by tainted lobsters, or the putrid salmon, now so plentifully hawked about the streets — whether by pines or cucumbers—iced champagne, or "Hodgson's cordial gin." This also is a point on which the admonitions of the medical man are extremely apt to be disregarded — most persons contemning, if not openly ridiculing, the idea of that doing them harm at one season which they take at another with impunity. The truth is, however, that the digestive organs appear not to be capable of doing so much work in the hot damp weather of our autumnal months as at other times; and if the same demand be made upon them, they will leave part of it undone. Besides, to pass from reasoning to facts, many of the suddenly fatal cases we have found to have followed within forty-eight hours after some indulgence in eating or drinking, by which the stomach had obviously been overcharged. Of the "external circumstances" to which we refer, our allusion is more especially to residence in a vicinity where the drainage is imperfect. On going into houses to see patients labouring under cholera, we have several times smelt out this cause very speedily, the drains being often very offensive. Besides, a reference to a map of London which contains the elevations of the different localities, will show at a glance that those situations in which the disease has been most frequent and most obstinate, are precisely those which are lowest, and the drainage from which is consequently most deficient.

To the medical reader, we fear, these

remarks will present no novelty; but they may not be altogether useless, if they direct attention to the practical suggestions to which they obviously point; and if they prove the means of inducing professional men to enforce on the minds of the public (especially during the present month) the necessity of applying for advice when any, the slightest, loosenes of the bowels occurs —of avoiding with religious scrupulousness any indulgence at table—and, as far as circumstances permit, of shunning the exhalations of stinking drains.

ANNIVERSARY OF THE PROVINCIAL ASSOCIATION.

The numerously attended meeting at Birmingham, of which, through the kindness of a correspondent, we were enabled to give an early account last week, seems to have gone off with universal satisfaction and applause. Dr. Conolly's retrospective address, we understand, was very splendid; and the report of Dr. John Johnstone, the President's, remarks, has pleased us much in the perusal. That gentleman placed in a very strong point of view the importance of establishing at the present period such associations as this: "A co-operation," as he called it, "the best calculated that can be devised to extend the limits, and increase the usefulness, of the healing art; for it is only by associating with our own the knowledge of others, and, as it were, amalgamating, concentrating, piling facts together, that we can arrive at great results. All great men have become so by taking advantage of the knowledge of preceding ages; and it is the ignorant empiric alone, who pretends that he only possesses all the learning of the world. A physician who would, by his own experience alone, assume that he should become consummate in his art, would require a life of several ages to do so, besides the possession of the most sublime genius. Study, indeed, may furnish us, in a very short time, with the discovery of ages; an hour is sufficient to inform us of truths which were purchased with the care and trouble of years; but solitude, though the school of genius, is a troubled ocean; the communication of knowledge in associations like ours, pours all the rivulets of information into a general stream, which fertilizes the soil through which it flows."

From the report of the Council, read by Dr. Hastings, we learn that the number of members now belonging to the Association is 450, being an increase of about 140 since the preceding year. It is in contemplation to establish a library, and a Medical Benevolent Charity, on the model of that of Birmingham, in connexion with the Association. Three honorary members have been elected, viz. Sir Astley Cooper, Sir Charles Bell, and Dr. Abercrombie, of Edinburgh.

The next anniversary meeting is to be held at Oxford, under the auspices of Dr. Kidd; and Dr. Prichard, of Bristol, is appointed to deliver the annual retrospective address.

SURGICAL REPORTS FROM THE LONDON DISPENSARY.

By R. R. Robinson, Esq.

Surgeon to that Institution.

Case I.—*Inflammation of the Fascia of the Fore-arm after Venesection.*

A young woman of strumous habit was admitted 10th January. She had been out of health many months; a surgeon had made attempts to bleed her in the median cephalic vein of the left arm, but from its deep situation, and the mass of fat covering it, it was not opened; the orifice was rather large. (Her arm festered when she was bled some years ago.) The wound was not uneasy for the first three days after the operation; but, contrary to orders, she went about her work, that of a housemaid,; the arm consequently became swelled, stiff, and painful; notwithstanding which, on the fifth day after the operation, she scoured a cold, damp passage. On the sixth the arm was more swelled and painful, and rather tense. The pain was augmented by firm pressure; not affected when slightly touched. Integuments of the fore-arm and lower part of arm hot, but quite white. The wound was indisposed to heal, had a strumous pale appearance, and discharged serum copiously; she had passed several restless nights, and was feverish. Bowels open; tongue white; pulse 96. Twelve leeches were applied partly above, partly below, the wound, where the tension was most considerable. A light bandage with Gou-

lard wash was first tried; but this increasing the pain, a poultice was substituted. The leeches bled very freely, and gave great relief. Dover's powder with calomel were taken at night, and saline antimonials every four hours. A little pain and tension remaining over the biceps muscle, eight leeches were applied two days subsequently, with an equally good effect. She was kept in bed and on low diet three days. The poultice feeling heavy, warm water and lint (with oil silk externally) were substituted with great comfort. Infusion of gentian, with liquor potassæ, were taken instead of the saline, —the fever having been removed,—under which she slowly regained strength, the pain subsided, and at the end of three weeks from the operation the wound was entirely healed, all stiffness was gone, and she had the perfect use of her arm.

Remarks.—In this case we have an illustration of inflammation confined to the fascia, attended with well-marked symptoms—viz. a sense of tightness, heat, swelling, and pain, augmented upon *deep*, but not upon slight pressure; *pallidity* of the integuments, and fever.

It is probable that in the operation the fascia was punctured; but as the patient was free from pain the first three days, there is reason to believe that she would have suffered but little had she not incautiously exposed herself to cold, and made improper use of her arm.

The circumstance of the orifice festering after venesection, on a former occasion, clearly shews (what I think is generally manifest where ill consequences follow that operation) that the constitution was much in fault; for when we see some persons shew great irritability upon the slightest scratch, and suffer extensive inflammation from the most trifling wound, while others in far more serious accidents exhibit nothing untoward, and speedily recover, it is impossible that the results can be solely, or in great measure, attributed to the injury inflicted. It is nevertheless undoubtedly true, that punctures of tendinous structures are attended with greater irritation than more extensive wounds of them.

This case is gratifying, from its yielding so quickly to the antiphlogistic treatment, and from subsequent stiffness being prevented, which I attribute in great measure to the arrest of inflammation before the deposition of coagulable lymph. The application of lint dipped in warm water, and covered with oil silk *, afforded great relief in this case, and I have seen it do so in many others. It possesses the advantages of a poultice—namely, heat and moisture, without its weight, and at the same time gives support to weakened vessels, so frequently advantageous in the treatment of inflammation.

Case II.—*Inflammation of the Fascia of the Leg cured by Incision and Pressure—Sloughing Ulcer—Hæmorrhage from the Bowels.*

James Walker, æt. 37, admitted September 14th; a sickly man, long subject to extensive bleeding from the bowels, which now continues; much fatigued yesterday from a very long walk. There is an extensive ulcer, in a fœtid sloughy state, on the right leg; left leg enormously swollen, heavy, immobile, and tense. The integuments about the calf very red, where there is a decided feeling of fluctuation, as if from matter under the fascia. From the extreme tenseness of the fascia here, and the doubt of an abscess under it, I made a free incision through the integuments and fascia, in the centre of the calf, which afforded relief; but no matter escaped.—Cat. panis.

Vespere.—Poultice imperfectly applied; much easier; much less tension, but more inflammation about the ankle. A bandage applied from the toes to the knee, and kept constantly wet with Goulard wash; some fever and much debility.

Magn. Sulph. ʒss.; Ant. Tart. gr. ¼, ter die.

Sept. 15th.—Slept well; considerably easier; leg much less swelled; and though last night it smoaked greatly upon applying the wash, it is now cool, less heavy, and the bandage which was last night applied is quite loose; less blood discharged from the bowels.

16th.—Still better; leg very much less inflamed, and greatly diminished in size; can move it about with facility. Fever subsiding; other leg well.

Moderate nourishment. Repetantur omnia.

21st. — Inflammation entirely gone; wound healing fast; ulcer on the other leg quite clean. After this, the left leg recovered its strength completely; the hæmorrhage decreased, the ulcer of the opposite leg slowly healed up, and the recovery was ultimately complete.

Remarks.—In this case the constitution was clearly in fault, as was evinced by the hæmorrhage from the bowels, and the appearance of the ulcer on the opposite leg. By this is illustrated the deceptive feel of fluctuation which some parts give, and the good effects of incision in diminishing tension, if matter does not follow. Supporting the weakened vessels by a

* I do not mention this as a new remedy, for the good effects of it have been published in the Edinburgh Medical and Surgical Journal some time since, though I had used it before I perused that paper.

bandage, at the same time that the increased action was kept under by a cold and sedative lotion, was attended with very marked good effects.

Case III.—*Inflammation and Suppuration of the Fascia around the right Shoulder-joint, discharging spontaneously.*

Joseph Flax, æt. 15, a baker's boy, strumous constitution, subject to draughts, admitted Feb. 7th. Has had neither blow or strain to his knowledge; felt an uneasiness in the right shoulder a week ago, and has since been unable to move it. This shoulder is much hotter than the other, but not red; it is also much more round and full. Upon motion, considerable pain is produced; as also when pressure is made upon the scapula and deltoid, when the head of the humerus is pushed against the glenoid cavity, and when the deltoid is grasped; slight rigors occasionally; some fever; bowels regular.

Hirud. viij.; P. P. ℈i.; Mist. Cath. Ant. ter die; Lot. Plumbi.

Feb. 9th.—Leeches bled freely and afforded a few hours' relief; the pain, however, recurred last night more than ever (it is generally worse at night); shoulder-joint more full, and tender, and immobile; integuments over the posterior part of the arm-pit tense: there is here a disposition to form an abscess; glands in the neck and axilla enlarged and tender; fever continues; nights restless; pulse regular; tongue white, thickly coated; several motions.

Repetantur Hirud. xij.; et Mist. Hydr. c. Cretâ, gr. v. o. n.

11th.—Arm more swollen, and very hot; the least motion exquisitely painful; an indistinct fluctuation in the supra-spinal fossa; glands in the arm-pit still tender; fever continues. Goes to his friends at Hackney to-day.

March 9th.—I was this day informed that an abscess took place upon the top of the shoulder, burst on the 23d ultimo, and discharged a considerable quantity of matter; all the constitutional irritation and pain subsided; the matter continued to discharge in smaller and smaller quantities until the 2nd instant, when it stopped entirely. He has in a great measure regained the power of moving his arm, and is fast getting well.

Remarks.—The application of leeches and cold lotions were productive of less good effect in this instance than usual. It is also worthy of notice, that although the abscess was allowed to burst of itself, it did exceedingly well—a fact contrary to general experience.

Case IV.—*Inflammation and Suppuration around the right Shoulder-joint, attended with pulsation, opened spontaneously.*

Anne Cash, æt. 42, admitted April 12th. Ill a month, with pain, redness, and swelling of the right shoulder. A physician, without seeing her, prescribed a blister to the nape of the neck, which was instead applied to the shoulder; after which the pain increased, but the redness disappeared, and a circumscribed tumor formed in this situation, which clearly contained matter, and points towards the lower part: there is a pulsation propagated to it from the subclavian artery; the pulse of the radial artery on this side is in no respect different from that on the other; weak and hectic; no appetite; nights restless; will not suffer the tumor to be opened.

Cal. gr. i.; Pulv. Dov. gr. viij. o. n.; Dec. Cinch. c. Acid; Sulph. Dilut. M. x. ter die.

April 14th.—Abscess broke yesterday, and discharged a full pint of matter. There remains an irregular hole at the lower part, which now discharges a little healthy pus and some air upon pressure; the surrounding parts are consolidated considerably; rests well; is stronger.

After this, nothing untoward happened: she gradually regained strength, the orifice closed, and the motions of the shoulder-joint were restored.

Remarks.—This case affords another illustration of the successful issue of spontaneous opening; but neither of these did better than those in which the lancet was resorted to; and the patients suffered pain for a somewhat longer time. It may be doubted whether the fascia was affected in this case or not; it is certain that the skin and subjacent cellular tissue were implicated. I am, however, inclined to think the collection of matter was under the fascia, from the extreme tenseness of that membrane, and the pulsations which the tumor derived from the axiliary artery, by which the case was somewhat perplexed. Two other facts are observable in the above, viz. the inexpediency of prescribing for a patient without seeing the case, and the inutility of a blister, as far as one example goes, applied over an abscess.

Case V.—*Extensive Inflammation and Suppuration of the Fascia after Parturition, cured by Incision and Bandage.*

Anne Fordham, æt. 19, admitted the 1st of March; a strumous subject; was confined of her first child six weeks ago; since which she has experienced pain in the calf of the right leg, which has lately increased. The integuments are quite white and hard, the leg is very much

swollen, and there is a distinct fluctuation underneath the fascia. There is fever of the hectic character, and much debility. An incision gave vent to three pints of good pus, with a few shreds of lymph and some clots of blood. A bandage was applied with goulard lotion, the bowels were opened by castor-oil, and she took 2 grs. of sulphate of quinine three times a-day. The orifice was kept open.

March 20th.—The limb was carefully pressed daily, which gave her much pain, and generally gave exit to ℥vi. of pus: the quantity, however, diminished by degrees. She gains strength. There is now no discharge. The wound is entirely well, and she can move the leg. Dismissed, cured.

Remarks.—This is an illustration of copious effusion of matter under the fascia after parturition, not I believe a very common occurrence, and is a good example of the effects of early opening and subsequent bandaging.

Case VI.—*Inflammation and suppuration of the fascia of the thigh from a blow, cured by incision, goulard lotion, and pressure.*

Edward Croul, æt. 14, admitted November 16th, fell down and bruised his right knee fourteen days ago; took no notice of it, until kicked, a short time back, by a boy in the groin; immediately after which he experienced pain on the inside of the knee, and inability to move the thigh. He was taken to an hospital, where 72 leeches were applied at different times, but was removed from there on the 14th instant. The whole of the right thigh and leg are very hot, swollen, red, painful, and indurated. There is a distinct fluctuation over the inner side of the knee; fever; bowels obstinately confined. An incision was made into the ham through the fascia, and a full pint of genuine pus removed, which had bagged chiefly over the inner condyle, where there was a hollow on its removal. Inguinal glands enlarged and painful.

P. P. ℈i.; Mist. Cath. Ant. bis die; Lot. Plumb; bandage; orifice slightly plugged.

November 17th.—Considerably easier: half a pint of pus escaped upon removing the plug; less fever; less bagging about the inner condyle, but pain there, when pressed; bowels well opened.

Rep. Mist. et Lot.

The bandage also continued, and a slight compress over the cavity.

18th.—Glands in the groin less tender; inflammation of the leg quite gone; limb considerably less swelled; copious purulent discharge from the wound. Can move the leg about much better, but cannot straighten it entirely; feverish.

Repetantur omnia.

22d.—Cavity all but filled up; discharge greatly diminished; fever gone; some pain in the knee, which is less contracted. Rep.

Soon after last report the cavity closed entirely; the wound healed: he regained the entire use of the limb, and was consequently discharged.

Remarks.—By this we learn the inefficacy of leeches, in some cases, in preventing the formation of matter; for, although they were applied almost as soon as the inflammation manifested itself, and were repeated to the number of *seventy-two*, pus was copiously effused. As far as my experience goes, (and I have made numerous trials,) I have found leeches oftener fail than succeed in arresting abscess, when once that tendency has manifested itself; I have, therefore, lately been disposed to apply them only when abscess threatens any important structure; under which denomination I would undoubtedly include the fascia.

Case VII.—*Extensive inflammation and suppuration of the fascia of the leg from a rusty nail—Knee-joint permanently stiff.*

Joshua Smith, æt. 23, admitted January 9th, a distressed weaver, ran an iron nail into the sole of the left foot a month ago, which came out by the side of the tendo-achillis; it was painful; a poultice was applied. In a week the pain subsided in the foot, and extended up the leg and knee to the groin, and he has been daily getting worse. The whole leg is more than twice the size of the other; rather hotter, but quite pale, except on either side of the patella; the integuments hard; the limb immoveable: any attempt at motion increases his suffering; occasional chills three weeks ago; none since; a distinct fluctuation on the outside of the patella; no appetite; nights restless; skin dry; countenance pale and wan; tongue tremulous and weak; pulse quick and irritable; bowels relaxed; great debility. An incision was made into the swelling on the outside of the knee, and a pint and a half of pus, streaked with blood, removed, with considerable relief.

Jusculum Bovini; Cat. Panis.; Pil. Opii. gi. o.n. Dec. Cinchon. ter die; limb placed in the straight position.

January 11th.—Leg much reduced in size; very much easier; has slept well; copious purulent discharge. Tongue clean and moist; pulse softer; bowels less relaxed; countenance less anxious.

February 27th.—The discharge continued considerable, but is now entirely stopped; the wound made by the scalpel

healed. Soon after last report the leg increased in size, and became excessively painful, so that he could not move it without excruciating agony, and continued so until the last few days. Latterly the swelling has again subsided, and he can move his toes. A small ulcer, which formed on the heel where the nail ran in, is now also healed. His general health is considerably improved.

March 5th.—Swelling and pain of leg quite gone; knee stiff. He is able to sit up.

19th.—Is able to walk about with crutches. From extreme poverty he is compelled to go into the workhouse.

I have since met him about frequently. His health is completely established, and he walks well; the knee remains, however, perfectly stiff.

Remarks.—This case shews the result of allowing coagulable lymph to become extensively effused, so as to glue the different textures together, and matter to collect. This man had been attended by another surgeon before I saw him, who told him his case was rheumatism, and *that he would do very well.* The consequence has been a permanent stiffness of the knee-joint, which, I think, might have been prevented, had more prompt means been adopted in the first instance.

Case VIII. — *Scrofulous Inflammation of several Joints, with Suppuration—Tubercles in the Lungs—Death.*

Henry White, æt. 52, a looking glass silverer, but unable to work for some time past; with the exception of a constant cough which he has had from birth, healthy until two years ago, when he complained of pain in his limbs and difficulty in walking, which increased. He went into a hospital, where, at first, he got better; but his complaint being considered chronic rheumatism, he was salivated and put into a *cold* bath; after which he was decidedly worse, and left the hospital. He then applied to the London Dispensary, and was admitted under Dr. A. Frampton, with whom I saw him. He was greatly reduced, confined to his bed, and unable to lie otherwise than on his back. The lateral and capsular ligaments of the left wrist-joint were destroyed, and the carpus dislocated forward; immediately above the joint was a circumscribed and distinctly fluctuating tumor; the skin covering it white and healthy. There was also another tumor, of similar appearance, but larger, in the centre of the biceps of this arm, and a third very large one over the right trochanter major. There was perceptible a distinct crepitus upon moving the left wrist. He never had *any rigors;* his general health was greatly impaired; he had no appetite; his nights were restless, and he suffered from hectic sweats. Among various remedies employed, were iodine, quinine, sarsaparilla, and soda. He grew weaker, notwithstanding. The right knee became affected; a similar tumor to that above described formed over the articulation of the index finger of the right hand, with the metacarpal bone. He latterly experienced severe pain in several joints, so that he could not bear to be moved. The integuments of the back sloughed, from pressure, to a considerable degree; he became more and more emaciated and feeble, and died on the 10th of May, 1833.

Dissection.—Lungs firmly adherent to the chest, by long old adhesions; one or two small grey tubercles in the upper part of the right lung; no vomicæ; two or three similar tubercles in the upper part of the left lung, which was more easily lacerable than the other; heart healthy; cartilages of the ribs ossified: upon the outer part of the sixth and seventh ribs on the right side, not far from the cartilage, was an abscess containing deep yellow thick pus. Abdominal viscera healthy; a pouch formed in the commencement of the transverse colon, by the contraction of the muscular coat upon some hardened scybala; structure natural.

The anterior part of the lower articulating ends of the left radius and ulna destroyed, together with the inter-articular cartilage, and part of the synovial membrane: what remained was preternaturally vascular, and contained matter which communicated with the tumor before-mentioned, which was found to be an abscess. The scaphoides lunare and cuneiform bones were bare, and deprived of their catilage.

The tumor in the centre of the arm was found also to be an abscess communicating with the shoulder-joint, the synovial membrane of which was gone, as also the cartilage covering the head of the humerus and lining the glenoid cavity.

The tumor over the trochanter major of the right thigh was composed entirely of pus, which communicated with the hip-joint. The synovial membrane here also was gone, together with the cartilage and ligamentum teres. The head of the bone was greatly diminished, and so soft as to be cut with a knife. The acetabulum was also denuded of its cartilage, but not perforated. A chocolate-coloured sero-purulent fluid issued freely from the joint when cut into. The toe on this side was turned inwards, and the limb was somewhat shortened, though the shortening appeared

greater than it really was, owing to the obliquity of the pelvis.

A considerable quantity of chocolate-coloured sero-purulent fluid issued from the right knee-joint when cut into. The anterior crucial ligament, the synovial membrane and cartilage, destroyed; the condyles of the femur and the head of the tibia denuded, and preternaturally vascular. From the tumor over the right index finger a similar pus escaped upon incision, and disease, similar to that already described, also existed here.

Remarks.—This case possesses interest, from its having been treated as rheumatism by a distinguished physician—from the number of joints affected by strumous inflammation, which sufficiently pointed out a constitutional, rather than a local, origin—from the injurious effects of mercury carried to ptyalism, and the inefficacy of iodine and a variety of other remedies.

CONNEXION OF THE UTERUS WITH THE PLACENTA.

To the Editor of the Medical Gazette.

Sir,

Considering that the following account of the appearances observed in the dissection of a gravid uterus may prove interesting, as it corroborates some of the important facts which have been brought before the public by Dr. R. Lee, on the connexion of the placenta with the uterus, I beg to forward it for insertion in the Medical Gazette.

Mrs. T——, aged 38, of rather a delicate and weak constitution, the mother of two or three children, in the last stage of pregnancy, and expecting her confinement every day, was attacked with cholera on the 21st of July, 1834, which proved fatal in eighteen hours, although the most prompt and judicious measures were employed by Mr. Murchison, a student at the school of Webb-street, to whose kindness I am indebted for the opportunity of making the *postmortem* examination, which was done fourteen hours after death, by Mr. Murchison, Mr. Tyson, and myself.

As the friends would allow only a partial dissection, we particularly examined the uterus; and on cutting through the abdominal parietes (which were extremely thin) the peritoneal covering of the uterus appeared more vascular than usual; the muscular coat was rather thinner than natural, and not so firm as it is generally found to be. The uterine veins or sinuses were very large, especially in the immediate neighbourhood of the placenta, towards which they appeared to run, almost like *radii*, from the circumference to the centre of a circle; they all passed longitudinally, and appeared to run for a long distance closely under the lining membrane of the uterus, before presenting their semicircular or oval openings, which were very distinct; and though great caution was used in tracing them, *none* could be found to enter the placenta, nor to pierce the tunica decidua uteri, which was beautifully seen passing behind the placenta. Vessels, apparently arteries as well as veins, of some size (three or four as large as crow-quills), were distinctly seen passing from the uterus into the placenta, directly from without inwards, and *vice versâ*, and crossing the uterine sinuses almost at right angles. The placenta separated very easily from the uterus, the vessels above alluded to offering but little resistance, the uterine veins offering none.

The mouths of these uterine veins were filled with a sort of duplicature or plug of the deciduous membrane, which was every where soft, and in many places quite pulpy.

A full-grown and well-formed male fœtus was found in the uterus. There was no dilatation of the os tincæ.

Your obedient servant,

C. Millard.

Dean-Street, Southwark,
July 29, 1834.

REPORTS OF CASES,

By J. Young, M.D.

Of Chester, Delaware County, Pennsylvania.

Effects of Lightning.

On the evening of June 14th, 1832, the house of Mr. C. Levis was stricken with lightning. One of his daughters, aged thirteen, was sitting on a chair in the door-way between two adjoining rooms, down the post or frame of which a portion of the electric fluid passed from an upper room; the child and chair were thrown several feet, and she fell, and remained senseless until I saw her, which was in about an hour after the occurrence. When I arrived, a number of neighbours had collected, and in the confusion of the moment, they were all calling on me to hurry and *bleed her*. I had never seen a case of the kind before, and had not acquired, by reading or otherwise, any fixed ideas concerning the treatment of such cases; and from the bustle attendant on this case, it afforded but slight opportu-

nity for eool reflection and determination; acting, however, on the surgieal maxim, *festina lente*, while they were procuring bandage, &e. for bleeding, a serious doubt arose on the propriety of bleeding, under the eircumstanees attending it. The patient appeared to me to be in a situation entirely similar to many eases of *severe concussion* from blows, falls, &c. that I had witnessed. She was entirely insensible to every external impression, with deep, slow, interrupted respiration; the relaxation of the muscular system was eomplete; the pulse, as to fulness, was nearly natural—it was soft, very easily eompressible, and very slow, being only about 60, as near as eould be ascertained by a seeond wateh; the pupil was sensible, though fully dilated. I believed to bleed under sueh eircumstanees, before reaetion took plaee, would effectually prevent it, and instantly resolved to treat it as a ease of coneussion from any other cause, with the exeeption of *dashing* cold water on the head with my hand, a praetiee I had somewhere seen, or heard recommmended, but have forgotten the authority; and I only resorted to it for the sake of *doing something* to satisfy the bystanders and parents, without expecting mueh good to result. In this, however, I was most satisfactorily disappointed. In less than half a minute, the patient began to toss about and moan, for the first time sinee the aeeident. By a eontinuanee of the dashing for about five minutes, she appeared so mueh aroused, as was evineed by erying, endeavouring to talk, though without being able, tossing, and shrinking from the eold, that we discontinued its use, had her wet elothes taken off, and had her put to bed and covered up warm. After this, she was alternately still, and inelined to doze and sleep for a minute or two at a time; then to roll from side to side, moan, and seemed in great distress; the surfaee was eold, but the pulse harder, more frequent, and quick; the pupils were contraeted, and every thing indicated a ehange, approaching to reaetion, to have taken plaee, I remained with her till 2 o'eloek, A.M. when it was established; she could talk and explain her sensations. She eomplained of very severe pain in the head, and I now apprehended danger from the opposite quarter, to antieipate whieh, I bled her to twenty ounees by estimate, keeping my finger on the pulse, and not stopping till there was a pereeptible change it it. Her head was bathed with eool vinegar, and as her feet were yet eold, they were enveloped in wetted horse-radish leaves. In half an hour she sunk into a eomposed, eomfortable sleep, from whieh she did not rouse till half-past seven o'clock. Finding the pulse again too active, and the patient still eomplaining of some headache, I again bled her twelve ounces; ordered a dose of Epsom salts to be given in three or four hours, with some other direetions eoneerning diet, stillness, &e. and left her, with a request to be sent for if it was thought neeessary. I heard no more from her for a week, when she had almost entirely reeovered from all the effeets of the injury.

Fortunately, aeeidents of this kind are not frequent; but still they are suffieiently so to render fixed prineiples in their treatment desirable. If any thing of the kind exists, I, and perhaps many others, are ignorant of where to find them; in faet, I do not recolleet reading any thing eoneerning it in any medieal work, although elaborate essays may exist. Situated in the eountry, and engaged in the laborious oeeupation of a eountry praetiee, we have not the faeilities for referenee of eity praetitioners. If, however, we are justifiable in drawing eonelusive inferenees from our ease, I believe the eorreet principle to aet on is to wait for reaetion before venturing to bleed, and to promote the taking plaee of this by eold water dashed on the head. *Query?* Might not this eold dashing be servieeable in eases of severe concussion from falls, blows, &c., when the patient lies for hours, and even days, before reaetion eomes on? I have not had an opportunity of trying it, sinee I witnessed its effeet in the above case, but I think it worth a trial.

Poisoning with Laudanum.

Oet. 8, 1827, I was ealled to Miss —— at 5 o'eloek in the morning. Found her eomatose, with deep, slow, stertorous, *puffing* respiration. Could learn nothing about her, only that she went to bed as well as usual the evening previous. It was impossible to arouse her by any means. Muscular relaxation eomplete. What should have occasioned sueh a state of things in a hale young girl, was a mystery at the time. There was evidently a violent determination to the brain, and I bled her; but I had not abstraeted more than six ounees before the pulse gave way so mueh as to render it neeessary to stop the flow. All that eould be done was to use sinapisms and frietions, as she eould not swallow: eold cloths were kept eonstantly on the head. These were faithfully applied; and in twenty minutes, the pulse having filled up, I again opened the orifiee, and drew about ten ounees more blood, when the pulse again failed, and I bound up the arm. I left at six o'eloek, after reeommending a eontinuanee of the sinapisms, frictions, and cold to the head. I saw her

again at eight o'clock. But very slight alteration had taken place, except now she moaned frequently, and thus evinced some more sign of returning animation. The pulse was again full, though soft and yielding; I bled again; the pulse held out till about a pound was extracted, when it again failed. I again left, with an injunction to persevere in the former external applications. I did not see her till five in the afternoon. On my entering the room she addressed me—"Ah, doctor! I never intended to have seen this time." I found her in strong labour, and in less than half an hour I delivered her of a fine boy. The history of the case was, that she had become pregnant, though she had so artfully managed to conceal it, that not a person about the house suspected it; in fact, her character was entirely beyond suspicion. She now informed us that she had taken *an ounce* of laudanum in a tea-cupful of whiskey, with the determination of destroying herself;—but she was thwarted in her designs, married the father of her infant, and makes a valuable wife. In this case, had I known that laudanum had been taken, I should have proceeded differently in the treatment, but certainly not more effectually. The whole ounce of laudanum (I was shewn the ounce vial, and she assured us it was full) remained in the stomach, producing its usual effects on the brain and nerves; and I account for the success attending the treatment, by supposing the repeated bleedings and cold applications to have kept down the determination to the brain, so far as to prevent any serious injury there, while the constant frictions with whiskey and Cayenne, together with the sinapisms, aided in keeping up the circulation in the extremities and surface, and thus enabled nature to triumph over the effects of the liquid poison. By care, and the usual means that would suggest themselves to any one accustomed to treating disease, she was completely relieved from every effect of it in five days.—*Amer. Jour. Med. Sciences.*

POISONING WITH SALMON.

Two persons, a man and a woman, at Maidstone, in Kent, have lost their lives by eating putrid pickled salmon. On examining their bodies professionally, the morbid appearances produced by an animal poison were clearly detected in the stomach and intestines. A coroner's inquest has been held in the case, and a verdict of *Manslaughter* returned. The vender of the delcterious article has accordingly been committed to jail on the coroner's warrant.

NEW MEDICAL WORKS.

Clinical Lectures in the Manchester Royal Infirmary. By Edward Carbutt, M.D. 8vo. 12s. boards.

A Treatise on Primary Geology; being an Examination, both Practical and Theoretical, of the Older Formations. By Henry S. Boase, M.D., Secretary of the Royal Geological Society of Cornwall, &c.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *July* 29, 1834.

Abscess	3	Heart, diseased	1
Age and Debility	40	Hooping-Cough	9
Apoplexy	4	Inflammation	43
Asthma	8	Bowels & Stomach	15
Cancer	2	Brain	4
Childbirth	4	Lungs and Pleura	3
Cholera	25	Insanity	2
Consumption	91	Jaundice	4
Constipation of the Bowels	4	Liver, diseased	7
Convulsions	34	Measles	7
Croup	6	Mortification	7
Dentition or Teething	8	Paralysis	4
Diarrhœa	1	Rheumatism	1
Dropsy	16	Scrofula	1
Dropsy on the Brain	12	Small-Pox	4
Dropsy on the Chest	1	Sore Throat and Quinsey	1
Erysipelas	2	Spasms	2
Fever	10	Stricture	1
Fever, Scarlet	11	Thrush	3
Fever, Typhus	2	Unknown Causes	4
Gout	3		
Hæmorrhage	1	Stillborn	20

Increase of Burials, as compared with the preceding week . . . 11

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

July, 1834.	THERMOMETER.		BAROMETER.	
Thursday . 24	from 53	to 79	29·96	Stat.
Friday . . 25	54	78	29·96	to 29·95
Saturday . 26	55	79	29·86	29·81
Sunday . . 27	50	67	29·75	29·79
Monday . . 28	55	79	29·91	29·93
Tuesday . . 29	52	80	29·95	29·92
Wednesday 30	53	75	29·82	29·81

Wind N.E. and S.W., the former prevailing.

Generally clear till the night of the 26th, when about ·2 of an inch of rain fell. The 27th and 28th generally cloudy; on the evening of the latter day much lightning from the S. and S.W. from 9 till near midnight, when a violent storm of thunder, lightning, and hail, commenced, and continued without intermission till near 1, when the lightning only remained to flash till day-break of the 29th: during the storm ·65 of an inch of rain fell; many small meteors also fell from the zenith in various directions from 1 to 2. The lightning is described by several persons who were exposed to it as being of a very peculiar character, illumining the air much more strongly than usual, and remarkably blue. The 29th and 30th generally cloudy, with much rain on the morning of the latter day.

Rain fallen, 1 inch and ·525 of an inch.

CHARLES HENRY ADAMS.

W. WILSON, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, AUGUST 9, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XLIV.

COMPLEX LABOURS.

HÆMORRHAGE—PLACENTAL PRESENTATION.

Since, when the placenta is situated over the mouth of the womb, attacks of hæmorrhage generally recur at uncertain intervals during the last few weeks of pregnancy, so it is more than probable that our attendance may be desired on two or three occasions previously to the accession of labour; the patient being suddenly seized with a gush of blood, while in bed perhaps, or otherwise quiet. She will very possibly suppose at first that the membranes have broken, and expect labour to follow rapidly; but, on examining her linen, she becomes immediately sensible that the discharge is blood, and in a greater or less alarm, summons us to her assistance. On our arrival, we probably find that the flow has ceased, and that she is more composed, though still perhaps a little faint.

Treatment.—Our first endeavour, under such circumstances, should be to calm the mental agitation; and the next, to prevent a return of the bleeding. Should the discharge not have been very profuse, we may with truth and propriety assure the patient that there is no present danger, and that the prevention of a recurrence will much depend on her own conduct; and we must proceed to lay down, in the strictest manner, rules for her future guidance. As a general principle, we shall find bleeding, and the exhibition of digitalis or other depressing agents, in such a case inadmissible; perfectly so indeed, unless the woman be plethoric,—unless the arterial system be acting with undue energy, or unless fever be present, or indications of local determination to some particular organ.

Nor shall we find it generally necessary to make a vaginal examination, unless indeed the pains of parturition have already shown themselves; for if the term of pregnancy be distant five or six weeks,—the os uteri being closely shut,—by such an inquiry we should gain no information, and we might disturb the coagula formed at the patulous vascular orifices; and thus we should run the risk of causing a renewal of the bleeding. However desirable, then, it might be positively to ascertain whether the placenta were situated over the os uteri or not,—since this knowledge is so difficult to acquire, and since the attempt would most likely produce an increase of the danger,—it is better that we should remain satisfied with suspicion than that we should disturb the temporary safeguard nature has established; especially as no means can be used except of a palliative nature, so long as the os uteri continues perfectly closed. But, on the other hand, if the flooding be still going on,—if the patient have arrived at the end of gestation, or near it, and particularly if the uterus be contracting at intervals, however weak the pains may be,—we should insist on making an examination per vaginam, and act according to the principles immediately to be laid down. We shall now presume, then, that labour has not commenced, that the term of gestation is not fulfilled, and that the discharge has entirely or nearly subsided on our arrival.

Absolute and uninterrupted quietude in the horizontal posture, and on a hard bed, must be forcibly enjoined, and an anti-hæmorrhagic regimen prescribed; she must breathe a cold atmosphere; be but lightly

covered; her diet must principally consist of nutritious fluids—cold and acid drinks may be given almost *ad libitum*, and ices may be allowed, unless they produce pain or shivering: every thing stimulating, both alcoholic or of any other nature, must be strongly forbidden. The mineral acids, under such a case, may be usefully employed; some gentle aperient will be required, and the acidulated infusion of roses, with small doses of sulphate of magnesia, is perhaps the pleasantest, and, at the same time, as efficacious a medicine, as any that can be exhibited. We must avoid a constipated state of bowels; because the straining necessary for the passage of hardened fæces may dislodge the coagula collected over the exposed vessels, and produce a return of the flooding. We must equally, also, avoid violent purging, lest the frequent evacuation of the rectum should occasion a like disaster. A cold enema may be administered daily, which will probably act beneficially in two ways, —both by clearing the rectum, and restraining the hæmorrhagic tendency. Opium may be exhibited if there be present much nervous irritability, an excited state of mind, or spasmodic and false pains; but as I have little faith in the power of opium to suppress hæmorrhage, I should not administer it with that specific intention; and I think I have obtained as much advantage from the pharmacopœial preparations of henbane or hemlock, in quieting an excited state of the nervous system under these circumstances, as from opium itself. Before our departure, we must direct, that on the occasion of another attack, cloths steeped in vinegar and water should be applied to the vulva and lower part of the abdomen, and that we should instantly be made acquainted with the occurrence. It is very possible that we may be called three or four times to the same patient under the same circumstances, and on each occasion may think right to repeat the same cautions and general directions.

But the time will arrive when the features of the case will be changed; when uterine action will supervene, and when the mouth of the womb will begin to be dilated, and the hæmorrhage consequently increased. It then becomes our duty carefully to consider when delivery shall be effected, under the conviction that it will be ultimately necessary, and that our patient will remain in imminent danger until it is accomplished.

The only question, then, left for consideration is, when shall delivery be attempted. It is evident that until a certain degree of dilatation is effected, the hand cannot be introduced to accomplish the proposed end; but it is equally evident that if we wait until the os uteri is widely open, the probability is, that the patient will be so exhausted as to leave little chance of her survival, even under the most skilful management. The two extremes, therefore, of forcing the hand through the mouth of the womb, while it will not admit of artificial dilatation without sustaining injury, and of delaying our means until the system is depressed beyond the hope of recovery, are both equally to be deprecated in practice; and it becomes a very nice point to determine the exact time when our assistance will be most serviceable.

We may lay it down as a maxim, that as soon as the os uteri is dilated to the diameter of half a crown, the hand may be introduced without injury, provided the term of gestation be fully, or nearly completed; and that it would be unwise to wait for its further development, because we may expect that with the increase of every line's diameter, there will be an increase of the bleeding, and that death may take place, as happened to the poor creature whose uterus is before you. When this specified degree of dilatation, then, is acquired, we are warranted in undertaking delivery, which is to be accomplished in the following manner. The patient, lying on her left side, is to be brought conveniently near to the edge of the bed, and gently restrained by the means I have before mentioned when speaking of transverse presentations. The operator having taken off his coat, and kneeling by the bed-side,—the left hand and arm being greased,—the fingers are to be collected into the form of a cone, slowly insinuated through the external parts, carried up to the brim of the pelvis, in the direction of the axis of the vagina, and with a slow semi-rotatory motion the os uteri must be carefully dilated, and the hand passed fully into the uterine cavity,—by the side of the placenta,—partially separating that organ from its attachment to the uterine neck. It must be introduced either anteriorly or posteriorly, in that direction where the placenta appears to be thinnest, because the edge of the organ will then most probably be sooner reached, and the uterine vessels will there be found smallest. It is not, however, always easy to distinguish the point at which the placental edge may be arrived at most readily: in this part of the operation, then, we must trust somewhat to accident. Having gained the membranes, they must be punctured, the hand carried into the centre of the ovum, run along the body of the child until the feet be felt; one or both of which limbs must be brought down through the rent in the fœtal membranes. The child's body is thus made to revolve on its own

axis, provided the head presents or it lies transversely, and the breech is brought into the pelvis. To facilitate the expulsion, and to ensure ultimately as perfect a contraction of the uterus as possible, gentle friction may be applied over the uterine tumor, through the parietes of the abdomen; and unless the hæmorrhage be continuing profusely, rapid extraction of the child's body should be most cautiously avoided.

When about to perform the operation just described, you must ever remember that your patient's life will depend principally on your knowledge, presence of mind, and perseverance; and when once you have undertaken the dilatation of the os uteri, you must unflinchingly proceed to the termination of the delivery, unless some extraordinary difficulty should present itself. The hand, then, must be passed slowly and carefully onwards; for to withdraw it would be to risk a renewal or perilous increase of the bleeding.

You would, indeed, suppose that the extensive separation of the placenta from its uterine attachment, in the introduction of the hand, as just described, must in all cases produce a frightful augmentation of the hæmorrhage, and that there would be little chance of the patient's survival. This, however, fortunately is not usually the case. On the first introduction of the fingers through the os uteri, there is certainly almost always a gush of blood, and that to a copious extent; but when the hand has fully entered the orifice, by the pressure it exerts on the open vessels it acts as a plug, and prevents any great additional loss; as it is carried further, the arm performs the same office; and when the breech of the child is brought into the vagina, or to rest upon the pelvic brim, the body itself causes a like compression: so that if the delivery be skilfully managed, the increase of bleeding which takes place under it is comparatively trifling.

Dreading the fresh accession of discharge which it was thought must necessarily attend the uncovering so many vascular orifices by the hand being slid between the neck of the womb and the placenta apposed over it, some practitioners have recommended that we should perforate the substance of the placenta itself, by working our fingers successively through it. According to Richter, it was Delcurye and Mohrenheim who first insisted on the superiority of this mode of acting. To me, indeed, it offers many objections; and the principal consists in the difficulty of its performance. It is by no means an easy matter to perforate the structure of the placenta by the fingers, when the organ is taken out of the body and lying on a table: how much more difficult must it be, then, to run the fingers through it, when it is attached over the os uteri, when there is no resistance behind, to assist us in our attempt! How much more likely is it that the mass may be lifted entirely away from its connexion with the neck of the womb, and carried up before the hand! And if this should occur, it is reasonable to suppose that the discharge would be much more profuse than if a smaller portion were separated, and the hand slid along between it and the open vessels. It certainly happens occasionally that the placenta is so soft as scarcely to bear being lifted by its edge without falling to pieces; but this is unusual, and our practice must be regulated not by the exceptions, but the general principle.

Besides, so long as we preserve the placenta entire, we prevent any loss of blood from the fœtal system; but when that organ is torn, the vessels must necessarily be ruptured, and the child, if it be alive, will most probably bleed to death. It is almost incredible how small a loss of blood is sufficient to destroy a newly-born infant. A few drachms oozing from the funis, if the ligature be loosely tied, has been known to cause a fatal result. By perforating the placenta, then, we run a great risk of destroying the fœtus. It may be argued, that the life of the child is not to be brought into competition with the safety of the mother; nor indeed should it; but as I believe there is equal, if not more danger, incurred to the mother by such a mode of action, I would endeavour by all means to preserve the child, provided that were possible. Again, it may be said that in many cases of placental presentation,—if not the majority,—the child is born still; and that therefore the chance of its ultimate survival is but small. This I am willing to grant also; but if the fœtal vessels be preserved entire, the death is caused, not by any loss of blood from the fœtal system itself, but in consequence of its being deprived of the benefits which result from the uterine circulation. During the faint under which the mother lies, the blood is neither sent in the same quantity, nor with the same velocity, to the uterine organ, consequently the changes necessary for the continuance of fœtal life cannot go on in the placenta; the child ceases to exist in consequence of the want of those changes; it dies from asphyxia,—as perfectly suffocated as if it were drowned after the commencement of breathing life. I perfectly coincide with Dewees in deprecating the practice of perforating the placenta in these cases; unless, indeed, its structure be unusually soft, and easily lacerable.

But the favourable time for the performance of the operation which I have

just noticed may have slipped by before we have an opportunity of seeing the patient; and we may perhaps find her faint, and gasping, and cold; the uterus quite inactive, with its mouth widely open, and possessing that degree of unresisting flabbiness which, to an experienced hand, is indicative of the most urgent danger. Under this state delivery would be indeed easy, but it would at the same time be followed by almost certain death: for if we empty the uterus under syncope, or deep and long-continued faintness, we cannot reasonably suppose it will take upon itself that degree of active contraction necessary to close the vessels, and place the woman in a state of safety. It would, then, be most injudicious to proceed at once to the operation. Our indication would rather be, to rouse the patient from the torpor under which she is suffering—to bring her system up to a certain point before we attempt to evacuate the womb. Stimulants here, then, are absolutely called for. Brandy, æther, ammonia, and other cordials, may be exhibited; and transfusion of blood may even be performed, with the view of inducing the temporary excitement so necessary to be procured before delivery be attempted. To the employment of opium under such circumstances I have objections, on the grounds stated at our last meeting, although sanctioned by high authority. I cannot agree with Professor Burns, that this drug will check the flow of blood; and I think we possess other cordials and stimulants quite as efficacious in rousing for a time the depressed vital powers, by the exhibition of which we do not incur the danger of eventually paralyzing the uterine energies. In most cases we shall find the ergot a serviceable remedy after the stimuli have taken effect, and before the operation is proceeded in. A dose or two of this medicine, indeed, may be given in every instance of placental presentation, previously to the delivery being undertaken. Let us, then, bear in mind, that although it is a maxim in obstetric practice never to allow a woman to die undelivered, if delivery can by any means be accomplished, still it should also be another maxim, never to empty the uterus during the continuance of an attack of syncope; for it is not the mere extraction of the child to which our mind should be directed, but leaving the patient in the most favourable condition, with respect to ultimate recovery, which the nature of the case will admit of.

Again, it is by no means impossible that such alarming symptoms may shew themselves before the os uteri has acquired the diameter of half a crown, as to render it extremely hazardous for us to delay our means until that degree of dilatation is arrived at. The blood may be pouring out in a copious and continued stream, or may be oozing in a less violent though steady draining; or coagula of considerable size may be passing from the vagina every few minutes: and it must be evident to the least attentive observer that such a state of things cannot be allowed to proceed unchecked. Two modes offer themselves for our choice: either immediate delivery, or endeavouring to restrain the flow, and delaying until the due degree of dilatation is effected. Our practice will mainly be guided by the state of the os uteri itself: if it appear soft, lax, and distensible, offering but little resistance to our finger in the attempt at dilatation, we shall mostly be able, under the use of sufficient caution, to pass the hand entirely through it without injury, even although its disc be not exceeding the diameter of a shilling; and, indeed, I have accomplished the operation of turning on more than one occasion, under these unpromising circumstances, by slowly insinuating the fingers *seriatim*. Although, then, such a proceeding be not desirable, if it can be avoided, —inasmuch as every minute's delay brings with it an augmentation of danger,—we are fully justified in effecting the dilatation of the os uteri thus artificially, even when, at the commencement of our efforts, it will scarcely admit the introduction of the tips of two fingers. For, as a principle, we shall find that delivery had better be had recourse to an hour too soon than an hour too late; and that the frequent fatality of these dreadful cases is to be attributed, in a great measure, to the operation having been delayed until the system was irrevocably depressed. The *dilatability* of the organ, then, is to be regarded as an indication of its capability of being fully opened,—as much as, or even more than, its existing state of actual *dilatation*.

But the hæmorrhage may be profuse, and may threaten immediate dissolution, while the os uteri is dilated to no greater extent than the size of a sixpence possibly, and while it remains in a rigid, unyielding condition; and this is particularly observable when labour has commenced previously to the full term of gestation being completed. It is seldom, however, that flooding proceeds to the extent of endangering life, without also causing a relaxed state of the uterine mouth. But occasionally the complication of dangers just adverted to may exist together. Any forcible attempt at opening it artificially would, under such a state, be certainly productive of injury, probably of a very serious character. As delivery, then, could not be accomplished, except under extreme hazard, no choice is left us but to endeavour to suspend the flow, and

to wait until the mouth of the womb has taken upon itself a more favourable condition. The common principles must here be most assiduously followed; perfect quiet, the application of cold, the removal of every cause of excitement, and the exhibition of cold and acidulated drinks. Local means may also in some degree avail us; the vagina may with advantage be plugged with a silk or cambric handkerchief, or lint steeped in oil, vinegar, or a weak solution of alum; a practice first recommended by Leroux;—inadmissible, however, in any other case of bleeding from the uterus, after four or five months of gestation are completed. Some practitioners, indeed, of great eminence (among whom I may mention the much-respected name of Merriman), object to the employment of this means in any case of labour near the close of pregnancy, fearing an internal accumulation of blood, favoured by the distensibility of the uterine parietes; for, as I have before shown you, the uterus at full time is never perfectly filled by the ovum, but capable of containing a considerable quantity of more matter. Thus, then, although the fluid be prevented draining through the vagina, much may be collected within the cavity of the womb, and a fatal termination may result. This reasoning is undoubtedly true, to its fullest extent, in accidental hæmorrhage before delivery, under retention of the placenta, and in floodings after the expulsion of that organ; nay, as the blood concretes in the uterine cavity, the viscus is more and more distended; its vessels become gradually more and more dilated; their orifices gape wider and wider, and consequently they are rendered capable of pouring out a larger quantity of blood in a given space of time. Thus, then, the means employed is adding, in a geometrically increasing ratio, to the peril of the case. Nor must another cause of additional hazard be overlooked: the external flow of blood being prevented, the source of danger is concealed, and it is possible for fatal deception to arise. But when the placenta is implanted over the os uteri, it is in my opinion unlikely that the bleeding will take place into the womb itself; and if the vagina be perfectly filled with the tampon, there is no other cavity in which the vital fluid can collect; so that I think we may occasionally have recourse to it with advantage. Blood is certainly less likely to accumulate at the cervix uteri than towards the upper part of the organ; and such a collection near the orifice as to endanger life can only, in my opinion, occur under a state of great laxity of uterine fibre, and extreme depression of the vital energies.

Another great objection to the use of the tampon in these cases, consists in the necessity of removing it whenever a vaginal examination is required to be instituted, for the purpose of watching the dilating process going on in the os uteri: but the frequency of return, and the strength of the uterine contractions, will in some measure indicate to us the changes taking place in that organ; and we must be guided by them in regard to the removal of the plug,—provided it staunches the flow outwardly, and provided also there are no evidences of internal hæmorrhage. Although, then, I consider the tampon may be occasionally useful, I am far from recommending it in preference to other means; and I think it should only be had recourse to in the rigid, unyielding condition of the os uteri just adverted to, when the discharge is alarming, and can be restrained in no other way, and when delivery is impossible.

A very few weeks since I had an opportunity of observing the valuable effects of plugging the vagina in the case under consideration. I was sent for by a professional friend, residing a few miles from London, to a patient who had suffered some violent attacks of hæmorrhage from placental presentation. When uterine action was established, the discharge, as might be expected, was increased. My friend was summoned, and found the os uteri but little dilated, and rigid, the bleeding continuing copiously. Not choosing to take the management of the case entirely on his own responsibility, he plugged the vagina with a sponge, and sent to town for my assistance. Between two and three hours elapsed before I arrived: the discharge was still going on, but in a very moderate degree. The patient had rallied considerably, and the uterus was acting occasionally. On withdrawing the sponge, I found the os uteri dilated nearly to the size of a crown. Through a very small portion of the organ anteriorly, I could distinguish the membranes protruding; I punctured them, let off the liquor amnii, administered two doses of ergot, and in about a quarter of an hour proceeded to deliver, which I effected by turning without much difficulty, and with a very slight additional discharge. The uterus acted strongly, and the patient recovered. I have no doubt that the tampon restrained the flow of blood in this instance considerably.

I have given you to understand that placental presentations are always fraught with extreme peril; I look upon them, indeed, as the most dangerous of all cases of hæmorrhage; and many causes contribute their share towards the production of this danger. The frequent losses of blood which occur previously to the accession of labour, tend, in no small degree, to de-

press the constitution, and render it unable to sustain the aggravated shock on the opening of the os uteri. The violent gushes which generally accompany the dilatation of the womb, and the natural and praiseworthy reluctance which most practitioners feel to a *forced* delivery, when the os uteri is in an undilated state (and this very feeling may induce delay beyond the period of safety—of which I have myself known instances), all combine to render this, indeed, a fearful case. But it appears to me that still another must be added: I mean, the sudden emptying of the uterus of the *whole* of its contents comparatively rapidly, and that too at a time when the constitution, weakened by hæmorrhage, is easily affected by any depressing action. We all know the effect of suddenly evacuating the water in ascites; we know that the most courageous and hardiest persons will sometimes fall into a state of syncope, even under the comparatively trifling operation of tapping; and we account for the faintness by the rapid removal of that pressure from the large vessels of the trunk—and perhaps from the viscera themselves—to which they had been for so long accustomed. It appears to me that the same obtains in delivery under placental presentation, as generally practised. The gravid uterus occupies a very large space in the abdominal cavity; during its gradual increase, it has been exerting a constantly augmenting pressure on all the parts surrounding it; these parts accommodating themselves to the inconvenience they must necessarily suffer during pregnancy, by the slowness of the organ's development. But when its contents are suddenly removed—when the liquor amnii is allowed to escape, and with it the fœtus is extracted also—an enormous decrease in its bulk is effected, considerable pressure is at once taken off, —and that, too, at a time when the system is suffering much from previous depressing causes—and we cannot wonder that collapse occurs as a consequence; even should the delivery have been perfected with but slight additional loss of blood. It has occurred, therefore, to my father (and in his sentiments I fully join), that in some cases it might be desirable partially to evacuate the uterus, and wait for a short period, before completing the delivery, provided this could be done without indueing a further separation of the placenta; and the discharge of the liquor amnii seems to offer us a means of accomplishing this end. For this purpose, however, the placenta must be perforated by some sharp instrument—a common trochar, for instance; and a probability therefore exists, that fœtal blood may be lost by the laceration of one or more placental vessels. Such a chance of injury we cannot guard against; and as in the most desperate cases the fœtus is generally born dead, perhaps it ought scarcely to be taken into our account. I have never myself resorted to this expedient, but it is suggested on the principle of emptying the uterus as slowly as possible, by which both the pressure is removed more gradually, and also a better opportunity is afforded the viscus of taking on itself expulsive action.

It is more than probable that spirituous stimuli may be required during the process of extraction, as well as after the completion of the birth. Since, however, their exhibition is such a nice point, it must be regulated by the extremest caution.

PARTIAL PLACENTAL PRESENTATIONS.

As there is no part of the internal surface of the uterus to which the placenta may not be occasionally attached, so we find it sometimes *partially* placed over the orifice at the beginning of labour; one-third, half, or two-thirds of the disc of the os uteri being covered by the placenta, and the remainder occupied by the membranes; and partial placental presentations are more frequently met with than cases in which the mass is centrally implanted over it.

Under this state of things there will exist the same liability to hæmorrhage during the development of the cervix uteri, as in the case just described;—the same symptoms, therefore, during the last months of pregnancy—the same sudden and occasional floodings, not to be accounted for by any apparent external cause—the same spontaneous cessation. On the accession of labour-pains also, the symptoms will be equally similar: we shall observe the same increase of discharge on the return of each pain, and the same diminution or subsidence in the interval of action; but the probability is that the hæmorrhage will not be so profuse, because we may calculate on the vessels that are opened being both fewer in number and smaller in calibre.

Treatment.—Although so similar in character and symptoms to the case last spoken of, partial placental presentations are by no means so hazardous; and they admit of a somewhat modified treatment. They can only be discriminated by a careful vaginal examination. On introducing the finger for this purpose, the edge of the placenta may be clearly felt, and the membranes passing off from it; a portion of the organ, thin and moveable, can also be distinguished, closing a part of the uterine mouth; while the remainder of the orifice is occupied by the membranes, through which the presenting part of the child may, perhaps, be perceptibly discerned.

It is very possible, if the os uteri be much dilated, that a considerable portion of the placenta may be propelled downwards into the vagina, apparently hanging loose in that cavity, but still connected within to the cervix uteri above. The danger will generally be proportioned to the quantity of the mass implanted over the uterine mouth; and the profuseness of the discharge will be regulated by the degree of separation.

Previously to the dilatation of the os uteri, our general management must be precisely similar to that already advised; but when labour is established, it must entirely depend on the state of the patient herself, and the urgency of the symptoms. Should the sanguineous appearance be but trifling,—which, however, is not often the case,—the labour may perhaps be allowed to proceed uninterfered with; but should a continued discharge be going on, it will be most prudent to rupture the membranes, and allow the liquor amnii to drain away; and this may with advantage be done, whatever degree of dilatation the os uteri may have acquired, and whatever degree of depression the patient's system may have suffered, provided the head present. Nor will it be found difficult to effect—either the finger-nail, the stillette of a catheter, or a pointed quill, being quite sufficient for the purpose. If the os uteri be covered but in a trifling degree by the placental mass, the best opportunity is afforded of compressing the vessels previously opened, by the descent of the head upon the pelvic brim, and of increasing the expulsive efforts of the uterus by the augmented stimulus propagated to its mouth. And if the placental attachment be more considerable, and the flooding consequently more copious, so that artificial delivery subsequently becomes necessary, the uterus is relieved of a part of its contents before the operation is commenced, and no small degree of the danger necessarily attendant upon the case thereby averted. Another desirable effect is produced by the diminution in the capacity of the uterine vessels in consequence of the partial contraction of the parietes; and a third by the probability of a further separation of the placenta to any great extent being much lessened; for so long as the membranes are entire, it stands to reason that the placenta is likely to be detached in the same proportion as they are protruded downwards into the vagina; but when the bag is destroyed, and the head presses with some power against the mouth of the womb, not only is the chance of an increased separation diminished, but a plug is formed by the compression which the head occasions.

Nevertheless, it must not be supposed that a natural termination, though so highly desirable, will invariably follow the proceeding I have recommended: the after conduct of the case, then, must depend on the continuance of the hæmorrhage, and the effect produced on the constitution. If the flooding be at once stayed, and the patient not much depressed, our indication would evidently be to allow nature an opportunity of perfecting the delivery unaided. Even if a slight oozing continued,—provided the uterus acted with vigour, the labour progressed, and the powers of life remained tolerably good,—it would be injudicious to interfere, because so much less danger attends a natural than an artificial birth. But, should the constitution become gradually weakened, should the pulse flag, and faintness occur, we must resort to manual delivery; and we shall find the operation of turning usually the most applicable to the case.

From the acknowledged probability that delivery may in the end be requisite, an objection has been strongly urged against this plan, under the impression, that, after the escape of the liquor amnii, the uterus may so strongly compress the fœtal body as to prevent the introduction of the hand for the accomplishment of the operation of turning. I have already laid it down as a principle, that under transverse presentations the passage of the hand and version of the fœtus is comparatively easy while the membranes are preserved whole, but that it becomes an operation of the utmost difficulty when the fœtal body is closely embraced by the uterine parietes. It has been supposed that the same difficulty would be met with under the circumstances I have just described. Such reasoning, however, is founded on false data, and is in itself, therefore, untenable; for should the uterus act sufficiently strongly to oppose a serious obstacle to the introduction of the hand, its contraction will be vigorous enough to propel the head so forcibly against the os uteri as to check the discharge by its own pressure, and eventually to expel the child,—provided, indeed, the pelvis be of ordinary capacity, and the soft parts acquire their usual distensibility. When the fœtus lies transversely, it cannot pass by the agency of nature alone, because of its unfavourable position; but if the vertex offer itself under a partial presentation of the placenta, such an impediment cannot exist.

After having punctured the membranes, then, the patient still requires careful and constant waching; and we must be prepared to act with promptitude, should circumstances require our further interference *.

* In Dr. F. H. Rambotham's last lecture, page 632, last column, line 12, after *warm bath*, the

ON THE

PRESENT STATE OF OUR KNOWLEDGE RESPECTING THE PHYSIOLOGY OF THE NERVOUS SYSTEM;

Being a Report presented to the British Association, assembled at Cambridge, June 1833.

By William Charles Henry, M.D.,
Physician to the Manchester Royal Infirmary *.

In the higher manifestations of life, nervous matter is invested with the most eminently vital attributes. It is the exclusive seat of the various modes of sensation, and of all the intellectual operations; or, rather, it is the point of transition, where the physical conditions of the organs, which are induced by external objects, pass into states of mind, becoming perceptions; and where the mental act of volition first impresses a change on living matter. These two offices of conducting motive impressions from the central seat of the will to the muscles, and of propagating sensations from the surface of the body and the external organs of sense to the sensorium commune, have been of late years shown to reside in distinct portions of nervous substance.

The honour of this discovery, doubtless the most important accession to physiological knowledge since the time of Harvey, belongs exclusively to Sir Charles Bell. It constitutes, moreover, only a part of the new truths, which his researches have unveiled, regarding the general laws of nervous action, and the offices of individual nerves. His successive experiments on function, guided always by strong anatomical analogies in structure, in origin, or in distribution, have led to the entire remodelling of nervous physiology, and to the formation of a system of arrangement based on essential affinities and on parity of intimate composition, instead of on *apparent* sequence or proximity of origin. Among the continental anatomists, MM. Magendie and Flourens have contributed most largely to our knowledge of this part of physiology; the former by repeating and confirming the experiments of Bell, as well as by various original inquiries; the latter by his important researches into the vital offices of the brain and its appendages. Much light, too, has been thrown on the functions of several of the encephalic nerves, and especially of those supplying the face and its connected cavities, by Mr. Herbert Mayo, who has analysed their anatomical composition, and pursued their course with singular precision; and has thus been enabled to correct some errors of detail in the system of Sir Charles Bell.

Nervous System.—In man, and in other vertebrated animals, the nervous system consists of the cerebrum, cerebellum, medulla oblongata, medulla spinalis, and of the encephalic, spinal, and ganglionic nerves. It seems most natural to observe this order of anatomical sequence in recording what is known of nervous functions.

Cerebrum, or *Brain-proper.*—The physiology of the brain has received of late years very considerable accessions, and its vital offices, *viewed as an entire organ*, have now probably been ascertained with sufficient precision. Some portion of this newly-acquired knowledge has been gathered from experiments on living animals, but the greater and more valuable part has flowed from the study of comparative development. In this latter field of inquiry, Tiedemann's elaborate history of the progressive evolution of the human brain during the period of fœtal existence, with reference to the comparative structure of that organ in the lower animals, merits an early and detailed notice. It had been discovered by Harvey, that the fœtus in the human species, as well as in inferior animals, is not a precise facsimile of the adult, but that it commences from a form infinitely more simple, and passes through several successive stages of organization before reaching its perfect development. In the circulatory system, these changes have been minutely observed, and faithfully recorded. Tiedemann has traced a similar progression in the brain and nervous system, and has moreover established an exact parallel between the *temporary* states of the fœtal brain, in the periods of advancing gestation, and the *permanent* development of that organ at successive points of the animal scale. The first part of his work is simply descriptive of the nervous system of the embryo at each successive month of fœtal life. It constitutes the anatomical ground-work upon which are raised the general laws of cerebral formation, and the higher philosophy of the science. In the second part, Tiedemann has established, by examples drawn from all the grand divisions of the animal kingdom, the universality of the law of formation, as traced in the nervous system of the

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following should have been added:—Chapman relates a case in which the placenta was expelled four hours before the child's birth: Perfect and Boudelocque each gives us a somewhat similar case; and Merriman cursorily mentions an instance that came within his own knowledge, in which the placenta was expelled "many hours" before the child was born.

* We have taken the liberty of omitting the introductory remarks, and some other passages of this excellent memoir; more especially those which seemed principally intended for the general reader.—Ed. Gaz.

human fœtus, and the existence of one and the same fundamental type in the brain of man and of the inferior animals.

The facts which have been unfolded by the industry of Tiedemann, besides leading to the universal law of nervous development, throw important light upon nervous function; for it is observed that the successive increments of nervous matter, and especially of brain, mark successive advances in the scale of being; and, in general, that the development of the higher instincts and faculties keeps pace with that of brain. Thus, in the zoophyta, and in all living beings destitute of nerves, nothing that resembles an instinct or voluntary act is discoverable. In fishes the hemispheres of the brain are small, and marked with few furrows or eminences. In birds they are much more voluminous, more raised and vaulted than in reptiles; yet no convolutions or anfractuosities can be perceived on any point of their surface, nor are they divided into lobes. The brain of the mammalia approaches by successive steps to that of man. That of the rodentia is at the lowest point of organization. Thus the hemispheres in the mouse, rat, and squirrel, are smooth, and without convolutions. In the carnivorous and ruminating tribes, the hemispheres are much larger, and marked by numerous convolutions. In the ape tribe the brain is still more capacious and more convex; it covers the cerebellum, and is divided into anterior, middle, and posterior lobes. It is in man that the brain attains its greatest magnitude and most elaborate organization. Sömmerring has proved that the volume of the brain, referred to that of the spinal marrow as a standard of comparison, is greater in man than in any other animal.

The most decisive researches that have been hitherto instituted on the functions of the brain, are those of M. Flourens. His mode of operating was to remove cautiously successive thin slices of cerebral matter, and to note the corresponding changes of function. He commenced with the hemispheres of the brain, which he found might be thus cut away, including the corpora striata and thalami optici, without apparently occasioning any pain to the animal, and without exciting convulsive motions. Entire removal of the cerebrum induces a state resembling coma; the animal appears plunged in a profound sleep, being wholly lost to external impressions, and incapable of originating motion; it is deprived, too, according to Flourens, of every mode of sensation. Hence the cerebrum is inferred to be the organ in which reside the faculties of perception, volition, and memory. Though not itself sensible, in the ordinary acceptation of the word,—that is, capable, on contact or injury, of propagating sensation,—yet it is the point where impressions made on the external organs of sense become objects of perception. This absence of general sensibility observed in the brain has also been experimentally demonstrated in the nerves dedicated to the functions of sight, of smell, and of hearing, and constitutes, perhaps, one of the most remarkable phenomena that have been disclosed by interrogating living nature. Flourens appears, however, to have failed in proving that *all* the sensations demand for their perception the integrity of the brain. He has himself stated that an animal deprived of that organ, when violently struck, "has the air of awakening from sleep," and that, if pushed forwards, it continues to advance after the impelling force must have been wholly expended. Cuvier has therefore concluded, in his Report to the Academy of Sciences upon M. Flourens' paper, that the cerebral lobes are the receptacle in which the impressions made on the organs of sight and hearing only, become perceptible by the animal, and that probably there too all the sensations assume a distinct form, and leave durable impressions; that the lobes are, in short, the abode of memory. The lobes, too, would seem to be the part in which those motions which flow from spontaneous acts of the mind have their origin. But a power of effecting regular and combined movements, on *external stimulation*, evidently survives the destruction of the cerebral hemispheres.

A very elaborate series of experiments on the functions of the brain in general, and especially on those of its anterior portion, have been since performed by M. Bouillaud. That observer concurs with Flourens in viewing the cerebral lobes as the seat of the *remembrance* of those sensations which are furnished to us by sight and hearing, as well as of all the intellectual operations to which these sensations may be subjected,—such as comparison, judgment, and reasoning. But he proves that the ordinary tactual sensibility does not require for its manifestation the presence of the brain; for animals entirely deprived of brain were awakened by being struck, and gave evident indications of suffering when exposed to any cause of physical pain. Bouillaud observes, too, that the iris continues obedient to the stimulus of light, after ablation of the hemispheres; and on this ground calls in question the loss of vision asserted by Flourens. Nor are the lobes, he contends, the only receptacle of intelligence, of instincts, and of volition; for to admit this proposition of Flourens would be to grant that an animal which retains the power of locomotion, which makes every effort to escape from irrita-

tion, which preserves its appropriate attitude, and executes the same movements after as before mutilation, may perform all those actions without the agency of the will or of instinct. Another doctrine of Flourens, which has been experimentally refuted by Bouillaud, is, "that the cerebral lobes concur *as a whole* in the full and entire exercise of their functions; that when one sense is lost, all are lost; when one faculty disappears, all disappear;" in short, that a certain amount of cerebral matter may be cut away without apparent injury; but that, when this limit is passed, all voluntary acts and all perceptions perish simultaneously. Bouillaud, on the contrary, has described several experiments which show that animals, from whom the anterior or frontal part of the brain had been removed, preserved sight and hearing, though deprived of the knowledge of external objects, and of the power of seeking their food.

The second part of M. Bouillaud's researches is entirely devoted to the functions of the anterior lobes of the brain. These were either removed by the scalpel, or destroyed by the actual cautery, in dogs, rabbits, and pigeons. Animals thus mutilated feel, see, hear, and smell, are easily alarmed, and execute a number of voluntary acts, but cease to recognize the persons or objects which surround them. They no longer seek food, or perform any action announcing a combination of ideas. Thus the most docile and intelligent dogs lost all power of comprehending signs or words which were before familiar to them, became indifferent to menaces or caresses, were no longer amenable to authority, and retained no remembrance of places, of things, or of persons. They saw distinctly food presented to them, but had ceased to associate with its external qualities all perception of its relations to themselves as an object of desire. The anterior or frontal part of the brain is here inferred to be the seat of several intellectual faculties. Its removal occasions a state resembling idiotism, characterized by loss of the power of discriminating external objects, which, however, co-exists with the faculties of sensation.

Magendie has described some curious experiments on the corpora striata, which, though closely analogous in their results to those on the cerebellum, have their proper place in this section. Removal of one corpus striatum was followed by no remarkable change; but when both had been cut away, the animal rushed violently forwards, never deviating from a rectilinear course, and striking against any objects in its way. In his lecture of February 7, 1828, Magendie, in the presence of his class, removed both corpora striata from a rabbit. The animal attempted to rush forwards, and, if restrained, appeared restless, continuing in the attitude of incipient progression. One thalamus opticus was then cut away from the same animal. The direction of its motion was immediately changed from a straight to a curved line. It continued for some time to run round in circles, turning towards the injured side. When the other thalamus was removed, the animal ceased its motions, and remained perfectly tranquil, with the head inclined backwards. These experiments, it may be observed, furnish no support to the opinions of MM. Fovelle and Pinel Grandchamps, who have assigned the anterior lobes and corpora striata as the parts presiding over the movements of the inferior extremities, and the posterior lobes and thalami as regulating the superior.

Cerebellum.—It may be regarded as nearly established by modern researches, that the cerebellum is more or less directly connected with the function of locomotion. The precise nature and extent of its control over the actions of the voluntary muscles are, however, far from being clearly determined. In the higher animals, the mental act of volition probably has its commencing point, as productive of a physical change in the brain-proper; though it must be confessed that some of the experiments of Flourens, and all of those of Bouillaud, indicate the persistence of many instinctive, and even of some automatic motions, after destruction of the brain. But there *does* appear sufficient evidence to prove that those volitions which have motion as their effect, whatever be their origin, whether in the cerebrum, cerebellum, or medulla oblongata, require for their accomplishment the co-operation of the cerebellum. This evidence has been mainly supplied by the same inquirers, whose researches on the cerebrum have been already analysed.

In the order of time, though not of importance, the experiments of Professor Rolando stand foremost. Injuries of the cerebellum, he observed, were always followed by diminished motive power; and this partial loss of power was always in direct proportion to the amount of injury. A turtle survived upwards of two months the entire removal of the cerebellum, continuing sensible to the slightest stimulus; but when irritants were applied, it was totally unable to move from its place. M. Flourens has since arrived at similar, but more definitive results. He removed in succession thin slices from the cerebellum. After the first two layers had been cut away, a slight weakness, and want of harmony and system, in the automatic movements,

were noticed. When more cerebellic substance had been removed, great general agitation became apparent. The pigeon which was the subject of operation retained, as at first, the senses of sight and hearing, but was capable of executing only irregular unconnected muscular efforts. It lost, by degrees, the power of flying, of walking, and even of standing. Removal of the whole cerebellum was followed by the entire disappearance of motive power. The animal, if laid upon its back, tried in vain to turn round; it perceived and was apprehensive of blows with which it was menaced, heard sounds, seemed aware of danger, and made attempts to escape, though ineffectually,—in short, while it preserved, uninjured, sensation and the exercise of volition, it had lost all power of rendering its muscles obedient to the will. The cerebellum is hence supposed by Flourens to be invested with the office of "balancing, regulating, or combining several sets of muscles and limbs, so as to bring about those complex movements depending on simultaneous and conspiring efforts of many muscles, which are necessary to the different kinds of progressive motion." Bouillaud, who has successfully disputed several of the opinions of Flourens respecting the functions of the cerebrum, fully coincurs with him as to those of the cerebellum.

Yet it must be admitted that there exists also conflicting experimental testimony on this subject. M. Fodera states that he has found the removal of a part of the cerebellum to be followed, in all cases, either by motion *backwards*, or by that position of the body which precedes retrograde movement. The head is thrown back, the hind legs separated, and the fore legs extended forwards, and pressed firmly against the ground. More complete destruction of the cerebellum occasions the animal to fall on its side; but the head is still inclined rigidly backwards, and the anterior extremities agitated with convulsive movements, tending to cause retrograde motion of the body. Injuries of one side of the cerebellum were observed to produce paralysis of the same side of the body; as might, indeed, have been anticipated from the direct course, without decussation, of the restiform columns which ascend to form the cerebellum. Magendie has described precisely the same results. A duck, whose cerebellum had been destroyed, could swim only backwards. In the course of his experimental lectures, Magendie, having removed the cerebellum in several rabbits, demonstrated to his class the phenomena of retrograde movement, exactly as they have been recorded by Fodera. It is, then, impossible to regard the conclusions of Flourens as fully established, opposed as they are by those of so skilful an experimenter as Magendie. Indeed, while Flourens conceives the cerebellum to preside over motion, MM. Foville and Pinel Grandchamps attribute to it the directly opposite function of sensation: and this doctrine seems to derive some support from anatomical disposition; for it has been proved by Tiedemann, that the cerebellum is nothing more than an expansion or prolongation of the corpora restiformia, and posterior columns of the spinal medulla, which columns have been shown by Sir Charles Bell to have the office of conveying sensations. But it is not the less true that all the recent experiments, even those of Fodera and Magendie, point to some connexion between the cerebellum and the power of voluntary motion. In the present state of our knowledge, it would be unsafe to contend for more than the probable existence of some such general relation.

This, then, is all that seems deserving of confidence respecting the functions of the cerebellum itself. But there are some singular phenomena which, though residing in other structures more or less near to the cerebellum, are so analogous to those already described, as to call for notice in this place. Magendie has described the results of injury to the crura cerebelli of a rabbit. Complete division of the right crus was followed by rapid and incessant rotation of the body upon its own axis, from left to right. This singular motion having continued two hours, Magendie placed the rabbit in a basket containing hay. On visiting it the following day, he was surprised to find the animal still turning round as before, and completely enveloped in hay. The eyes were rigidly fixed in different lines; that of the injured side being directed forwards and downwards, that of the other side backwards and upwards. If both crura were divided, no motion followed. Magendie hence concluded that these nervous cords are the conductors of impulsive forces which counterbalance one another, and that from the equilibrium of these two forces result the power of standing, and even of maintaining a state of rest, and of executing the different voluntary motions. The inquiry naturally presented itself, whether these forces are inherent in the crura themselves, or emanate from the cerebellum or some other source. To determine this question, portions of substance were removed from both sides of the cerebellum, but unequally, so as to leave intact $\frac{3}{4}$ on the left side and $\frac{1}{4}$ only on

the right. The animal rolled towards the right side, and its eyes were fixed in the manner already described. But the left crus being divided, the animal rolled to the left side. Hence it appears that section of the crus has more influence over the lateral rotation of the body than injury of the cerebellum itself; and that the impulsive force does not belong (at least exclusively) to the cerebellum. When the cerebellum was divided precisely in the median line, the animal seemed suspended between two opposing forces, sometimes inclining towards one side, as if about to fall, and again thrown suddenly back to the opposite side. Its eyes were singularly agitated, and seemed about to start from the orbits. Similar movements followed division of the continuous fibres in the pons Varolii. Serres has described a case of similar rotatory motion occurring in the human subject. A shoemaker, habituated to excess in alcoholic liquors, after great intemperance, was seized with an irresistible disposition to turn round upon his own axis, and continued to move so till death ensued. On inspecting the brain, one of the crura cerebelli was found much diseased, and this was the only alteration of structure visible in any part of the nervous system.

Medulla Oblongata. — The medulla oblongata, or "bulbe rachidien," is reducible into six columns, or three pairs, viz. two anterior, or pyramidal, which partially decussate, two middle or olivary, and two posterior or restiform, which proceed forwards without crossing. It is continuous in structure with the spinal marrow, and enjoys, by virtue of this relation, the same function of propagating motion and sensation. But it is distinguished from the spinal medulla by special and higher attributes, being endowed with the faculty of originating motions, as well as with that of regulating and conducting them. The medulla oblongata, with the cerebrum and cerebellum, constitute, in short, according to Flourens, those portions of the nervous system which exercise their functions "spontaneously or primordially," and which originate and preside over the vital actions of the subordinate parts. To this latter order of parts, which require an exciting or regulating influence, belongs the spinal medulla. In the superior class, Flourens seems to assign even a higher place to the medulla oblongata than to the cerebrum or cerebellum. For the cerebrum, he observes, may act without the cerebellum; and this latter organ continues to regulate the motions of the body after removal of the cerebrum. But the functions of neither cerebrum nor cerebellum survive the destruction of the medulla oblongata, which seems to be the common bond and central knot combining all the individual parts of the nervous system into one whole.

The medulla oblongata was regarded by Legallois as the mainspring or "premier mobile" of the inspiratory movements. He repeated before a Commission of the Institute of France the leading experiments on which his opinion rested. In a rabbit five or six days old, the larynx was detached from the os hyoides, and the glottis exposed to view. The brain and cerebellum were then extracted without arresting the inspirations, which were marked by four simultaneous motions,—a gaping of the lips, an opening of the glottis, the elevation of the ribs, and the contraction of the diaphragm. Legallois next removed the medulla oblongata, when all these motions ceased together. In a second rabbit, instead of extracting at once the entire medulla, it was cut away in successive thin slices. The four inspiratory movements continued after the removal of the three first slices, but ceased after the fourth. It was found that the fourth had reached the origin of the eighth pair of nerves. If, instead of destroying the part in which this motive influence resides, it be simply prevented from communicating with the muscles which are subservient to inspiration, a similar effect ought to be produced. Now it is obvious that the medulla oblongata must transmit its influence to the muscles which raise the ribs, through the medium of the intercostal nerves, and therefore of the spinal marrow, and to the diaphragm through the phrenic nerves, and to these through the spinal marrow. In another rabbit, therefore, the medulla spinalis was cut across about the level of the seventh cervical vertebra. The effect of this operation was to arrest the elevation of the ribs, the other three inspiratory motions still continuing. A second section was made near the first cervical vertebra, and consequently above the origin of the phrenic, with the effect of suspending the contraction of the diaphragm. The par vagum was next divided in the neck, and the opening of the glottis ceased. There remained, then, of the four inspiratory movements, only the gaping of the lips, which, however, was sufficient to attest that the medulla oblongata still retained the power of producing them all. This power had ceased to call forth the other three motions, only because it no longer had communication with their organs.

M. Flourens, in a recent memoir already referred to, has confirmed and extended the views first announced by Legallois. He has distinctly traced the comparative action of the medulla spinalis and oblongata,

on respiration, in the four classes of vertebrated animals. In birds, he found that all the lumbar and the posterior dorsal medulla might be destroyed without impeding the respiratory function, though it was arrested by removal of the costal medulla. In the mammalia the costal also might be removed; for though the raising of the ribs ceased, the action of the diaphragm continued as long as the origin of the phrenic nerve remained uninjured. In frogs, all the spinal medulla may be destroyed, except the portion whence spring the nerves supplying the hyoideal apparatus. Every part of the spinal marrow may be removed in fishes without affecting respiration; for all the nerves distributed to the respiratory organs of fishes have their origin in the medulla oblongata. It is hence apparent that the spinal marrow exercises only a variable and relative action on the respiratory function, in the different classes of vertebrated animals. In descending from the higher to the lower points of this scale, the spinal marrow is seen progressively to disengage itself from co-operation in these movements, while the medulla oblongata tends more and more to concentrate them in itself, till in fishes the proper functions of the two medullæ show themselves completely distinct, the spinal ministering to locomotion and sensation, and the oblongata to respiration. The medulla oblongata is, then, the "premier moteur," or the exciting and regulating principle of the inspiratory movements in all classes of vertebrated animals; besides participating, by virtue of its continuity with the spinal marrow, in the proper functions of that organ. From a second series of experiments, M. Flourens concludes that there exists a point in the nervous centres at which the section of those centres produces the sudden annihilation of all the inspiratory movements; and that this point corresponds with the origin of the eighth pair of nerves, commencing immediately above, and ending a little below that origin,—a result precisely agreeing with that obtained by Legallois.

Spinal Marrow.—It is apparent, that the functions of the three grand divisions of the nervous system, already described, have not yet been distinctly and fully ascertained. Our knowledge of those, which next fall under survey, is more definite and substantial. The vital offices of the spinal medulla—regarded by Legallois as the mainspring of life, and as alone regulating the actions of the heart and nobler organs—are now reduced to conveying to the muscles the motive impulse of volition, and to propagating to the sensorium commune, impressions made on the external senses. It is not invested with the power possessed by the cerebrum and cerebellum, and perhaps by the medulla oblongata, of spontaneously originating muscular motions. It is mainly, if not exclusively, a *conductor;* a medium of communication between the brain and the external instruments of locomotion and sensation. Flourens, indeed, conjectures that it also has the office of associating the partial contractions of individual muscles into "mouvemens d'ensemble," necessary to the regular motions of the limbs.

The great medullary cord is divided by a double furrow into two lateral halves; and each of these is again subdivided by the insertions of the ligamenta dentata into two columns, one posterior and one anterior. It has been long known that section of any part of the spinal marrow excludes from intercourse with the brain all those parts of the body which derive their nerves from the cylinder of medulla *below* the point of injury. The muscles, so supplied, are no longer obedient to the control of the will, and the tegumentary membranes similarly situated entirely lose their sensibility. This interruption of the relations which subsist between the central seat of volition and sensation, and the rest of the body, whether due to direct injury of the great nervous masses or communicating nerves, or produced by the pressure of extravasated fluids, by morbid growths, or by various poisonous matters, constitutes the condition known by the name "paralysis." In cases of this kind, it is frequently observed that the powers of sensation and locomotion are simultaneously impaired or destroyed. But examples are not wanting, even in the earliest clinical records, of the total loss of one of those faculties, with perfect integrity of the other. Such facts naturally suggested the belief that the power of propagating sensations, and that of conveying motive impressions, resided in distinct portions of the nervous system. This opinion, however, remained mere matter of conjecture until a recent period, when it was unequivocally established by Sir Charles Bell. From the original experiments of that most distinguished physiologist, repeated and confirmed by Magendie, it follows that the faculty of conducting sensations resides exclusively in the two posterior columns of the medulla, while that of communicating to the muscular system the motive stimulus impressed by volition, is the attribute of the two anterior columns. The same limitation of function is found in the nervous roots which spring from these separate columns. Thus each spinal nerve is furnished with a double series of roots, one set of which have their origin in the anterior medullary column, and one in the posterior. The spinal nerves are, in consequence of this anatomical composition, nerves of twofold function, containing in the same sheath distinct conti-

nuous filaments from both columns, and exercising, in the parts to which they are distributed, the double office of conductors of motion and sensation. It will afterwards appear, in our history of individual nerves, that all those which spring from the brain, except the fifth and eighth pairs, possess only a single function.

Sufficient experimental proof of the foregoing propositions has been furnished by Sir Charles Bell and by M. Magendie. Thus, division of the posterior roots of the spinal nerves is uniformly followed by total absence of feeling in the parts of the body to which the injured nerves are distributed, while their motive power remains undiminished. Magendie has further observed, that if the medullary canal be laid open, and the two posterior cords be touched or pricked slightly, there is instant expression of intense suffering; whereas, if the same or a greater amount of irritation be applied to the anterior columns, there are scarcely any signs of excited sensibility. The central parts of the medulla seem also nearly impassible. They may be touched, and even lacerated, according to Magendie, without exciting pain, if precautions are taken to avoid the surrounding medullary substance. In general, the properties of the spinal marrow, and especially its sensibility, seem to reside mainly on its surface; for slight contact, even of the vascular membranes covering the posterior columns, caused acute pain.

The first experiment of Sir C. Bell consisted in laying open the spinal canal of a living rabbit, and dividing the posterior roots of the nerves that supply the lower limbs. The animal was able to crawl. In his second trial, he first stunned the rabbit, and then exposed the spinal marrow. On irritating the posterior roots, no motion was induced in any part of the muscular frame; but on grasping the anterior roots, each touch of the forceps was followed by a corresponding contraction of the muscles supplied by the irritated nerve. Magendie has described the following experiments, which he has since declared were made without any knowledge of the prior ones by Sir C. Bell. The subjects chosen for the operation were puppies, about six weeks old; for in these it was easy to cut with a sharp scalpel through the vertebræ, and to expose the medulla. In the first, the posterior roots of the lumbar and sacral nerves were divided, and the wound closed: violent pressure, and even pricking with a sharp instrument, awakened no sensation in the limb supplied by the nerves which had been cut; but its motive power was uninjured. A second and a third trial gave the same results. Magendie then divided in another animal, though with some difficulty, the anterior roots of the same nerves on one side. The hind limb became flaccid and entirely motionless, though it preserved its sensibility. Both the anterior and posterior roots were cut in the same subject, with destruction of motion and sensation. In a second paper, Magendie has related the following additional facts. The introduction of nux vomica into the animal economy is well known to give rise to violent tetanic convulsions of the whole muscular system. This property was made available as a test of the functions of the separate orders of nervous roots. It was found that, while all the other muscles of the body were agitated, when under the influence of this poison, by violent spasmodic contractions, the limb, supplied by nerves whose anterior roots had been previously divided, remained supple and motionless. But when the posterior roots only had been cut, the tetanic spasms were universal. It would seem, however, that the seats of the two faculties of conducting motion and sensation are not strictly insulated by exact anatomical lines, but that they rather pass into each other with rapidly-increasing intensity. Thus irritation of the *anterior* roots, when connected with the medulla, gives birth, along with motive phenomena, to some evidences of sensibility; and, *vice versâ*, stimuli applied to the posterior roots, also undivided, occasion slight muscular contractions. In this last case it is, indeed, probable that the irritation travelled from the posterior roots upwards to the brain in the accustomed channel, and gave rise to a perception of pain, which prompted the muscular effort. Indeed, after division of the posterior nervous roots, ordinary stimulants, applied to the ends not connected with the medulla, produced no apparent effects, though the galvanic fluid directed upon either order of roots gave rise to muscular contractions. These were more complete and energetic when the anterior roots were the subjects of the experiment.

[To be continued.]

ON THE EFFECT OF FRACTURES

ON THEIR SURROUNDING PARTS,

More particularly in reference to Fractures of the Lower Extremity.

By John Grantham, Esq.

I shall, in the first place, endeavour to give a general description of each structure, with some pathological remarks;

and, in the second, make some observations on the diagnosis and treatment.

First.—Bone possesses two properties—solidity and elasticity; the former increases with age; the latter, which resides in the gelatinous and cartilaginous parts of the organ, is most observed in infancy. The vitality peculiar to bone consists only of organic sensibility and insensible organic contractility: from these two properties the formation of callus is derived, and the cicatrization of fractures is produced. Ossification is not properly completed until towards the 16th or 18th year; and sometimes later. Bones may be classed into three distinct states—the mucous, the cartilaginous, and the osseous: and in the union of fractures we have three processes—1st, the development of granulations; 2dly, the transformation into cartilage; 3rdly, the change of this cartilage into bone. This process requires time, according to the age and health of the patient, from 20 to 60 days. If the fractured parts be not kept from motion, and the parenchyma of cicatrization is prevented from uniting, an unnatural union will be formed, notwithstanding the effusion of the nutritive substances. The effects of fractured bone on the neighbouring parts are contusion, laceration, and effusion. In cartilage, animal sensibility is never met with, but mere insensible organic contractility or tone, and that in an obscure degree. Sympathies are almost wanting in this system, so that inflammation must assume a chronic type, unless we admit of this comparison: for instance, an inflammation of the cellular tissue assumes an acute form when it lasts only a few days, and a chronic one when it exceeds 30 or 40: now, in a cartilage, this last term may characterize an acute stage, whilst a succession of several months is required for a chronic one, instances of which are so frequently met with in diseases of the articulations. The fibrous tissue, which includes the tendons, ligaments, and periosteum, can only be known by negatives: it has not the sensibility of the nervous structure, nor the contractility of the muscular; it only obeys the action imparted to it. Nevertheless, vital activity in this structure is more than in bone and cartilage, as it is more frequently the seat of pain from laceration and inflammation. In a natural state, I said they were destitute of nervous sensibility, but in a morbid state they are highly sensitive. It is worthy of remark, that inflammation of this structure never produces suppuration. Pus is never secreted by the fibrous structure. In this structure, sympathies of every description are met with. All that is known respecting the nature of the muscular fibre is, that it differs from others; that it cannot be compared either with that of the nerves or vessels, nor with that of tendons or of the cellular tissue. Now we shall find, that in this point of view, all these systems differ essentially from each other; that, consequently, there cannot exist the slightest analogy between them in what regards their nature, from which the properties are constantly derived. In muscle, the two great sensations of life are very manifest; namely, animal and voluntary sensation. Inflammation prevents contraction in a muscle; laceration, contusion, and effusion, tend to impede animal contractility in the muscle. A deficient supply of blood to a muscle relaxes its powers. *The muscle must be in a state determined by the laws of its organization to answer to cerebral excitement.* The muscular system acts a very important part in sympathies which never take place without the two-fold action of the brain and nerves. The cellular tissue gradually decreases in the extremities, from above downwards: it is permeable throughout, yet it varies in density; indeed, this structure can only be regarded as a reservoir for fat and lymph. The cellular serum is much increased or diminished during digestion or after sleep, in consequence of the perspiration. It is well known that, in anasarca, the quantity of cellular serum is increased, and is entirely suspended in inflammation: there is no doubt but this fluid contains particles for nutrition. The fat is found in greater abundance in the upper portion of the extremities and in infants. The animal properties do not reside in this structure until it becomes morbidly affected: for instance, phlegmonous inflammation, &c. The healing art is indebted to Corvisart for having demonstratively shown, that almost all infiltrations of the cellular structure are symptomatic. The cellular tissue undergoes the same changes as the skin. From what has been remarked, the vitality of this membrane is very superior to the other organs with which it is classed, as the fascia, tendons, cartilages,

and ligaments. Suppuration takes place in this structure with a suddenness to which few organs afford a parallel. Granulations and cicatrizations may be said to have their origin in this structure. The cellular tissue which surrounds the arteries, is never the seat of serous effusion, nor does fat ever accumulate there: it is *rarely* attacked by inflammation. The veins are provided with a similar covering or texture, but much less dense and firm; it likewise contains no adipose substance, secretes little serum, suffers no distention in dropsies. The cellular substance envelops the muscles, the arteries, the veins, and, without doubt, the absorbents, forming, as Bordeu has so happily expressed it, an atmosphere for each. To the separation of life in the different organs, through the means of the surrounding cellular structure, reference must be made to the absolute distinctness of disease; nevertheless, experience shows that it is also an agent to convey disease. Effusion of serum, air, or blood, either separately or combined, takes place in the cellular structure, subcutaneous and cutaneous tissues. The effusion of serum appears to take place in the subcutaneous cellular tissue in a much larger proportion than in other parts: it has a particular tendency to accumulate there, in consequence, without doubt, of the excessive laxity of this membrane. The submucous cellular tissue is connected with the cellular tissue of the skin from which they are continued, and with which there is an analogy of structure, but with this difference,—the density of the former is less than the latter. The skin,—which consists of three membranes—the cuticle, the reticulum, and corium, the function of which is for inhalation and perspiration. The cuticle is very lowly organized, resisting the effects of suppuration, and other modes of destruction, for a great length of time. The reticulum Malpighianum is a fine mucous membrane, connecting the cuticle with the corium: besides nerves and absorbents, innumerable blood-vessels penetrate to its external surface; a vast number of sebaceous follicles are also dispersed through it, and diffuse an oil over the skin, which is very thin. The corium is also beset with the roots of the hairs.

Secondly.—The diagnosis of fracture is best ascertained by the use of the stethoscope. The following is an extract from what has been published by M. Lisfranc, and translated into English by Mr. Alcock:—"The stethoscope applied over the place of fracture, on the slightest motion of the part, conveys a much more decided crepitus than is perceived by the naked ear, during the most extended movements of the part. In many cases, even the slight pressure of the ear on the stethoscope suffices to produce the crepitation,—a circumstance of no small importance, as freeing the patient from the pain necessarily excited by the motion requisite in the manual examinations. The crepitus yielded by the more solid bones is sonorous, and resembles the sound produced by breaking a piece of wood across the knee: it is accompanied with a sensation of roughness unpleasant to the ear. The sound yielded by the spongy bones is duller, and resembles the effect of a rasp on wood; except that now and then this noise is broken by sounds of a clearer kind, like those afforded by the compacter bones, only not so loud. The sound from oblique fractures is stronger than from those which are transverse; but when one end of the fractured bone rides over the other the sound is then obscured, and in some cases may not be perceived without slight extension or counter-extension of the limb. If the fracture is comminuted, the sensation as of distinct portions of the bone is conveyed by the stethoscope. When fluids are effused around the fracture, a gurgling is combined with the crepitation, and which is compared to the sound produced by a shoe full of water." A dry crepitous rattle is produced by inflammation of the cellular structure, wherein the serum becomes suppressed and the cells distended with air, which may be mistaken for the crepitus arising from fracture: it is much louder, and may be distinctly heard by the patient or by-stander: it is heard by making gentle pressure with the fingers or end of the stethoscope over the injured part: it is most distinct on the third day, and decreases on the fifth: it is a sound apt to be mistaken for fracture of the fibula. We have a sound like this in the common subcutaneous emphysema, on pressing interruptedly with the hand on the affected part.

The treatment of fractures may be divided into mechanical and medical: the methods I have used in the mechanical part may be seen by reference to the 12th volume of the Medical Gazette.

In the medical part of the treatment of fractures, two great principles must be followed, and they will apply to all local injuries of the extremities; first, tranquillity of the arterial and nervous systems; secondly, complete quietude of the injured part. These principles lead to this object; viz. that the keeping of injured structures in their normal condition is most advisable for the action of restoration of the parts from the effects of contusion, laceration, or effusion. As local diseases become the source of constitutional derangement, so, on the other hand, local disease is often the result of a general disturbance of the system, and their reciprocal actions are produced through the medium of what has been called an universal sympathy. Dr. Armstrong used to say, when lecturing on local irritation, "the fever which would be developed had three stages; first, of depression; second, of excitement; third, of collapse. The great object in the management of this simple fever is to prevent the occurrence of inflammation. Inflammation, it must never be forgotten, may arise in the progress of simple fever, and thus it may be converted into common inflammatory fever; though unquestionably it often begins and terminates as common simple fever; that is to say, it is merely accompanied by general and local simple excitement; conditions which are fairly separable from inflammation." The changes in the generation of animal heat are effected by the energy of the vital powers only. The corium which covers the body, and the internal surface of the alimentary canal, contribute to regulate the temperature of the body. The opinion respecting the action of the cutaneous vessels in exciting, moderating, and almost extinguishing our heat, is supported by the physiological and pathological facts of some parts being frequently of a higher or lower temperature than the rest of the system. It is certain that transpiration is increased by elevation, and absorption is increased by depression of temperature. From these observations it will appear how necessary it is that after first placing the fractured limb in a proper position, we should attend to the animal heat, upon the regulation of which the success of the case much depends. Having restored the extremities to their natural temperature, by means of artificial heat through the medium of the stomach and skin, the next object is to attend to the force of the heart's action: here we must be guided by the health of the patient in reference to hereditary and common causes. When the fracture is simple, and the pulse full, from 10 to 16 ounces of blood taken from the arm, after reaction has taken place, acts as a powerful preventive to inflammation; at the same time it assists the action of medicine in unloading the alimentary canal, and exciting the glands and absorbents, so as to produce a healthy state of the excretions during the first four days. Cold applications to the part act very beneficially though the medium of the skin on the cellular structure, after which time it is necessary to be cautious in the use of cold to the skin. Mr. Abernethy used to say, when speaking of the regulation of temperature in the application of cooling lotions, "do not take the law into your own hands, and so diminish the temperature of parts as to kill them." It has been previously remarked, that the vitality of the skin and cellular structure is great, and matter forms there with rapidity. Now there is danger in the external application of cold during the suppurative stage; and it must be borne in mind that when there is a deficiency of blood there is also a diminution in the heat, and *vice versâ*. This leads to the consideration of injuries to the other structures,—the fibrous and muscular. The suspension of natural heat in the integuments would tend to excite and engorge the vessels pertaining to these structures: hence I consider it necessary in such cases to raise the standard of heat, and increase the action of the organs of perspiration. In local injuries, independent of fracture, it will be seen how important it is to pay attention to time in giving the limb rest, as well as the method of subduing the inflammation, and if I might make one general observation, it is this,—cold applications are the most injurious that can be applied to the skin in cases of inflammation of the cartilaginous, fibrous, tendinous, or muscular structures. The importance of general and local bleeding appears very evident, with the precaution not to deprive the injured structure of its vital properties.

TREATMENT OF SORE NIPPLES BY NITRATE OF SILVER.

To the Editor of the Medical Gazette.

Sir,

There are few sources of suffering, of an apparently trifling nature, productive of greater misery, than chaps, abrasions, and inflammation of the nipples, in nursing women. For a long time, I tried all the measures usually recommended, both regular and empirical, to cure them; but I never had any reason to congratulate myself on my success. I hear daily of the distress occasioned by such accidents; and having at length fallen on a method of treatment which never fails to give immediate relief, and ultimately to effect a cure, I cannot withhold it longer, lest there be even one practitioner in extensive practice who may be unacquainted with it, and so lose the benefit of a remedy which I regard as a blessing to every woman in the condition before mentioned. Indeed, under my impressions of its value, I should be culpable in not making it known. Many, perhaps, may know it; but there are certainly many who do not, much to the loss of their puerperal and nursing friends and patients; and for the sake of these I may be excused for troubling you.

My method is briefly this. Having gently, but carefully, dried the nipple, touch it freely with a sharp pencil of nitrate of silver. Be sure to insinuate the pencil into the chaps or chinks; then wash the nipple with a little warm milk and water. In most instances, the pain, though smart at first, soon subsides, and a little simple ointment, or one made with the flowers of zinc, is all that is requisite to heal the sore. I occasionally wash the nipple with a saturated solution of borax, before and after suckling the infant. Some suffer a great deal of pain from the application of the caustic; this must not be heeded. A draught, containing an opiate, such as Sol. Mur. Morph. 30 drops, soon brings relief, and the part is presently easier. Some require to be touched more than once, —nay, several times; but each succeeding time it is less painful. I have heard of a solution of nitrate of silver being tried: I can positively assert that it is inferior to the solid caustic, both in relieving and healing these painful affections. Now, my apology for asking you to insert these remarks is, that I am deeply impressed with the value of the remedy recommended, and should consider myself much to blame if I did not communicate a fact which I am sure will be highly useful to some, if not a very large proportion, of the profession.

I am, sir,
Your obedient servant,
Alex. J. Hannay, M.D.

Anderson's University, Glasgow,
July 25, 1834.

ACTION OF MERCURY IN THE ABSORPTION OF LYMPH.

To the Editor of the Medical Gazette.

Sir,

I have perused in your journal the Gulstonian Lectures delivered by Dr. Burrows at the College of Physicians, with that pleasure which the result of patient investigation and clear induction must always elicit. Those lectures are important from their practical utility, and as shewing how much chemistry may be brought to bear on pathology. Nevertheless, at the end of the second lecture, published in your Gazette for July 19, 1834, a theory is broached, which, however ingenious, does not appear to me tenable. If you can find room in your pages for the following remarks on the subject, you will oblige me by an early insertion.—I am, sir,

Your obedient servant,
W. Martin Coates.

Endless-Street, Salisbury,
August 4, 1834.

If I understand Dr. Burrows rightly, he attempts to explain the power of mercury in producing the absorption of lymph deposited during inflammation, by considering that the secretions being rendered alkaline by that medicine, dissolve the lymph, and so render it more fit to enter the minute absorbents; and mentions as an instance, its influence in causing to disappear the lymph deposited on the iris during inflammation of that membrane. Were this allowed, how could we account for the rapid healing

of many ulcerations, particularly those of syphilitie origin, under the influenee of mereury?—or, let us pass to an affection of the eye,—corneitis. Here we have, in the first stage of this inflammation, inereased vaseularity; in the second, deposition of lymph on or between the layers of the eornea; and if the excitement be too great for the weak vitality of the part, uleeration takes plaee. Now all these stages are benefited by mereury after depletion; so that, in one instance, lymph is absorbed; and in another, (as eireatrization is produeed by the organization of lymph in the form of granulations,) this substance must have been deposited during the mereurial proeess.

The rapid healing of uleers, under the influenee of mercury, is well seen in those of the cornea termed strumous: here we see the white speek of lymph in the cavity formed by the uleer: this beeomes organized; the edges of the uleer approaeh each other, and the little white cieatrix is formed, whieh is rarely or never entirely absorbed.

Now I regard thus the influence of mercury:—That it has great power in controlling inflammation in every stage: when this aetion is arrested, there can be no further deposition of lymph; and that which has been poured out is soon attacked, and removed by the absorbents as a foreign body.

In ulceration of the eornea, mercury tends to arrest the inflammatory aetion of whieh the uleer is the eonsequenee. The seeerning vessels pour out lymph—vessels shoot into it, forming granulations. These rise to the surfaee, eontract, and the edges of the ulcer are brought together; the lymph then becoming no longer useful is absorbed, and a fine cieatrix is left, which, unless placed in the very axis of vision, cannot interfere with it.

So that mereury eauses the absorption of lymph, or the deposition of it, as in the formation of granulations; not by any specific action on the tuberele of lymph, or ulcer, but by controlling inflammatory aetion—the cause of both.

I offer these remarks with diffidence, when I eonsider the high character of Dr. Burrows, as an aeute observer and eorrect reasoner; but, as truth is my object, if I am in error, I shall be grateful for correetion.

CASES OF UNUNITED FRACTURE,

Treated by the introduction of a Seton,

By E. M'Dowel, Esq.

[The two following interesting cases are related in a letter addressed by Mr. M'Dowel, an eminent surgeon of Dublin, to one of his friends in London, aecompanied with the permission to make them public, if he should think proper. The gentleman to whom they were addressed has transmitted them to us, for insertion in this journal.—Ed. Gaz.]

Case I.—Miehael Flood, aged 25, was admitted, under my care, into the Richmond Surgieal Hospital, Brunswiek-Street, January 16, 1830, with ununited fracture of the left femur. The fraeture was below the eentre, and was very oblique; the bones overlapped, the muscles were wasted and flabby, and on attempting to bear any weight on the limb it bent inwards: the limb was useless.

Previous history.—Has been very healthy; never had syphilis. The fraeture oeeurred two years previously, and splints were kept on for two months. Being then urged to use the limb, he got out of bed; and, in the first attempt to bear on it, the recently-eonneeted parts separated. Splints and very firm pressure were employed for three months longer; but no union: a blister was afterwards applied; but at the expiration of twelve months the fracture was still disunited. He then came to Dublin, and was in Stevens' Hospital for nearly six months. A bandage, previously dipped in glue, was applied; short splints, firmly bound on; and he was allowed to go about on erutches. At the end of six months there was less yielding at the fraeture, but no union, and a useless limb. He then eame under my care. The period for using mercury, to exeite a sufficient degree of action in the parts, and thus favour the formation of callus, had gone by. On the 20th of January, an incision was made down to the fracture on the inside of the thigh, and a long seton needle, with a thick seton of silk, was passed between the overlapping bones, and these brought out at the outer side of the limb. The operation was bloodless. The limb was then placed in a

double-inclined plane. On the following day there was some local and constitutional disturbance; the fever lasted six days, and was moderate; the inflammation was not violent; the suppuration became rather profuse, and in about fourteen days the seton was removed. The progress to perfect cure was very slow, but was so complete, that in 1832 this man was employed as a grave-digger in the burial-ground attached to the Cholera Hospital, where, unfortunately, he had full occupation. The limb recovered its plumpness, but was about one inch shorter than the sound, the bones having overlapped.

In the following case the seton failed:—

Anthony M'Donnell, aged 52, admitted November 10th, 1832. Four months since, the left humerus was fractured very obliquely, two inches below its centre. He was in Stevens' Hospital for twelve weeks, during which time the arm was carefully kept in splints, with a tin case for the elbow; a glue bandage was employed, and he was also salivated, but without advantage. A false joint was developed, the ends of the bones became smooth and rounded. Emaciation of the arm. Nine years previously he had syphilis, and was twice salivated; and his gums afterwards remained spongy and tender. Since the last course of mercury, he has had constant pain in the vertex, and some dyspnœa.

November 12th.—Seton passed; little fever succeeded; but bronchitis supervened, with a thin, fœtid, and profuse discharge from the seton, which it was necessary to remove in ten days. No benefit was derived from the operation.

66, Eccles-Street, Dublin,
July, 1834.

PROFESSORSHIP OF ANATOMY AND SURGERY IN THE UNIVERSITY OF DUBLIN.

To the Editor of the Medical Gazette.

Sir,

The advertisement inserted in the newspapers, and which was copied into the London Medical and Surgical Journal of the 12th instant, does not state correctly the law regarding the professorships in the School of Physic in Ireland. It would appear by the advertisement, that the board of Trinity College might, if they thought fit, have continued the present professor of anatomy and surgery for another period of seven years, without the form of an election; whereas the School of Physic Act directs, that no professor can hold his office beyond fourteen years without being again elected to it. It was, therefore, in conformity with this law, and not by any act of the board, that the professorship became vacant, to which I was re-elected on the 29th instant, although the utmost pains had been taken to create a prejudice against me, by grossly false and absurd representations of the evidence I gave before the Parliamentary Committee on Medical Education.—Your obedient servant,

James Macartney,
Professor of Anatomy and Surgery,
Trinity College, Dublin.

Dublin, July 31, 1834.

MEDICAL GAZETTE.

Saturday, August 9, 1834.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

Cicero.

MEDICAL EVIDENCE AT CORONERS' INQUESTS.

There is a clause in the new County Coroners' Bill, which is of great importance to medical men throughout the kingdom; and we notice the bill solely with reference to that clause. It is of little or no consequence to members of the profession, as such, that a new distribution of coroners throughout the country is to take place on and after the 1st of January next; that the electors are to be those, and those only, who are qualified to vote for knights of the shire; that the business of the election, if a poll be demanded, must be finished in one day; or even that there is not a word in the bill to provide for the choice of coroners from among medical practitioners exclusively. This

latter point may be considered by some as a disappointment or a grievance. We have seen a notorious politico-medical contemporary,—himself a disappointed candidate for a coronership,—insisting largely that none but medical men should fill the office, (though heaven knows what claim *he* has on that score!): and we are aware that it has of late been the opinion of several respectable persons that the office of coroner would require for its proper discharge a competent share of medical information. From the exclusive doctrine of our worthy contemporary we beg leave wholly to dissent; but we are ready to allow that there is, to a certain degree, an appearance of reason in the opinion of the other party. A competent share of medical knowledge we should think most desirable for the greater number of our public functionaries, and for none, perhaps, more than for the coroner, of whose official functions it forms so large a part to inquire into the nature and manner of death in doubtful and mysterious cases. It does not, however, follow that because an acquaintance with medical matters is so likely to be useful to the coroner, that therefore he should be, *par excellence*, a medical man. A member of the profession, it is true, if he give up his practice, or the greater part of it, may duly prepare himself for presiding at inquests. Undoubtedly he may, by a moderate application to the study of the law, render himself a sufficiently able judicial investigator. But it must not be forgotten that there is much connected with the office in question that is not medical: many of the coroner's duties resemble those of the Sheriff—for whom, indeed, on certain occasions, he has to act as a substitute; and above all, the person who acts in the capacity of judge in a court of criminal inquiry, should be possessed of an amount of legal knowledge respecting evidence, and a familiarity in the mode of eliciting and regulating it, for which we are not aware that any particular part of his education is peculiarly adapted to prepare the medical man. With this feeling, we by no means regret that our legislators have omitted to point out practitioners of medicine as the fittest candidates for the office of coroner; and those who turn their thoughts to the subject will, we doubt not, agree with us, that medical men of any ability have quite enough to occupy their time, and that legitimately and honorably, without meddling with the business of low lawyers—such as the coroners of the present day usually are—or embroiling themselves with the pettifogging routine which commonly characterizes the proceedings of public inquests. The vocation of the medical man is better displayed when he presents himself as a witness, by whose enlightened testimony difficulties are removed and obscurities cleared up. In doing this hitherto, he has been subject to hardships: he has found it burthensome and discouraging to tender his services before coroners' courts; and when he has been able to escape from the unpleasant duty, he has gladly left it to be performed by any one else—even the incompetent and the worthless. The chief vexation was, that his valuable time was taken from him without remuneration, and generally without even thanks. This, however, will not be the case for the future.

In the new Bill there is a clause, which was introduced among other amendments in the progress of the measure through the Upper House. It is as follows:—

"And whereas at the taking of inquisitions on the bodies of persons lying dead, it is frequently necessary, for the more satisfactory explanation of the cause of the death of such persons, that a *post-mortem* examination of such bodies, or of some part thereof, should be made by some surgeon or other person of the medical profession, and his

evidence given on such examination: And whereas there is at present no remuneration provided for such surgeon or other person, whereby great difficulties exist in procuring such *post-mortem* examination to be made: Be it therefore enacted, that in every case where the assistance of any such surgeon or other person, for the purpose of such *post-mortem* examination, and his evidence thereon, shall be thought necessary and required by the coroner and jury, the constable of the parish or place in which such inquest shall be held shall, on the direction of the coroner, pay to every such person, if he shall require it, a reasonable fee (such fee in no case to exceed the sum of one pound) before he shall be called upon to give his evidence; and such sum so paid by the constable shall be reimbursed to him out of the funds provided for the relief of the poor of such parish: provided always, that such remuneration shall not be given to any such person, where he shall be called upon only to give evidence as the medical attendant of such deceased person during his lifetime, or at the time of his death."

Although there is nothing in this clause particularly striking or dazzling at first sight, in the way of "boon" to the profession, yet it contains matter of much moment, inasmuch as it recognizes and establishes the right of medical men to remuneration, when called upon to give their professional aid on inquiries touching the cause of death. The amount of remuneration, it is true, is humble in the last degree, yet still it is gratifying that the principle of treating medical practitioners as if they were the slaves of an arbitrary government, is henceforth abolished. It was an odious, and in some sort a tyrannical power, which the coroner hitherto possessed, of calling upon any professional man, no matter how high his rank, or how engrossing his avocations, to attend an inquest, and give his opinion on whatever subject appeared to that officer to require such elucidation. We do not say that the power was often exercised in a tyrannical or oppressive manner, or that it was always obeyed when sought to be put in force; but the very fact of such a mode of oppression being in existence, liable to be practised by any coroner who should feel disposed for vexatious harshness, was a grievance disgraceful to the free subjects of a constitutional government—not to say disgusting and intolerable to the members of a liberal and enlightened profession.

The last proviso of the above clause, however, by no means pleases us. Its purport is, that a medical practitioner who shall have been in attendance on the deceased person during his lifetime, or at the time of his death, shall not be entitled to remuneration for the evidence he may be called upon to give at the subsequent inquest. This, we presume, proceeds on the supposition that the medical attendant is always sufficiently remunerated for his professional services, and that therefore he can very well afford to give his evidence at the inquest for nothing. But the framers of the Bill labour here under a great mistake; for, generally speaking, we may be bold to aver, that in perhaps not one out of fifty of the cases in which inquests occur, are the medical attendants paid for the exercise of their art. It may happen, indeed, in cases of suicide occurring in high life, and where fatal accidents have happened to persons of quality, that the aid tendered by the professional man is remembered even in the hour of trouble, and that his fairly-earned *honorarium* is not withheld from him. But where the indigent wretched are, as they are most likely to be, the subjects on whom medical assistance is bestowed in the moment of danger, is it reasonable that to the hardship of rendering those services gratuitously should be added the nuisance of being obliged to attend an inquest, and give evidence, without even a hope of remuneration? We think that those who framed this part of the clause must have had parish

surgeons, or some other salaried functionaries, in view, and only thought of deaths occurring in the workhouse; for otherwise we cannot see a shadow of justice in the enacting of such a provision.

But there is still, we suspect, a mode of escaping the unpleasantness of this arrangement, and of frustrating the niggardly scheme of the managers of the Bill. It would seem to us, that, according to the general tenor of the clause, a *post-mortem* examination must be paid for in all cases; and surely there are few instances in which a conscientious opinion can be pronounced, relative to the cause of death, without an inspection of the body. If the medical man be called on, like any other witness, to speak to some *matter of fact* connected with the circumstances of the death which are investigated, he must, of course, give his testimony, as a duty he owes to society and the laws of his country; but if it be his professional *opinion* which is required, he stands in quite a different position;—he is just as fully entitled to his fee, in this case, as the lawyer who examines him, or the presiding officer, who cannot satisfy the jury without availing himself of his professional aid. In the last resort, however—and supposing his just and equitable rights denied him—why may he not insist on the necessity of examining the body, and demand his fee, before he utters a syllable of his evidence? This is obviously the proper way in which to act.

We perceive that the Bill does not extend to Scotland or Ireland. In the former, there are no coroners; inquisitions regarding the cause of death are conducted there, in many respects, on a much better principle than they are here; while in Ireland, we believe, the right of medical men to remuneration for their evidence on inquests, has long been recognized and respected. Whether or not the other provisions of the Bill be desirable in the sister kingdom, we are not prepared to say; nor should we feel warranted in giving any opinion.

We have only to add, that the Bill is at present *all but* passed—being delayed by a mere point of form. The Commons are desirous of having it clearly expressed in the Bill that the coroner's is an open court; a clause which the Lords dispensed with as superfluous, when the measure was before them. The consent of their lordships is expected this day (Friday, the 8th).

ST. GEORGE'S HOSPITAL.

CLINICAL OBSERVATIONS ON FATTY TUMORS,

By Mr. Brodie.

Mr. Brodie removed two tumors of this description, one lying on the dorsum of the scapula of a man, the other occupying the same situation in a woman. After the operations, Mr. Brodie made the following observations.

As there are no other operations, I shall take the opportunity which is offered, by your being here assembled, to say a few words in illustration of the nature, varieties, and treatment of fatty tumors.

Such tumors as those which I have just removed are of very common occurrence. They are composed of fat, but it is finer, more delicate, and of a looser texture, than ordinary fat. They are usually found lying immediately under the common integuments, but not always, for I have occasionally known them to be deep-seated under muscles. They are always imbedded in ordinary fat; so that under the skin of the penis, scrotum, and eye-lids, where there is simple cellular membrane, and no fat, they never exist. In general they are of spontaneous origin; by which, of course, I mean nothing more than that we do not see the cause on which they depend. In some instances they seem to arise from long-continued pressure, and in others they follow a slight accident. A gentleman consulted me concerning a fatty tumor, situated upon one shoulder. He told me that some years ago, in making a sudden effort with his arm, he experienced a sensation as if something had cracked or burst near his shoulder, and that very soon afterwards, exactly in the spot to which this sensation had been referred, he discovered a small tumor, which gradually increased to a very large size. A lady, whom I have lately seen, has what appears

to be a small fatty tumor, also in the neighbourhood of the shoulder, of which she gives a history similar to that which I have just related.

If you examine one of these tumors, you find, as I have already mentioned, that it is composed of fat, a little differing in its characters from the fat which is of original formation. It is inclosed in a thin membranous cyst, closely adhering to the substance of the tumor by its internal surface, and more or less firmly adhering to the surrounding parts by its internal surface. If the tumor be of moderate size, and it has not been subjected to the influence of pressure, this external adhesion is generally very slight; but if the tumor has obtained a large size, or it has been much compressed, the adhesion becomes very firm and close.

For these tumors I know of no remedy but excision. But here, of course, two questions arise: first, will any harm arise if such a tumor be allowed to remain; and secondly, is it liable to return, as malignant tumors are liable to return, after the operation?

In answer to the first of these questions it may be observed, that these tumors often, and indeed generally, continue to grow until they become annoying to the patient from their bulk, as well as from the deformity which they occasion. But this is not all: I have known suppuration to take place, and a large abscess to be formed in the centre of the tumor. This had occurred in a patient, who was admitted some years ago into this hospital, with a large fatty tumor, weighing several pounds, situated on the back, between the shoulders. The abscess had burst, and there was a constant and profuse discharge from it of pus mixed with oleaginous fluid. Of course an abscess of this kind cannot be healed, and the removal of the tumor is, under such circumstances, a matter, not of choice, but of necessity.

I remember to have heard Sir Astley Cooper observe, that he had reason to believe that tumors which were originally composed of fat, sometimes become converted into tumors of a malignant kind; and I once removed a tumor in which it appeared to me that such a conversion of its structure was taking place. When the tumor, to which I allude, was divided with a knife, the greater part of its substance seemed to be composed of fat, but of greater firmness and more dense than usual, and in certain spots there were deposits approaching very nearly to the appearance of medullary substance.

With respect to the other point—namely, the liability of these tumors to return—I may say, that I am not aware of more than a single instance in which this happened in my own practice. The case to which I allude was that of a gentleman whose case I have already mentioned, and who felt something give way near his shoulder just before the tumor appeared. The tumor was large, and the cyst adhered more closely than usual to the surrounding parts; and I have no doubt that the recurrence of the disease was to be attributed to some small portion or portions of the tumor not having been removed in the first operation. The second operation was very troublesome, on account of the close attachment of the tumor to the cicatrix; but it was successful, the patient having lived for several years afterwards without the tumor being regenerated.

In removing one of these tumors, your best mode of proceeding is to make a free incision at once into its substance. You will then see very distinctly the membranous cyst, by which it is enveloped; and as this adheres closely to the tumor, and but loosely to the surrounding parts, you may in many instances complete the operation with your fingers; having no occasion to use the scalpel, except at one point, where the principal blood-vessels of the tumor penetrate into it. In other cases, however, a more elaborate dissection is necessary, in consequence of the cyst having contracted a firm adhesion to the parts in the neighbourhood.

There is another kind of fatty tumor of much more rare occurrence than those of which I have just spoken, in which there is an unnatural deposit of fat in one or more parts of the body, but not inclosed in a distinct cyst, having no well-defined boundary, and which, therefore, cannot be removed by an operation. If you were to cut down on one of these tumors, you would not know what was natural structure, and what was diseased structure; what ought, and what ought not to be taken away. You may, however, do much in these cases by means of medical treatment. The solution of pure potash, or the *liquor potassæ*, exercises a very beneficial influence over the disease. I have known some cases in which this unnatural deposit of fat became almost entirely absorbed under the use of this medicine. The following will serve as an example. A poor fellow was under my care, as an out-patient of the hospital, with some large tumors of this kind in his neck; one in front, making an enormous double chin; and two others projecting at the posterior part; one below and behind each ear. He said that they gave him no pain, but that they were a very serious calamity in other respects. He was by occupation a gentleman's servant, and his strange and grotesque appearance was such that no one would hire him. I gave him the *liquor potassæ*,

gradually increasing the dose, until he took as much as a drachm three times daily. Under this treatment, which was continued for eight or nine months, with some occasional intermissions, the tumors became reduced to about one-fourth of their original size, and this without the smallest injury to the general health, and not much loss of flesh generally. It might be one or two years after this, that, in visiting a private patient in Mortimer-Street, this man opened the door; and when he saw me, said, "Sir, I must thank you for the cure which you made of my complaints. I have continued quite well, and have been in service ever since I left the hospital." There is another circumstance in this case which is worthy of being noticed. At one time, while there was an intermission in the use of the potash, I gave the patient the tincture of iodine. The consequence was, that he became generally very much emaciated, while the tumors grew larger, and continued to do so, until the iodine was discontinued.

There is a third kind of fatty tumor, which never attains a large size, but which exists in combination with others of the same kind, in various parts of the body. You find them under the integuments, in the arms, back, or anywhere else, varying from the size of a pea to that of a small walnut; and sometimes ten, or twenty, or even more than that, in number. If you dissect out one of these tumors, you find it composed chiefly of fatty matter; but it is of a much more firm and dense structure than the common fatty tumor. It is manifestly absurd to attempt to remove these tumors by operation. There would be no end to doing so, as one forms after another, and they evidently depend on a peculiar state of the general system; besides which, you may get rid of them by other means. The medicine which I have just mentioned, that is, the *liquor potassæ*, should be given to the patient in large doses; and when it has been taken for about a month or six weeks, or even sooner, the tumors will become lessened in size, and at last they will entirely disappear.

There is still another, that is, a fourth variety of fatty tumor, which is of very rare occurrence indeed. The tumor is composed of a very dense and solid fat, covered by a very thin and smooth membrane. But the membrane is not simple, like that of the common fatty tumor, but reflected like the *peritoneum*, or *pleura*, or *tunica vaginalis*. When you cut down on the tumor, you first expose a cavity formed by the external layer of this reflected membrane, and you find the tumor covered by the internal layer of the membrane, attached to one side of the cavity, and projecting into it as the testicle projects into the cavity of the *tunica vaginalis*. These tumors are of a small size, and several may exist in different parts of the body. I mention them merely as a matter of curiosity, as I cannot undertake to point out the marks by which they may be distinguished from other tumors in the living person.

CLINICAL OBSERVATIONS,

By Mr. Cæsar Hawkins,

At St. George's Hospital.

1. *Cancer of the Breast, &c.*
2. *Cancer of the Uterus.*
3. *Phagedenic Ulcer of the Face in old persons.*
4. *Cancerous Tumors of the Face and Nose.*
5. *Cancer of the Lip and Nose.*

There is scarcely any subject, perhaps, which, considering the present state of surgery, is so little understood, as to its causes and different forms, as cancer. It is, indeed, but a few years since all hard swellings of every sort were classed together under the name of scirrhus; and this term being also used for the early state of cancer, a confusion was introduced which led to the consideration of every hardness, even the most innocent, as a malignant disease. Nor is the distinction even now understood on the Continent, for you will find persons of eminence removing the neck of the uterus, for what they call scirrhus of that organ, in young women of 18 or 20, some of whom have subsequently borne children; the real disease having, of course, been only that hardness which is produced by inflammation, and which is perfectly curable without such an operation as this.

Much progress has, indeed, of late been made by the distinction between the class of fungoid tumors and cancer. Tumors which were at first called soft cancer by many surgeons, and which were supposed by Sir Everard Home to be cancer of muscular structure, but which are now understood to belong to a distinct genus, having at least three different species—the melanoid, hæmatoid, and encephaloid. Cancer and fungous tumors are, indeed, allied to each other; so that the two structures may be found together, or a tumor of one kind removed in any part may be followed, in the same place, by another of the opposite kind. Even the local cancer of the scrotum may be succeeded by fungus hæmatodes; for a case of chimney-sweepers' chancer has been published by

Mr. Langstaff, which was operated upon, and the patient died with fungus hæmatodes of the os innominatum and lumbar glands and liver. Still, notwithstanding their alliance, there are many particulars, besides their structure, in which the two kinds of disease differ materially from each other, and which influence our practice. It is to British surgery exclusively that we are indebted for this distinction, (and especially to the labours of Hey, and Abernethy, and Wardrop); a distinction which is even yet so little comprehended by our continental brethren, that a common vascular nævus is called by the name of fungus hæmatodes,—the only point in common being the liability to hæmorrhage.

But while a good deal has been done with regard to fungoid diseases, much less progress seems to me to have been made in our knowledge of cancerous complaints, though there are, probably, at least as many species of cancer as of fungus, differing from each other in their origin and appearance, in their symptoms and progress, the course of which ought to be distinguished from each other, as requiring a different line of treatment: some cases being attended with much new structure, others with scarcely any tumor; some being extremely painful, others causing very little suffering; in some the constitution being highly impregnated with the poison, in others the disease seeming nearly local.

You may have watched with advantage the case of Sophia Green, with cancer of the breast; in whom an immense mass of hard tumor involves the gland and all the neighbouring structures, the skin is studded with numerous cancerous tubercles, and the glands, both in the axilla and the neck, are enlarged with the same disease. You have seen her suffering in the most frightful manner, from pain in the tumor, which is extensively ulcerated, sloughy, and bleeding. You see her exhausted and nearly dying, chiefly from incessant vomiting and obstinate constipation, making it not improbable that some part of the alimentary canal is obstructed with a cancerous tumor; and from some symptoms of incessant pain in the neck and head, and inability to move her head without holding it with her hands, it seemed, at the time of her admission, not unlikely that even the vertebræ of the neck might be affected with the disease *. On the other hand, I had a patient under my care three weeks, for another complaint, before she even mentioned to me that she had had cancer of one breast for twenty years, which had been ulcerated twelve years; so slow had been its progress, and so little suffering had she experienced. Some years afterwards, a similar tumor formed in the other breast, with enlargement of the axillary glands.

You have also had the opportunity of contrasting this disease, as it affected the uterus, in two patients, in contiguous beds. In the one, who died very recently, the bladder having been destroyed to a considerable extent before her death, you saw the most dreadful suffering for a long time; her countenance not only shewed how acute her pain must have been, but had the exact expression and appearance which cancer usually produces. In the other, Sophia Purchase, who came into the hospital when nearly dying of hæmorrhage from the cancerous tumor of the uterus, some months ago, there has been, till lately, scarcely any pain, and her countenance throughout has been calm and resigned, without any other appearance than that of extreme paleness from loss of blood. Now, indeed, she suffers a good deal; but it is not so much from the cancer of the uterus as from the bowels, which are irritated, and in a state of frequent diarrhœa, because the ulceration has destroyed the front of the rectum, and made it one cavity with the vagina.

It is evident, then, that the differences I allude to do not arise merely from the particular organ or structure which is the seat of the cancer, since the course of the disease is so different in the same organ. Neither does the degree of pain depend on the mind and temper only, though, no doubt, this is capable of producing some difference; nor does the different progress of the complaint arise merely from an original difference of constitution, though some influence is probably derived from this circumstance also. What I wish you to understand is, that in the same structure, (the breast, for instance, where you can most easily watch its course), and in persons apparently of the same constitution, you will meet with four or five different kinds of cancerous tumor; so that, if you see any one of them in its early stage, you can anticipate, to a certain extent, what will be its course, its appearance at another period, its comparative ra-

* This woman was examined after the lecture, as Mr. Hawkins found that she had died on the preceding night, and besides the local disease mentioned in the lecture, which had softened two of the ribs below it, it was ascertained that there was a tumor in each ovarium, consisting of several cysts of fluid, communicating with the fallopian tubes, which were distended with a brownish semifluid substance, while the uterus itself was healthy. These tumors, filling the brim of the pelvis, pressed upon and obstructed the rectum, as it lay just in the angle between them. The lumbar glands were also enlarged and pulpy. The fifth cervical vertebra had some irregular bony projections from its body, and in the interior were some cells of bloody fluid, which Mr. Hawkins was inclined to think might be the result of cancer in the bone.

pidity, or its influence upon the system; so that you will be guided by this knowledge in having recourse to an operation, or abstaining from it.

At the same time, you are no doubt aware that malignant diseases are really much influenced by structure and texture, some parts being, for instance, more liable to one form than the other; so that cancer is common in the breast, and fungous hæmatodes rare; while, on the other hand, fungous diseases of the testis are frequently met with, and cancer is scarcely ever seen in that organ. So again, fungus hæmatodes of a muscle, or other soft texture, runs a somewhat different course from the same disease in a bone; and cancer is modified according as it occurs in the breast, or stomach, or uterus. In the skin especially, you will see in general a considerable difference in its appearance and progress, and degree of malignancy, from cancer in other parts of the body; and even different parts of the skin seem to be to a certain extent, differently affected.

I will now proceed to consider some affections of the skin of the face, in which I think you will see three different diseases, all ultimately fatal, if left to themselves, and all frequently spoken of under the name of cancer, or little distinguished from each other, though really requiring to be understood in practice.

1. The first I have been accustomed in my lectures to call the *phagedenic ulcer of the face of old persons.* It is an ulcer generally seen at the angle between the cheek and nose, or in the inner angle of the eye, the surface of which is dark and shining, never deep or painful, never bleeding or forming a tumour in the skin around it, or from its surface, and not secreting the fœtid discharge of cancer. As I have not lately had an example of it at the hospital, I will not now dwell upon it further; but refer you to a very good account of it, recently published in the Dublin Hospital Reports, by Dr. Jacob. Dr. Jacob does not seem, however, to be aware, of what I believe is the case, that this ulcer possesses this character of malignancy,—that there is a new structure formed, at first like a small excrescence in the cutis, not elevated like a wart, but rather flattened like a mole; the remains of which may even afterwards be traced in some parts below the ulcer, in the shape of a thin layer of dark-brown substance, evidently distinct from the ulcer and from the surrounding parts. It possesses, however, as far as I know, the very lowest degree of malignancy; so that the glands are not at any time enlarged, nor is any contamination of the system evinced by the appearance of malignant disease in any other part of the body. If, therefore, the thickening or ulcer be removed by the knife, with the whole of the basis or new structure, the patient may be considered quite safe from any return of the disease, either in the same part or elsewhere. But if, on the contrary, the disease be irritated by caustic or other means, which do not eradicate this basis, the patient may be quickly carried off, with frightful destruction of the cheek, and eyelids and nose, and much constitutional disturbance.

2. The next case may be called the *cancerous tumor of the face of old persons.* I say of the face, because I have not seen it elsewhere; and I call it cancerous tumor, because it is not like the more usual form of cancer of the skin, though it has partly the appearance of cancer, as it is sometimes met with. In its early stage there appears to be a little round tumor in the substance of the cutis, which is for a long time indolent, and without pain, and situated very often on the prominence of the malar bone, or centre of the cheek. At last the surface ulcerates, and a prominent mass of firm florid red granulations arises from the surface, to the height of an inch or more, much larger than the tumor beneath, and overlapping the skin around it. This fungous projection preserves its circular or oval form, discharges a thin, but not unhealthy pus; but becomes, towards this period, very painful, the pain being of a lancinating kind.

Here is a drawing, taken from W. Standfast, who is now in the house for a return of this complaint; and this is the preparation of the tumor, after its removal. Another plate and preparation before you, will show you the disease when somewhat larger in size, but still preserving its characteristic appearance, in a patient who was also in the hospital last year. This kind of tumor seems to possess a second degree of malignancy; it contaminates the surrounding parts, which are gradually converted into a structure similar to the original tumor; and it now and then, but not often, affects the absorbent glands: this is perhaps the case in Standfast; but it does not, as far as I know, affect the rest of the system as cancer of the lip does; and even to a late period the disease differs in appearance from cancer, as it is usually seen in the skin, not having the peculiar fœtor and watery discharge, and not bleeding and sloughing, with the ragged foul hollow centre, observed in that form of disease.

[There were two casts on the table, illustrating this difference; one of a large prominent tumor on the nose and cheek, in a florid, healthy countenance; the other of a foul, ragged, everted ulcer, opening into the nostrils, with the characteristic

appearance of a cancerous disease, both in the part and in the countenance of the patient, from whom the cast had been taken; although both patients died of their respective forms of complaint not long after the casts were taken, when attending the hospital.]

Possessing, then, this low degree of malignancy, you may remove the tumor, with a reasonable prospect that it will not return, provided no portion of the tumor be left behind: it is generally confined to the skin, and scarcely projects below it; and the skin around the tumor has no root extending into it, as the parts around a cancerous tumor of the lip, and still more of the breast, so often have. The patient is not quite safe, therefore, but nearly so. The old man from whom Mr. Babington removed this tumor remains, I believe, free from the disease, though some apprehension was at one time entertained of a return in the wound. Why, then, has the disease returned in Standfast?

This man was admitted, at the age of seventy-nine, July 10th of last year, with a small tumor on the right cheek, just over the malar bone. The notes tell us that it was about an inch in diameter, and projected from the skin, the surface being larger than the basis. It was ulcerated over its whole extent, the granulations being small and soft, and discharging a thin pus. It was surrounded by a reddened skin; and though the ulcer was moveable upon the malar bone, it seemed that some condensation of the cellular texture had taken place below it. It was sometimes painful, but not acutely so. His health, considering his age, was pretty good. It began like a wart, about eight months ago, and has been ulcerated about three months.

July 18th.—I removed the tumor, with some of the surrounding skin, and a good deal of fat below it. The wound healed readily, and he was discharged on the 7th of August. In October, however, he returned with the cicatrix perfect, but with a small hard lump attached to the cicatrix, and to the malar bone. Why, then, did this tumor form again?

In this preparation the fat has been removed from the tumor, so that you can see the small round tumor projecting about a quarter of an inch below the line of the skin from which the fungous ulcer had arisen. This is its usual appearance, and had this been all, he would probably have been cured; but besides this, there was a small projection standing perpendicularly from the skin, by the side of the round portion of tumor, not above the diameter of a crow-quill, and of some length; and which, from its situation, must have been cut across about half an inch below the skin, and overlooked at the time of the operation, from the quantity of fat which surrounded it, and covered the main portion of the little tumor. Hence then the unsuccessful result of the case: it has procured you, however, the opportunity of watching the further progress.

It was my wish, when he came back in October, to have again removed the tumor, with the portion of the malar bone to which it was then attached; but he refused the operation. The tumor then went on increasing slowly, and becoming more painful; the skin ulcerated in the centre, and the probe passed in to some depth, and he was re-admitted into the hospital in January of the present year. At this time the tumor was intimately connected with the bone; it was divided into two portions, one of which projected outwards more than the other, and was ulcerated, with a deep hole in the centre, discharging a watery pus. Around this the skin was loose and œdematous, as when he was in the hospital before, but was apparently sound. The outer portion of the tumor raised the skin, but was not connected with it as the ulcerated portion was. The tumor was very painful,—the pain of a lancinating kind, shooting through the eye, and often affecting the sight. A small gland was felt enlarged under the chin, and two others were felt hard and enlarged at the margin of the sterno-mastoid muscle.

Under these circumstances, then, with the bone affected, and glands enlarged, at the age of 80, the case was not one to press for his consent to another operation. I am not, indeed, even at present, quite certain of the state of these glands, for though they remain hardened, they have not increased at all in size.

March 5th, our notes say the disease has advanced a good deal lately, the ulcer extending further around the opening, and deepening by a central slough under the malar bone, which appears itself to be softened. Towards the eye the tumor has extended under the lid, and there has been a good deal of superficial inflammation of the globe of the eye. The pain is very great in the face and head, and he has much fallen away, having also purulent expectoration from the bronchiæ.

April 1st.—He has been for some time incapable of sitting up from weakness and suffering, the pain being often great, even when the morphia (which he was taking) influences him enough to keep him almost constantly dosing. The ulcer is more healthy, and the tumor around has not extended, so that a large hollow in the centre, with occasional sloughs, occupies a great part of the swelling; and the probe goes deeply into this, as if the malar bone

was softened. Soon after this he rallied again, and the purulent expectoration diminished, and the pain lessened.

May 4th.—The edges are healthy, and even seem disposed to cicatrise. In the centre a large cavity seems to be formed, even in the malar bone, some portions of which strike against the probe, which passes through it as if through the orbitar plate.

July 5th.—The tumor has spread a good deal lately, especially towards the upper part, so that a great part of the lower eyelid is thickened and hard, like scirrhus, and the temple is much swollen. The margin of the ulcer has a very healthy appearance, red and granulating, and discharging a healthy pus,—and this even to the deep cavity in the centre, which is no longer sloughy. The probe passes about four inches into this hollow, without striking against any bone, passing, as it seems, below and to the outside of the globe of the eye, through the parietes of the orbit into nearly the bottom of that cavity, all the intervening bone having been absorbed. The mass of tumor projects perhaps an inch and a half beyond the former line of the cheek, and is about three inches across at its broadest part; it is sometimes very painful, but has no disposition to bleed.

Such, then, has been the progress of this case towards its final termination, becoming more like a cancer of other parts of the body, as it affects the deeper parts than the skin, but even now remarkable for its healthy appearance, and the absence of bleeding and sanious discharge. The poor man is gradually becoming weaker with pain, and diarrhœa, and cough.

3. I now proceed to the subject of *cancer of the lip*.

This disease commences by a small tumor in the substance of the lip, insulated, hard, and attached to the skin, which forms a part of it; or it has sometimes the appearance of a small tubercle confined to the skin, and not projecting into the texture below the skin; and it is generally situated close to the margin of the lip. This becomes chapped and ulcerated after some time, or the surface looks simply excoriated, and frequently scabs over, till at last a florid ulcer is established, with a central slough, and surrounding hardness. You may see the early appearance of the tumor in this plate, which I took from a patient, Jane Nevin, aged 45, who was admitted into the hospital April 10th, 1833. You see a small, circular, projecting tumor, partly on the skin, and partly on the red portion of the margin of the lip. It has a small, deep, excavated ulcer in the centre, discharging a purulent fluid, and readily forming scabs, around which there is a prominent hard margin of the same colour as the skin. I performed the usual operation on the 25th, and she left the hospital cured, I hope permanently, on the 8th of May.

I recollect there was in this case what I have sometimes observed after the operation for cancer of the lip, severe pain for some time afterwards in the cicatrix, described as of a lancinating kind, and similar to that felt before the operation, making the patient and the surgeon also sometimes imagine that the disease is about to return. Not long since a woman was sent to me by a friend and former house-pupil of mine, on account of a similar pain, after he had operated on her in the country. Such pain as this I am alluding to, comes on two or three weeks perhaps after the removal of the cancer; and it is like that often felt after a burn or other ulcer has healed; and it disappears again in a short time with a little strong goulard, or other simple application.

A second stage of the disease was shewn you in William Stauncenott, admitted into the hospital July 2d. You saw in this man an ulcer with hard everted borders, close to the right angle of the mouth, the centre foul and very painful, and discharging copiously; the tumor being altogether rather more than an inch in breadth. Health pretty good. The tumor began about a year ago, and he attributed its origin to smoking; but it only became painful last September, and ulcerated about the same time, since which time it has increased rapidly. Here is the portion of lip, which I removed by the usual operation last Thursday, the 10th, the wound, as you have seen, being so completely united by the first intention, that you scarcely see where some part of the cicatrix is situated, and with so little alteration of appearance, that the portion of lip is hardly missed. Here, again, is a plate, and the preparation, taken from a patient who was operated on at Christmas last, in which you see the cancerous ulcer in a further stage, nearly the whole lip having, in fact, been removed by operation. The patient, however, as perhaps you have seen, returned to the hospital yesterday, with several glands much enlarged and hardened, with, no doubt, a scirhous structure, though the new lip is sound.

You have heard my patient attribute his complaint to the irritation excited by his pipe in smoking; and no doubt this is frequently the immediate cause of the development of the disease. I have been asked, however, by several gentlemen, whether the ulcer in this case might not be one simply of an unhealthy character, and not really cancerous. Now there certainly are very often intractable irritable sores about the lips and mouth, and still more frequently, perhaps, on

the tongue, excited by the irritation of diseased teeth, or other local cause, which bear much resemblance to the sore of cancer; and I beg to refer you to an instructive paper upon this subject in the Medico-Chirurgical Transactions, by Mr. Earle; it is, I think, in the 12th volume. I believe, however, that you can generally distinguish such cases from cancer without much difficulty. You may perceive, perhaps, the bad tooth, or ascertain the habit of smoking, or of chewing tobacco; and on the removal of the cause of irritation, the sore may heal. Inquire into the origin, and you will find it, from the patient's own description, to have been a chap, or excoriation, or pimple; while in cancer, just as in our present patient, you will almost always find on inquiry that there was a lump or tumor for some months, or even a longer time, before its surface ulcerated and became painful. The irritable sore may of course take place at any age; and you can usually discover some derangement of the general health, on which its duration depends, and which you can get rid of by tonics or purgatives, and especially by mercurial alteratives, or arsenic. Cancer of the lip, on the contrary, takes place almost invariably in persons of advanced age, of fifty or sixty; seldom so young as in the patient I first mentioned, where I operated at the age of forty-five; and the patients are almost always remarkable for their florid healthy complexion, and the good health they have previously enjoyed. But further: the different nature of the complaint is at last evinced by the proof of malignancy afforded in cancer, by the enlargement of the glands of the neck, and the morbid poison may be a long time dormant in them, as in the case I alluded to just now; for there, the cicatrix being still perfect, the tumor was doubtless entirely eradicated; yet some months afterwards the glands began for the first time to show marks of the virus, which must have been absorbed previous to the operation.

Cancer of the lip, then, it appears possesses a third degree of malignancy, rendering the return in the parts around the original disease very probable, producing not unfrequently enlargement of the absorbent glands of the neck, and causing also, sometimes, a similar disease in some other part of the body. Our reliance upon an operation is therefore much less than in either of the two preceding diseases which we have considered; our prognosis must also be less favourable than in the apparently more decidedly constitutional disease of chimney-sweepers' cancer. I recollect, for instance, the case of a man, who was operated on by Mr. Keate not long since, who had lost one testis and great part of the scrotum forty years before, and whose father also, who lived to the age of seventy-nine, had three several times had a tumor removed—the first when he was quite young, the last by Sir C. Mac Gregor, when I was house-surgeon at the Lock Hospital, not one of them returning in the same part.

With cancer of the lip, on the other hand, the disease often returns, either in the part or in the glands. Still the chances are such, that the operation is always proper, even when the disease has made very considerable progress, as the part will readily heal; nor is it to be refused even when some gland is enlarged, though of course, in such circumstances, the gland should be removed at the same time with the diseased portion of the lip.

A man was under my care at the hospital, about a year and a half ago, who had had the operation performed on the lip in the country, and a gland, which the man said was enlarged at that time, was unfortunately left. The cicatrix of the lip had remained sound, but there was, when received here, a great mass of cancerous glands on one side of the neck, with a large cavity in the centre, bleeding and sloughing from time to time, so as quickly to carry off the patient. When the glands are thus affected, they generally form a tumor, of almost stony hardness, under the jaw, sometimes forming one continuous ulcerative surface, with a cancerous ulcer of the lip; at other times an ulcer separate from the lip. This may carry off the patient rapidly by hæmorrhage and sloughing, or he may sink under irritation, by slow degrees; even the jaw-bone being involved in the ulcer, during which time the patient suffers dreadfully, and the previous healthy-looking countenance is changed into the peculiar expression of malignant disease; or the tumor may press upon the fauces or windwipe, suffocating or starving the patient. There was a curious case at St. Bartholomew's Hospital, in which the patient was at last very quickly carried off, because the cancerous ulcer unfortunately cicatrized a short time before the patient's death, the contraction of which prevented deglutition almost entirely.

The operation which you saw me perform was the usual one of removing a triangular portion of the lip, and then uniting the cut surfaces by ligatures, which I think much better than the hare-lip pins, as exciting less irritation, and causing no puckering whatever of the lip. In this case I removed two ligatures on the second day, and the others on the fourth, and the lip is, as you can see, quite as good as before. In this way even the whole lower lip, almost from one commissure to the other, may be removed, without deformity, from the readiness with which the chin yields. At the time of his admission, however, I

had some thoughts of performing another operation, invented by Dupuytren, which consists of the removal of a narrow slip of the lower lip, of a semi-lunar form, including the disease in the centre, and ending in a point at each commissure, and then gradually raising the skin of the chin upwards, to supply the place of what is removed. I thought of doing so in this case, because near the other end of the lip was a little excoriation; so that if this also was cancerous, the whole lip must have been removed, if the common operation was chosen; but this abrasion readily healed, and was evidently not cancerous, and I therefore returned to the usual method, because I think it, as a general rule, preferable to the semi-lunar incision; though if the sore be very long and very superficial at the margin of the lip, I should then choose Dupuytren's plan. I prefer the other, because there is hardly any deformity after it, and it almost always heals by the first intention, and because, by the usual triangular portion being taken out, you are, I think, more sure of getting entirely beyond the contaminated part of the lip, and the patient preserves completely the action of the orbicularis muscle, which he must in part lose if the other plan be adopted for the removal of a cancerous ulcer of any depth from the margin; so that although the new lip can in great measure retain the saliva, yet its movements must be to a certain degree awkward and impaired. Your choice of operation should therefore depend upon the circumstances of each particular case.

SUPPOSED EFFICACY OF PERUVIAN BARK AS AN ANTIDOTE TO ARSENIC.

By Henry Perrine, M.D.

I perceive, under the head of American Intelligence, in the May number of your journal, a "note" to yourself, "On the Peruvian Bark as a counter-agent to the Poisonous Effects of Arsenic," which was elicited by the outlines of my own case in your number for November last*. The note itself simply contains the report of an unknown person having successfully treated several cases of poisoning by arsenic with large doses of Peruvian bark and milk, without stating whether they were followed by vomiting; and thinks it, therefore, worthy of inquiry how far the action of the poison may have been checked by the Peruvian bark in my case. Hence, in this note to yourself, I say, that in my opinion the powdered Peruvian bark had in my stomach about the same effect as an equal quantity of pine saw-dust, chesnut charcoal powder, ground Indian corn, or any other comminuted matter with which the undissolved arsenic would as readily mix, to which it would as firmly adhere, and with which it should as soon be carried out of the stomach by vomiting. As *milk* by coagulating *in* the stomach is infinitely preferable for these purposes, I drank it abundantly, but hurried out the coagula, and their enveloped arsenic, as soon as formed, with mucilaginous drinks and blue vitriol—of which seven doses were taken in rapid succession. Having no faith in antidotes, as soon as the vomited fluids exhibited no traces of arsenic, I tried to make the remaining poison travel through my intestines as fast as possible; and as calomel in pills was the only purgative which my stomach would retain, I continued it, aided by numerous clysters, until the discharges from the bowels no longer turned green with a solution of the sulphate of copper, which occurred on the morning of the fourth day. I should nevertheless have probably died, had not the magnanimous mercury, at the most critical period, pervaded my system and equalized its circulation. Nor was that the only dangerous epoch in which I have owed to this noble medicine the continuance of my feeble existence*.

* Med. Gaz. xi. 771.

HEREDITARY RESEMBLANCES.

In the Philosophical Transactions for 1821, are two papers illustrative of this curious subject. Lord Morton being desirous of domesticating the quagga in this country, endeavoured to procure some individuals of the species; but being disappointed in obtaining a female, gave a male to an Arabian chesnut mare: the issue was a female hybrid, which showed her mixed origin both in form and colour. The same Arabian mare was afterwards bred from by a black Arabian horse, and the result was, a filly the first year, and a colt the year after, both of which, though fine specimens in most respects of their Arabian origin, were yet marked with certain stripes and lines characteristic of the quagga; nor were the manes those of the Arabian breed. The second communication on this point is one by Mr. Giles, in which he states, that in the litter of a black and white sow, by a wild boar, the chesnut colour of the boar prevailed; that a second litter from the same sow, by a boar of a very different breed, retained many peculiarities of the former sire; and that even in a third litter, the chesnut colour

* American Journal of the Medical Sciences.

was to a certain extent evident. These are surely striking facts, shewing how hereditary peculiarities not belonging to the female may be transmitted by her, in successive instances, to the offspring of a male who has them not.

STATURE AND WEIGHT OF MAN AT DIFFERENT AGES.

M. Quetelet, of Brussels, has drawn up a table, of which the following are some of the particulars. It may be considered as in some sort an average, or an approximation, for the Caucasian race, in a temperate climate.

	MALES.		FEMALES.	
Age.	Imp.ft.	lbs. Av.	Imp.ft.	lbs. Av.
0	1·64	7·05	1·60	6·41
1	2·29	20·84	2·26	19·38
5	3·23	34·78	3·19	31·67
10	4·18	54·07	4·09	51·87
12	4·54	65·76	4·43	65·76
15	5·07	88·69	4·91	89·03
18	5·43	127·58	5·13	112·54
20	5·51	138·79	5·17	117·50
30	5·52	140·37	5·18	119·82
40	5·52	140·42	5·18	121·80
50	5·49	139·95	5·03	123·85
60	5·37	136·07	4·97	119·75
70	5·32	131·27	4·96	113·60
80	5·29	127·54	4·94	108·88
90	5·29	127·54	4·93	108·81

From this table we learn, 1st. That at an equality of age, the male is generally heavier than the female; but that, towards the age of 12, an individual of either sex has the same weight. 2dly. That the male attains the maximum weight about the age of 40; but after that, he begins to grow lighter, and about 80 he is found to have lost 13·23 lbs. avoird.; his stature being also diminished 2·75 inches. 3dly. That the female attains the maximum weight later than the male—towards the fiftieth year. 4thly. That when the male and female have attained their complete development, they weigh almost exactly twenty times as much as at the moment of birth, while their stature is only about $3\frac{1}{4}$ times what it was at that period.—*Edin. Philos. Journal.*

NEW MEDICAL WORKS.

Some Observations on the Preparation and Medicinal Employment of the Ioduret and Hydriodate of Iron. By Anthony Todd Thomson, M.D. 8vo. 2s. 6d. sewed.

Observations on the Diverse Treatment of Gonorrhœa Virulenta, &c. &c. By J. M. Churchill, F.L.S. 8vo. 1s. 6d. sewed.

In the Press.—A new volume of the Medico-Chirurgical Society's Transactions.

LITERARY INTELLIGENCE.

Mr. John Murray is preparing a fourth edition of a Manual of Experiments, illustrative of Chemical Science, systematically arranged.

WEEKLY ACCOUNT of BURIALS,

From Bills of Mortality, *Aug.* 5, 1834.

Abscess	1	Hooping-Cough	6
Age and Debility	38	Hydrophobia	1
Apoplexy	3	Inflammation	26
Asthma	3	Bowels & Stomach	16
Cancer	2	Brain	1
Childbirth	4	Lungs and Pleura	5
Cholera	33	Insanity	2
Consumption	68	Jaundice	1
Convulsions	50	Locked Jaw	1
Croup	1	Measles	7
Dentition or Teething	12	Mortification	3
Diarrhœa	1	Paralysis	7
Dropsy	12	Rheumatism	1
Dropsy on the Brain	15	Scrofula	1
Erysipelas	1	Small-Pox	7
Fever	7	Spasms	2
Fever, Scarlet	6	Stone and Gravel	1
Fever, Typhus	1	Tumor	1
Gout	1	Unknown Causes	71
Hæmorrhage	1		
Heart, diseased	3	Stillborn	24

Increase of Burials, as compared with the preceding week . . . 11

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

July, 1834.	Thermometer.		Barometer.	
Thursday . 31	from 51 to 74		29·78 to 29·76	
August.				
Friday . . 1	55	77	29·73	29·77
Saturday . 2	56	75	29·82	29·83
Sunday . . 3	50	76	29·80	29·79
Monday . . 4	51	77	29·80	29·82
Tuesday . . 5	50	74	29·84	29·86
Wednesday 6	48	70	29·81	29·78

Wind variable, S.W. prevailing.

The 31st cloudy; rain in the morning; a peal of thunder about half-past one, A.M. The 1st, cloudy; a little rain in the afternoon, accompanied by thunder; very vivid and forked lightning in the evening. The 2d and 3d, generally clear; lightning frequent on the evening of the latter day. Sunshine frequent during the 4th and 5th; a few drops of rain about 8 P.M. on the 5th. The 6th cloudy, with frequent showers after noon.

Some good crops of wheat have been secured.

Rain fallen, ·125 of an inch.

Charles Henry Adams.

NOTICE.

Medicus.—The letter cannot be published unless the name of the writer be attached to it. Medicus himself will be convinced of the propriety of this, when we inform him that the letter to which his is an answer was also sent, in the first instance, anonymously, and refused. We cannot compel one correspondent to expose himself, and suffer his opponent to fight under cover.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, AUGUST 16, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XLV.

COMPLEX LABOURS.

ACCIDENTAL HÆMORRHAGE.

The second variety of hæmorrhage before delivery depends on the partial separation of the placenta from its attachment to the body or fundus of the uterus; and as it is evident that, unless the mass be situated over or near to the os uteri, flooding need not necessarily accompany the dilatation of the orifice, so it is equally plain that the discharge in the case under consideration must be regarded as purely of an accidental nature.

It is probable that before the termination of gestation one or more attacks of hæmorrhage may appear; and that the first may be traced to a blow or fall, sudden or unusual exertion, or violent mental agitation: but it sometimes only shows itself at the beginning of labour, and may then, perhaps, be referred to undue and irregular action of the uterine fibres, at that particular part against which the organ is apposed. We generally observe, also, that when labour is established, the discharge is diminished in quantity, or wholly suspended, while the uterus is contracting; and returns more copiously in the interval of action. In both these respects the suspicious symptoms differ materially from those which would lead us to believe that the placenta was implanted over the os uteri; for I have shown you that as the uterine neck expanded by a gradual growth—the fibres dilating circularly from above—its surface slipped away from its connexion with the placenta; and that this separation was almost always attended by discharges of blood at uncertain intervals during the last few weeks of pregnancy, coming on without any assignable cause: that on the accession of labour also, with each contraction there was usually an increase of the bleeding, and a diminution when the pain declined. The pressure exerted on the ovum by the parietes of the uterus, and the temporary plug consequently created at the open orifices of the uterine vessels, will easily account for the diminution of discharge in the acmè of pain, under the accidental species of hæmorrhage.

Although of a character to excite considerable anxiety, this case is very much inferior in danger to placental presentations, either partial or entire. The means of diagnosis have been already pointed out: it is known by the membranes being discernible, protruding more or less through the os uteri, and the placenta being completely out of the reach of the finger; and it only remains that we should consider the

Treatment.—On this subject there still prevails a diversity of opinion among practical men, though the great majority strongly recommend the adoption of the plan I myself pursue,—an early rupture of the membranous cyst. This simple proceeding I have found subdue the discharge, in the case under consideration, even more completely than when the placenta was partially implanted over the uterine orifice; and, as far as my observation has gone, it has been attended with the happiest results.

Previously to the establishment of labour, indeed, the general treatment before recommended may be enjoined; and it will frequently be found that the discharge is arrested by a rigid adherence to the anti hæmorrhagic system: but when the flooding continues while the os uteri is di-

lating, other means must be had recourse to beyond those of a mere palliative kind; and the evacuation of the liquor amnii, on the one hand, and immediate delivery on the other, have each, even in the present day, their advocates.

Till within the last few years no part of obstetric practice was founded on more uncertain principles than the treatment of hæmorrhages before delivery,—some authorities advising the case to be left to the agency of nature alone, others to puncture the membranes, and others again contending for immediate delivery in every instance,—but all agreeing that when the discharge is profuse, and the patient's life placed in imminent hazard, emptying the uterus artificially offers the only chance of safety. And this discrepancy of opinion and advice evidently arose from the true nature of the different causes of the discharge not being well understood. Since the excellent treatise of Rigby, however, has become so generally known to the profession, not only is the obscurity in which these cases were shrouded removed, but a fixed and determined principle of practice is established for our guidance.

Guillemeau, the celebrated pupil of the more celebrated Parcy, following the suggestions of his preceptor, advised delivery by the feet in all cases of dangerous hæmorrhage; and this method was almost universally adopted till the time of Clement, who insisted on the more simple plan of rupturing the membranes: and to Puzos, the pupil of Clement, the credit is due of first publicly advocating this practice. Still, however, as little or no distinction was drawn between those cases in which the placenta presented first, and hæmorrhages of a purely accidental nature, the practice could not be considered as based on scientific or sure grounds, until Rigby, with the most praiseworthy zeal, directed his observant mind to the subject.

The great advantages resulting from letting off the waters of the ovum have already been noticed, when partial placenta presentations were under discussion. The vessels of the uterus are diminished in size by the contraction of the uterine fibres;—the open orifices are in a degree plugged by the parietes being brought into closer and stronger contact with that portion of the placental mass disunited from the uterine surface; and the pains are usually increased in frequency and power by the augmented stimulus which the os uteri sustains.

The measure, indeed, is sanctioned by the authority of Denman, Baudelocque, Merriman, Blundell, my father, and many other men of acknowledged practical experience. Rigby has reported a great number of cases in which the rupture of the membranes entirely put a stop to the previous discharge,—and he states that he never had occasion to turn the child in any instance where this expedient was resorted to. Merriman mentions that he has adopted the same means in upwards of thirty cases of accidental hæmorrhage; "that as yet he has had no reason to be dissatisfied with the plan, for in every instance the discharge has either entirely ceased, or been so much diminished, as to secure the safety of the patient; and yet there were some among these patients whose cases, from profuse hæmorrhage, were abundantly alarming." In my own practice, out of twenty-five cases of this kind, of very aggravated nature, in twenty-three instances the labour was terminated naturally and safely after the rupture of the membranes, and in two the loss of blood had been so profuse before I saw the patients as to induce me to deliver artificially; in both instances, with a fatal result.

Of partial presentation of the placenta, or its implantation on the neck of the womb, close to the os uteri, within reach of the finger under examination, it has fallen to my lot to see numerous cases. I have the detailed histories of forty-four, all of which I have personally attended. It is singular that of these forty-four, in six the fœtus offered itself at the os uteri with the breech, and in five transversely. In most of these cases, however, labour came on prematurely; but I have remarked also, that under complete placental presentations, a preternatural position of the fœtus is more frequent than ordinary. In forty the membranes were ruptured some time before delivery was proceeded in; in thirteen of these the labour was terminated by the agency of the natural powers alone; in twenty-six, turning was accomplished, and that without much difficulty, the discharge not ceasing on the evacuation of the liquor amnii, but in the great majority being most materially lessened; and one was terminated by the forceps. In three of the transverse cases the operation was undertaken immediately. Eight of these patients died; one from malignant puerperal fever, which was raging at the time; another from an inflammatory attack ten days after delivery; two in whom the placenta was strongly adherent to the cervix uteri, giving much trouble in its separation; and the remainder apparently from the excessive loss of blood suffered previously to delivery being effected: one of the cases in which the child lay transversely was among these latter.

I have said that there exists still a diversity of opinion among practitioners as to the propriety of rupturing the membranes in accidental hæmorrhage; and its utility

is denied by Dewees, Hamilton, Burns, Stewart, and some others. Three great objections have been taken to the practice:—first, that gestation is necessarily suspended by the evacuation of the waters of the ovum;—secondly, that the time is uncertain at which delivery will be perfected after the operation,—during which interval the dangerous symptoms may be much aggravated;—and, thirdly, that as puncturing the membranes will not always suspend the flow of blood, should delivery become requisite, its performance will be rendered extremely difficult, in consequence of the strong contraction of the uterine parietes around the fœtal body. To the first objection the answer is easy and conclusive; for, since we may presume that labour has already commenced, by the dilatation of the uterine mouth, gestation must have ceased before the operation is resorted to. Even should the term of pregnancy be distant,—inasmuch as large losses of blood usually excite uterine action, and we may therefore presume that a premature expulsion of the ovum will ensue,—puncturing the membranes can but hasten the event; it does not originate the disposition. Besides, should the woman's life be endangered by the profuseness of the discharge,—since the probability is that the complete evacuation of the uterine cavity will alone place her in a state of safety,—the preservation of an immature fœtus cannot be put into competition with the chance of recovery afforded to her. The uncertainty of time at which effective uterine action will be established, has been adduced as another serious objection; and this appears to me as untenable as the former: for in my own practice I have usually found the contractions speedily increased, both in frequency and strength, after the measure has been resorted to; and the same observation must be made on a perusal of the cases detailed by Rigby. The third objection, at first sight, would seem the most plausible; but I have already replied to it, by observing, that if the uterus contracts powerfully enough to refuse admittance to the hand, its action will be sufficient to expel the fœtus, or at least so to compress the open vessels as to put a stop to any further flow of blood in an immoderate degree. But if confirmation were required, I might with confidence advert to the experience of Rigby, Merriman, and my own, advanced in the statements which I have just laid before you.

For reasons before given, I consider it my duty strongly to recommend this practice in preference to immediate delivery: for my opinion is perfectly at variance with Professor Burns, who asserts that experience has taught us puncturing the membranes cannot be relied on. On the contrary, we may affirm that experience taught Rigby, Denman, Merriman, and many others, that this easy and gentle expedient could be trusted to in the great majority of instances; and personal observation has long impressed me with the conviction of its high value. Nor am I more disposed to agree with the Professor in his eulogium on the use of the tampon, in cases where rigidity of the os uteri precludes the possibility of immediate delivery, although sanctioned by the authority of Dewees, Capuron, Mad. Bourgeois, and Gardien: because, notwithstanding the blood may be prevented flowing externally, it may still collect in such quantities in utero, as to destroy life. The practitioners, indeed, whose names I have just mentioned, deny the possibility of such an occurrence, while those of our own country (particularly Hunter, Denman, Barlow, and Merriman) look upon the uterus, at the termination of pregnancy, as capable of containing a body much larger than the ovum, and fear an internal accumulation of blood in consequence of its distensibility. Of this fact, indeed, more than one instance has come within my own knowledge. If such be the case, then, the tampon must prove a dangerous application, and should not supersede the rupture of the membranes. It is certainly possible that the plug may be advantageous in cases where the membranes have been broken, where the os uteri continues rigid and undilated, and where any attempt at delivery must be attended with danger to its structure; but such cases, according to my own experience, must be of very rare occurrence.

After the evacuation of the liquor amnii, it may perhaps be admissible to administer the ergot—unless, indeed, the mouth of the womb be preternaturally rigid: stimuli may be required if the patient be much depressed; but opium, for the reasons more than once adduced, I should avoid. Friction, and moderate pressure on the uterine tumor, may have the effect of exciting increased action, and the dilatation of the os uteri may be forwarded by the fingers introduced carefully within it during a pain—a means recommended by Merriman, but one which I have myself seldom found it necessary to employ.

Should the discharge continue to flow outwardly, or should indications of internal bleeding be present—the symptoms, indeed, being those of loss of blood generally, together with a flabby and relaxed state of uterine fibre—delivery must be had recourse to without delay, as offering the only reasonable chance of safety.

On the subject of hæmorrhage before delivery, I would strongly recommend you

to peruse with attention Rigby's Essay, already alluded to, as well as a much more recent treatise, by Mr. Ingleby, of Birmingham; which I look upon as one of the most practically useful productions of the day in our department of medicine; and with most of the observations contained in which I perfectly coincide.

Placenta complicated with shoulder presentation, or small pelvis.—You are already aware, that when the placenta is situated either entirely or partially over the os uteri, the child may present with the breech or transversely. Under an entire placental presentation, such a preternatural position of the fœtus would not influence our practice; because delivery would be required, not in consequence of the mode in which the child lay in utero, but because of the unfortunate misplacement of the placenta itself;—and, indeed, it is more than probable that its position would not be detected until the hand was introduced into the uterine cavity: under either case, extraction must be made by the feet. Should the placenta, however, be but partially occupying the orifice, while the breech is at the brim, the membranes may be ruptured and time allowed for its descent, provided the flooding be restrained. But, on the other hand, if the child lie transversely, it would be better to proceed to the delivery at once—to treat the case, indeed, as a transverse presentation, — proceeding with extraction as slowly as is consistent with the safety of the infant.

A placental presentation may also be complicated with a distorted pelvis; so that, though we may have turned the child and brought down the breech and body, we may be unable to extract the head. Under such circumstances, the cranium must be perforated in the manner before explained. This is a complication which seldom occurs, but it has happened to me to meet with three such cases, and very embarrassing I found them. Much time must be occupied in the delivery, and you would suppose that during it the hæmorrhage would be profuse: such, however, fortunately was not the case in either of the instances I attended. In all, the head was perforated behind the ear; and the delivery was accomplished with less difficulty than I expected.

Summary.—Our first duty, then, in floodings before delivery, consists in ascertaining whether the placenta presents over the os uteri; and if so, whether the orifice be wholly or partially occupied by it. If it be found entirely covering it, we must turn the child as soon as the mouth of the womb is dilated to the size of half a crown, or even before, should the flooding continue violent; if partially, we may rupture the membranes — provided the head present — and hold ourselves in readiness to deliver by turning, expecting that probably the flooding, although it may abate, will not quite cease. If no part of the placenta be discoverable by the finger, we may rupture the membranes as early as possible, and hope by this means to put a stop to the hæmorrhage; but at the same time we must be prepared to turn, in case our expectations are disappointed. We may exhibit the ergot of rye in most cases; give stimuli if they be required; and should the os uteri be rigid and undilated under placental presentation, either entire or partial, or under accidental hæmorrhage after the membranes are broken, we may venture to plug the vagina; at the same time keeping a close watch on our patient, lest internal flooding be going on.

HÆMORRHAGE SUBSEQUENT TO THE RUPTURE OF THE MEMBRANES.

Flooding seldom comes on after the membranes have ruptured before the birth of the head, unless there have been hæmorrhage previously; but if a discharge should appear to such a degree as to call for our interference, delivery must be resorted to —by turning, if the head be above the brim of the pelvis, and the os uteri not thoroughly dilated—by the long forceps, if the head have entered the pelvis sufficiently low to prevent our raising it for the introduction of the hand into the uterus, but not low enough to enable us to feel an ear—and by the short forceps, or the vectis, if one or both ears be distinctly within reach of the finger. One or other of these methods will generally be found adequate to the end; but should there exist a small pelvis, tumors, or preternatural rigidity of the soft parts, we may be obliged to perforate the head.

It sometimes happens, that after the head is born, a considerable time elapses before the uterus again acts to expel the shoulders and body; and during this interval, flooding may come on. In such a case, we may endeavour to stimulate the uterus to increased energy by pressure and friction, and the exhibition of the ergot; and we may expedite the delivery by gentle and careful traction, in the hope that the uterus will, as it were, follow the body of the child during its extraction, separate and throw off the placenta, and eventually close its cavity.

HÆMORRHAGE AFTER THE BIRTH OF THE CHILD.

Hæmorrhage under labour, by far the most frequently occurs after the birth of the child, and previously to the expulsion of the placenta; and the flow is often most sudden, rapid, and profuse. At the very

time, probably, when the husband and friends are congratulating themselves on what they consider the fortunate termination of the case, and when the medical attendant is joining in those congratulations, danger is insidiously hovering around, and death is sometimes rapidly, though secretly, approaching.

Dependent on entire or partial separation of the placenta.—Flooding after the child's birth is dependent on the same general causes as before its expulsion—viz. the separation of the placenta, more or less, from its uterine attachment, and the womb not being capable of contracting its cavity so as to close its vessels. We know that unless the uterine cavity be empty, its perfect contraction is prevented, and consequently the complete closure of the vessels is impeded; and that so long there is a great probability, nay, almost a certainty, of hæmorrhage occurring. If, then, the placenta be partially or wholly retained in the uterus, and a portion of it be separated from its attachment, the vessels must continue open, and we must therefore have more or less of discharge.

Often profuse in quantity.—There is always, as I before mentioned, a certain quantity of blood lost upon the separation of the placenta and its protrusion; usually amounting to a few ounces. It seems to consist of not much more than that portion which was contained within the uterine vessels, and which is squeezed out mechanically, through their open orifices, by the contraction of the uterine fibres; so that little is lost to the system generally. But when the discharge is copious, all the vessels of the body are proportionably emptied; and from the rapidity with which the blood flows, we cannot wonder at the instantaneous depression which sometimes occurs.

I have before given you directions, immediately after the child is separated, and given into the care of an attendant, to place your right hand between the thighs of the patient, upon the abdomen, that you may ascertain the state of the uterus, with regard to the degree of contraction it has taken on itself, and whether or not the placenta has passed from its cavity; and I have mentioned that there are five conditions in which it may be found differing essentially from one another, and each indicating a state of greater or less security.

After having made this external examination, I have also directed you to pass the first finger of the right hand into the vagina, and examine for the placenta before you leave the bed-side of the patient. I have informed you, that we may feel persuaded the placenta is in the uterine cavity, if that organ be found large externally; but we become positively certain, if, on running the finger along the funis umbilicalis up to the pelvic brim, we cannot detect the mass; because, if it be lodging in the vagina, it would be within our easy reach.

I trust it dwells in your remembrance, that I cautioned you most strongly against any attempt to remove the placenta from the cavity of the uterus by traction at the funis umbilicalis,—considering such attempts dangerous, and therefore highly to be deprecated, unless the insertion of the cord be most easily discoverable, and unless the principal bulk of the mass can be perfectly surrounded by the finger, introduced as in a common examination.

Propriety of waiting a certain time for the expulsion of the placenta.—With these cautions in our mind, then, presuming the patient free from flooding, we are to wait a certain length of time for the expulsion of the placenta from the uterine cavity: but that time must necessarily have a limit. The management of the placenta, indeed, has at different ages been conducted on the most diametrically opposite principles. From the writings of Hippocrates we gather that it was not the custom to use any means but the most gentle for the purpose of extracting the placenta; but Celsus plainly counsels us to introduce the right hand into the uterus, and remove the secundines, *quoties infans protractus est.* Since, however, the chapter in which these words occur is dedicated to the method to be employed for delivering a dead child, and since they immediately follow his instructions to that effect, we may naturally conclude that this interference was only recommended after a forced delivery had been resorted to, and not in common natural cases; and the word *protractus* seems to favour such an opinion. It has been supposed, indeed, that Celsus counselled this hasty removal of the placenta upon all occasions; and Denman has evidently adopted this view; but I cannot think it is justified by the expression employed. Ætius, who borrows this part of his work from Philumenus, recommends that the placenta, when retained, should be removed by the introduction of the hand; and that if the os uteri be shut, and the operation consequently rendered difficult, relaxing means should be used; that the endeavours, however, should only be persisted in for the first and second day, and if unsuccessful, that the woman must no longer be fatigued; for in a few days the mass will putrify, and come away in a dissolved state. Parey recommended the removal of the placenta immediately the child was born; but at the same time cautioned his readers that it was to be done in the gentlest and softest manner—first, by pulling at the funis;

and if that did not succeed, by the introduction of the hand into the uterus. Parey's advice was but partially followed; the practice inculcated was implicitly adhered to for many years, while the excellent cautions by which it was enveloped were entirely forgotten; and hence the most disastrous effects resulted. The hand was rudely thrust into the uterus on all occasions, and the placenta as rudely torn away. Nor did this mischievous custom receive a check in England till Dr. Hunter determined to oppose it with all his authority; for the instantaneous withdrawal of the placenta was taught by Chapman in 1733, and sanctioned by Manningham in 1739, in the practice at the lying-in ward of St. James's Infirmary, which was the first attempt at the establishment of an hospital for parturient women in this metropolis. In Smellie, also, we find the same system prevailing, though in some degree modified. He directs us to let the woman rest a little after the fatigues of the birth, unless there be danger of hæmorrhage, "that the uterus may, in contracting, have time to squeeze and separate the placenta from its inner surface;" then turning the funis round two fingers, or wrapping it in a cloth, to pull gently from side to side, desiring the woman to assist our endeavours "by straining as if she were at stool, blowing forcibly into her hand, or provoking herself to retch, by thrusting her finger into her throat." If by these methods the placenta cannot be brought away, to introduce the hand and deliver it.

Some years before Hunter commenced practice, Ruysch, whose name is justly rendered famous as an anatomist, particularly by his discovery of wax injections, had been appointed President of the Obstetric College at Amsterdam, and was empowered by the magistrates to regulate the practice of midwifery in that city. Numerous cases having come within his knowledge, illustrating the fatal effects consequent on the barbarous custom of that age, he wrote with much force and ingenuity against it, forbidding the extraction of the placenta in any case; and from his spirited opposition we may date the commencement of the present improved practice. Ruysch certainly trusted in much too great a degree to the unaided efforts of nature, and himself ran into the opposite extreme. Much allowance must, however, be made for the strength of his language, and his universal reliance on nature's powers, since his arguments were intended to uproot a most pernicious and dangerous practice; and they must be regarded, therefore, as those of a partial advocate. Hunter, induced by the same feelings, and having witnessed the same kind of calamities, adopted the system Ruysch was so powerfully advocating; and we are told by Denman, on the authority of Dr. Hunter himself, that after much thought and hesitation, his colleague in the obstetric department of the Middlesex Hospital, Dr. Sandys, and himself agreed to leave the placenta to be expelled entirely by nature, without attempting to render any assistance whatever. In the first instance in which this *experiment* was tried, twenty-four hours elapsed before the placenta passed; but as no ill consequences followed, the trials were repeated; and it soon became the general rule in that establishment to leave the expulsion of the mass to nature's unassisted powers. The occurrence, however, of some fatal cases induced Dr. Hunter to modify his treatment; and it is well known that, before his death, he was in the habit of removing the placenta by the hand if flooding supervened; and I believe he also recommended its withdrawal at the expiration of four hours from the child's birth, if it had not previously passed, although there might be no hæmorrhage.

Thus, then, in the present day we are in the habit of following the most rational and judicious practice which, in my opinion, has ever been adopted. With the fatal consequences attendant on profuse floodings always before our mind, we do not hesitate to remove the placenta by the introduction of the hand into the uterus, as soon as a discharge occurs to such an extent as to bring the patient's life into the least peril: and I think myself warranted also in advising you to abstract it by the same means, provided it is not expelled within a limited period. The time, therefore, that we are to wait before proceeding to withdraw it, becomes a matter for our consideration of deep interest; and I cannot help thinking that four hours will in general be found too long. In my own practice I am generally in the habit of delaying operating for an hour and a half after the child's birth; and I consider it most probable, that if the placenta be not expelled into the vaginal cavity at the expiration of that period, and there be not more than the usual discharge, it will almost always be found extensively adherent to the uterine structure; for if no adhesion exists, we may expect that a portion of the mass will be separated, and hæmorrhage will result. Should this prove the case, then, it is most likely that every hour's delay will increase the strength of the uterine contractions around the placental body, and consequently add to the difficulties which will beset us in our endeavours to remove it.

Still, however, under these circumstances time must not entirely guide us, nor its lapse be our only indication for the

removal. The state of the uterine contractions must not be overlooked. Should the womb be acting powerfully and vigorously, I should be induced to abstract the placenta earlier than the specified period, under the belief that adhesion had taken place, or that irregular contraction in the uterine fibres was the cause of its being retained, and that nature would not be able to surmount the difficulties of the case unaided; while, on the other hand, if the uterus remained inactive and sluggish, I might be inclined to delay longer, provided there was no alarming discharge, in the hope and expectation that the uterus would, after the lapse of a little more time, resume its expulsive action, and terminate the case without manual assistance; and this particularly if the contractions during the birth of the child had been feeble, or the labour lingering. I am pretty well persuaded, that in the majority of those instances where the placenta has been naturally expelled after a retention of many hours, it has been lodging the principal part of the time in the vagina, totally excluded from the uterine cavity; and this I think very likely to have happened in the case related to have occurred under Dr. Hunter's superintendence, because the principle on which it was conducted was that of perfect non-interference. It happened to me once to be called to a patient on the seventh day after delivery of a mature fœtus, in whose uterus the placenta still remained; it was completely putrid, the uterus strongly contracted around it, and in a state of active inflammation. Its removal was perfectly out of the question, for the orifice would not admit the introduction of two fingers. She died the next day.

Cases are on record in which the placenta never passed from the uterus; it having been supposed that the whole or the principal part of it had been absorbed by the action of the uterine vessels;—and some continental practitioners of the present day are strong advocates for ascribing to the uterus the power of absorbing portions of placenta, when left after the child's birth. Knowing the astonishing resources which nature possesses, and the wonderful contrivances she adopts for the purpose of restoring the system to a healthy state, we should scarcely have the temerity to deny the possibility of such an occurrence, even were it not admitted by respectable authority; but we should certainly not expect that the placental mass, or any great proportion of it, would be removed by such means; and we should be acting most unwisely if we left it within the uterus, in any case, with that hope.

In the year 1829 I was requested to visit a young unmarried woman, of respectable connexions, on the sixth day after delivery of a first child, in consequence of the placenta being still retained in utero. I learned from the gentleman who had attended the case, that the labour had been lingering; the child at full time born dead; that the funis had broken from the placenta soon after the birth, and the mass had never come away; but that there had been no hæmorrhage. I found the uterus painful, and considerably larger than it should have been, had the cavity been empty; the discharge from the vagina was scanty, and slightly putrid. The os uteri was almost closed, and I could feel no part of the placenta. She was suffering under a slight degree of fever; but there were no urgent symptoms of immediate danger. Two days afterwards she appeared much in the same state. My friend watched her narrowly for more than a month, during which time a portion of placenta, the size of a wallnut, was expelled. She recovered her health perfectly, and returned to her friends in about six weeks. I am informed that nothing more passed of a solid character; but whether she has ever menstruated I do not know. I shall not enter into any speculations on the case, as to whether absorption may have taken place, or what change may have occurred in the placenta itself; but I place every reliance on the statement, that so long, at any rate, as she remained under my friend's immediate superintendence, the placental mass did not escape from the vagina.

ON THE

PRESENT STATE OF OUR KNOWLEDGE RESPECTING THE PHYSIOLOGY OF THE NERVOUS SYSTEM;

Being a Report presented to the British Association, assembled at Cambridge, June 1833.

By William Charles Henry, M.D.,
Physician to the Manchester Royal Infirmary.

[Concluded from p. 670.]

Besides the evidence obtained by direct experiments on living animals, several important facts have been gathered from the pathology of the nervous system in man. These consist of cases of insulated paralysis of either motion or feeling, referred to the changes in structure observed after death. Sir Charles Bell has himself recorded several examples of this kind, strongly confirming his experimental

results; and others of similar tendency are scattered through the successive volumes of Magendie's Journal. But it must be admitted that evidence of this kind is seldom distinct and conclusive. The structural changes, induced by disease, are rarely so circumscribed in seat and extent, as to represent adequately the operations of the scalpel; and often when they are thus isolated within anatomical bounding lines, they affect, by pressure, or by the spread of the same morbid process, in a degree too slight to leave decided traces, the functions of contiguous parts; thus clouding the judgments of the best pathologists, and invalidating their inferences. There is, however, a very remarkable case described by Professor Royer Collard, to which these objections do not apply.—Sprévale, an invalided soldier, was upwards of seventeen years the subject of medical observation in the Maison de Santé of Charenton. This individual remained for the last seven years of his life with the legs and thighs permanently crossed, and totally incapable of *motion*, though retaining their sensibility. On opening after death the spinal canal, there was found the pultaceous softening (*ramollissement*) of the whole *anterior* part of the medulla, and of almost the whole of the fibrous cords which form it. The *anterior* roots of the spinal nerves had also lost their accustomed consistency; while the posterior surface of the spinal cord, and its investing membrane, were healthy. Several of the cases observed by Sir Charles Bell furnish also unequivocal proof of the soundness of the views developed by experiment.

There exist, indeed, few truths in physiology established on so wide and solid a basis of experimental research and pathological observation, as those deduced by Sir Charles Bell, the original discoverer, and by Magendie, his successor in the path of inquiry, respecting the offices of the spinal medulla. This organ may now be regarded as mainly, if not solely, a medium of intercourse between the external world and the brain, and again between the brain and the voluntary muscles; its two anterior columns being subservient to motion, its two posterior to sensation.

The work of Legallois, entitled, "Expériences sur le Principe de la Vie, notamment sur celui des Mouvemens du Cœur et sur le Siége de ce Principe," was the first remarkable essay on the relations between the heart and the spinal cord. It will, however, be sufficient to allude in general terms to the conclusions of Legallois, since they have been entirely subverted by the subsequent researches of Dr. Wilson Philip and M. Flourens. Legallois's main doctrine was, that the principle which animates each part of the body resides in that part of the spinal medulla whence its nerves have their origin; and that it is also from the spinal cord that the heart derives the principle of its life and its motion. The experimental proof supposed to establish these propositions consisted in destroying in different rabbits portions of the cervical, dorsal, and lumbar medulla. Cessation of the heart's action was affirmed to be the constant result of the operation; but even in some of Legallois's own experiments, the motions of the heart continued after considerable injury had been inflicted on the spinal cord, and especially on its lower divisions. Still more unequivocal is the evidence that has been advanced by Dr. Wilson Philip, in his Inquiry into the Laws of the Vital Functions. His experiments, which were very numerous and judiciously varied, show that the circulation continues long after entire removal of the spinal marrow, and that by artificially maintaining respiration, the motions of the heart may be almost indefinitely prolonged. Flourens, in the tenth volume of the Mém. de l'Académie, has lately confirmed Dr. Philip's views: he has shewn that the circulation is entirely independent of the spinal marrow. The influence apparently exerted is only secondary, being due to the suspension of the respiratory movements. Thus all those portions of the spinal marrow which can be destroyed in the different classes of animals without arresting respiration, may be removed without affecting the circulation. In fishes and frogs the entire spinal cord may be destroyed without checking the heart's motions, because in these classes the medulla oblongata presides exclusively over the respiratory function.

Nerves.—The classification of nerves which is most convenient to the physiologist, is based upon their vital properties or functions. Such an arrangement would distribute them into—1, nerves of motion; 2, nerves both of motion and sensation; 3, the nerves ministering to the senses of sight, smell, and hearing; and 4, the ganglionic system, or, according to Bichat, nerves of organic life. Sir Charles Bell has added a fifth class, comprising nerves which he supposes are dedicated to the respiratory motions; but it will afterwards appear, that the existence of an exclusive system of respiratory nerves is not supported by sufficient evidence.

I. The first class of nerves, exercising the single office of conveying motion, comprehends the third, fourth, sixth, portio dura of the seventh, the ninth, and perhaps two divisions of the eighth, viz. the glosso-pharyngeal and spinal accessory. Mr. H. Mayo's experiments, detailed in his Anatomical and Physiological Commen-

taries, No. 11 (and Journal de Physique, tom. iii.) throw much light on the functions of several of these nerves. The motions of the iris, he shows, require the integrity of the third pair, division of these nerves being always followed by full dilatation of the pupils, which cease to be obedient to the stimulus of light. If the divided end of the nerve communicating with the eye be pinched by the forceps, the iris contracts. Hence it is apparent that diminution of the aperture of the pupil is the result of action, and dilatation of the pupil the result of relaxation, of the iris. Flourens has shown that complete extirpation of the tubercula quadrigemina also paralyses the iris, and that irritation of those bodies excites its contractions. The same effect is noticed by Mayo to arise from division or irritation of the optic nerve. He divided the optic nerves within the cranium of a pigeon immediately after decapitation. When the end of the nerve connected with the ball of the eye was seized in the forceps, no action ensued; but when the end attached to the brain was irritated, the iris immediately contracted. These several experiments clearly indicate the dependence of the iris upon the optic nerve, upon the tubercula from which one root of that nerve springs, and upon the third pair. The stimulus of light impinges upon the retina, is conveyed along the optic nerve through the tubercle to the sensorium, whence the motive impression is propagated to the iris by the third encephalic nerve.

It is not so easy to define the precise mode of action of the pathetici, or fourth pair of nerves. Sir Charles Bell supposes that they are destined " to provide for the insensible and instinctive rolling of the eye-ball, and to associate this motion of the eye-ball with the winking motions of the eye-lids." He even conjectures, that " the influence of the fourth nerve is, on certain occasions, to cause a relaxation of the muscle to which it goes." It is certain, however, from its exclusive distribution to the superior oblique muscle, that the fourth is a nerve of motion. The sixth nerve is also a nerve of voluntary motion, and is sent to the rectus externus of the eye-ball.

Sir Charles Bell has placed the portio dura of the seventh pair among his respiratory nerves. There is, however, no doubt that it is simply a motive nerve, and that it is indeed the only nerve of motion which supplies all the muscles of the face, except those of the lower jaw and palate. Division of this nerve occasions no expression of pain, according to Bell; but Mayo's experience is opposed to this absence of sensibility. " The motion of the nostril of the same side instantly ceased, after its section in an ass, and that side of the face remained at rest and placid during the highest excitement of the other parts of the respiratory organs." These and similar observations are all consistent with the opinion, that the seventh is simply a nerve of voluntary motion. It will afterwards appear that it has no claim to any further endowment.

Mr. Herbert Mayo infers from his experiments, that the three divisions of the eighth pair are all nerves both of motion and sensation. Thus the glosso-pharyngeus is a nerve of motion to the pharynx, and perhaps of sensibility to the tongue. He observed that, " on irritating the glosso-pharyngeal nerve in an animal recently killed, the muscular fibres about the pharynx acted, but not those of the tongue." Irritation of the spinal accessory produced both muscular contractions and pain. The par vagum, he conceives, bestows sensibility on the membrane of the larynx, besides conveying the motive stimulus to its muscles. This nerve has been the subject of experiment from the earliest times, and Legallois has minutely described the results obtained by successive inquirers. These were singularly discordant, and gave origin to the most opposite theories of the mode of action of the par vagum. In the greater number of experiments, section of this nerve was followed, after a longer or shorter interval, by death. Piccolhomini contended that the division of the nerve was fatal, from its arresting the movements of the heart, and after him Willis supported the same doctrine. By Haller, on the contrary, the cause of death was sought in disturbance of the digestive functions. Bichat and Dupuytren seem to have been the first to obtain a glimpse of the true seat of injury. The former remarked that the respiration became very laborious after section of the nerve, and Dupuytren distinctly traced death to asphyxia. Legallois has established by numerous experiments the accuracy of this last view. He has shown that, in very young animals, death is the immediate consequence of the operation of cutting either the par vagum or its recurrent branch, and that the suddenness of the effect is due to the narrowness of the aperture of the glottis in early age. In adult animals, the asphyxia is induced by the effusion of serous fluids, and ropy discoloured mucus, into the bronchial tubes and air-cells. More recently, Dr. Wilson Philip has practised the section of the par vagum with an especial reference to its influence upon digestion. He divided the nerve below the origin of the inferior laryngeal branch, as in this case the dyspnœa is much less considerable than when the wound is inflicted on the higher por-

tion. It was found, in all these trials, that food introduced into the stomach after the operation remained wholly undigested. Hence Dr. Philip infers the dependence of secretion upon nervous influence—a conclusion, it has been remarked by Dr. Alison, not logically deducible from the experimental data.

The par vagum cannot then, it is obvious, be included in the class of nerves subservient solely to motion; and it is even doubtful whether the other two divisions of the eighth pair are not also endowed with sensibility. Respecting the function of the ninth, or lingual, there is, however, no place for hesitation. It has been experimentally proved by Mr. Mayo to supply the muscles of the tongue; though he also asserts that pinching it with the forceps excited pain. Three of these nerves, the third, sixth, and ninth, arise, it was first remarked by Sir Charles Bell, from a tract of medullary matter continuous with the anterior column of the spinal marrow; and hence their exclusive office of conducting motive impressions.

II. There are thirty-two pairs of nerves of similar anatomical origin and composition, which possess the twofold office of communicating motion and sensation. Of these, all excepting one (the fifth pair of the cerebral nerves) spring from the spinal marrow. These thirty-one pairs are precisely analogous in formation, being all constituted of two distinct series of roots, one from the anterior column, and one from the posterior column of the spinal marrow. The posterior funiculi collected together form a ganglion, seated just before this root is joined by the anterior root. It has been already stated, that the power of propagating sensation resides in the posterior column, and in the nervous roots arising from it, and that the motive faculty has its seat in the anterior column and roots. The evidence, also, supplied by Bell and Magendie, that the spinal nerves are hence nerves of double office, has been fully detailed. It remains, then, to establish the title of the fifth pair of cerebral nerves to be included in the same class with the spinal nerves.

The analogy in structure and mode of origin between the fifth pair and the nerves of the spine has been long matter of observation. Prochaska has thus distinctly noticed it in a passage of his essay De Structurâ Nervorum, published in 1779, first pointed out to me by my friend Dr. Holme:—"Quare omnium cerebri nervorum, solum quintum par post ortum suum more nervorum spinalium, ganglion semilunare dictum, facere debet? sub quo peculiaris funiculorum fasciculus ad tertium quinti paris ramum, maxillarem inferiorem dictum, properat, insalutato ganglio similunari, ad similitudinem radicum anteriorum nervorum spinalium?" Söemmering has also pointed out with equal clearness the resemblance in distribution between the smaller root of the fifth, and the anterior roots of the spinal nerves. But Sir Charles Bell was the first to establish the identity of their functions, and to arrange them prominently in the same natural division. His experiment consisted in exposing the fifth pair at its root, in an ass, the moment the animal was killed. "On irritating the nerve, the muscles of the jaw acted, and the jaw was closed with a snap. On dividing the root of the nerve in a living animal, the jaw fell relaxed." In another experiment the superior maxillary branch of the fifth nerve was exposed. "Touching this nerve gave acute pain; . . . . . the side of the lip was observed to hang low, and it was dragged to the other side." Sir Charles Bell concluded that the fifth nerve and its branches are endowed with the attributes of motion and sensation. This, though correct as regards the nerve itself, viewed as a whole, is strictly true only of the lowest of its three *divisions*, viz. the inferior maxillary. The ophthalmic and the superior maxillary, the subject of the last experiment, are nerves simply of sensation. Mr. Herbert Mayo, in the essay already referred to, has pointed out this error, and has defined with minute precision the relative offices of the fifth and seventh nerves. By a careful dissection of the fifth nerve he found that the anterior branch, or smaller root, which goes, as Prochaska was aware, entirely to the inferior maxillary, is distributed exclusively to the circumflexus palati, the pterygoids, and temporal and masseter *muscles*. He observed, that section of the supra and infra-orbitar branches, and of the inferior maxillary, near the foramina, whence they emerge, induces loss of sensation in the corresponding parts of the face. It may then be regarded as fully proved, that the trigeminus, or fifth pair, is the nerve which bestows sensation on the face and its appendages, and motion only on the muscles connected with the lower jaw. The other muscles of the face derive their motive power from the portio dura of the seventh nerve.

M. Magendie has also published several memoirs on the functions of the fifth pair. In these he attempts to prove that the olfactory nerve is not the nerve of smell; that the optic is but partially the nerve of vision; and that the auditory is not the principal nerve of hearing. It is in the fifth pair that he supposes all these distinct and special endowments to reside. But the experimental proof will be found

to be singularly inconclusive. The olfactory nerves were entirely destroyed in a dog. After the operation, it continued sensible to strong odours—as of ammonia, acetic acid, or essential oil of lavender; and the introduction of a probe into the nasal cavity excited the same motions and pain as in an unmutilated dog. The fifth pair was then divided in several young animals, the olfactory being left entire. All signs of the perception of strongly odorous substances—as sneezing, rubbing the nose, or turning away the head—entirely disappeared. From these facts, Magendie infers that the seat of the sensations of smell is in the fifth, and not in the first pair of nerves. It is obvious that Magendie has confounded two modes of sensation, which are essentially distinct in their nature and in their organic seat—viz. the true perceptions of smell, and the common sensibility of the nasal passages. The phenomena, which he observed to cease after the section of the fifth nerve, are the results of simple irritation of the pituitary membrane, and are manifestly wholly unconnected with the sense of smelling, since they are producible by all powerful chemical agents, even though *inodorous*—as, for example, by sulphuric acid. No proof has been given that the true olfactory perceptions do not survive the destruction of the fifth pair. Indeed, in a subsequent paper, Magendie confesses that the loss of sensibility in the nasal membrane, after section of the fifth, does not prove the residence of the sense of smell in the branches of that nerve; but merely that the olfactory nerve requires, for its perfect action, the co-operation of the fifth pair, and that it possesses only a special sensibility to odorous particles.

There is even less ground for supposing that the fifth pair is in any degree subservient to the senses of sight and hearing. After cutting this nerve on one side, the flame of a torch was suddenly brought near the eye, without inducing contraction of the pupil; but the direct light of the sun caused the animal to close its eyelids. Thus the sensibility of the retina, though somewhat impaired, was not destroyed by division of the fifth pair. But section of the optic nerves was immediately followed by total blindness. In another rabbit, Magendie divided the fifth pair on one side, and the optic nerve on the other. The animal, he states, was completely deprived of sight, though the eye, in which the fifth pair only had been cut, remained susceptible to the action of the solar rays. No evidence, however, is offered to shew that the animal was entirely blind: on the contrary, the only change observed, on approaching a torch to an *uninjured* eye, was contraction of the iris; and this, we are told, was actually observed in the eye of the side on which the fifth nerve had been divided.

Magendie has assigned another singular function to the fifth pair—viz. to preside over the nutrition of the eye. Twenty-four hours after section of this nerve, incipient opacity of the cornea was observed, which gradually increased till the cornea became as white as alabaster. There was also great vascularity of the conjunctiva extending to the iris, with secretion of pus, and formation of false membranes in the anterior chamber. About the eighth day, the cornea began to detach itself from the sclerotica, the centre ulcerated, and the humours of the eye finally escaped, leaving only a small tubercle in the orbit. In this experiment, the nerve had been divided in the temporal fossa, but when cut immediately after leaving the pons Varolii, the morbid changes were less marked, the movements of the globe of the eye were preserved, the inflammation was limited to the superior part of the eye, and the opacity occupied only a small segment of the circumference of the cornea. After division of the nerve near its origin in the medulla, no traces of disease were discoverable in the eye till the seventh day, and these symptoms never became very prominent. Several cases have been since recorded of structural disease of this nerve in the human subject, with the concomitant symptoms. That of Lainé, described by Serres, in the 4th vol. of Magendie's *Journal*, furnishes strong support to the views of Magendie.

A different explanation of this fact, and of others, which have a tendency to refer secretion and nutrition to the control of the nervous system, has been proposed by Dr. Alison. Mucous surfaces are protected from the contact of air and foreign bodies by a copious secretion, which is evidently regulated in amount by their sensibility, since it is increased by any unusual irritation. This is especially true of the membrane of the eye. Now section of the fifth pair is known to paralyse the sensibility of that organ, and the contact of air or other irritating body upon the *insensible* membrane, instead of inducing an augmented mucous discharge, will excite the inflammatory process described by Magendie. The disorder of the digestive function, which followed division of the par vagum in the experiments of Dr. Wilson Philip, and the ulceration of the coats of the bladder after injury of the lower part of the spinal marrow, are attributed, by Dr. Alison, to the same cause.

The class of nerves which comprehends the fifth pair and the thirty-one pairs of spinal nerves, becomes, after the union of their roots, invested with a two-fold endowment, and continues so throughout

their entire course and final distribution to the muscular tissue. It would appear, indeed, from a later paper of Sir Charles Bell, that nerves of sensation, as well as of motion, are necessary to the perfect action of the voluntary muscles. "Between the brain and the muscles there is a circle of nerves; one nerve conveys the influence from the brain to the muscle; another gives the sense of the condition of the muscle to the brain." In the case of the spinal nerves, this circle of intercourse is at least probable; but proof of its necessity must be obtained from observing the habitudes of those encephalic nerves which minister exclusively to motion. Now it is found, on minute dissection, that the muscles of the eye-ball, which are supplied by the third, fourth, and sixth motive nerves, also receive sensitive filaments from the ophthalmic branch of the fifth; and that the muscles of the face, to which the portio dura is distributed, are also furnished with branches of sensation from the fifth. Sir Charles Bell has further shewn that the muscles of the lower jaw, to which the motive impression is propagated by the muscular branch of the inferior maxillary, draw nervous supplies also from the ganglionic or sensitive branch of that division of the fifth pair. This complicated provision has its origin, he supposes, in its being "necessary to the governance of the muscular frame that there should be consciousness of the state or degree of action of the muscles."

III. The olfactory, auditory, and optic nerves, are gifted with a special sensibility to the objects of the external senses, to which they respectively minister. Magendie seems to have been the first to prove, experimentally, that they do not also share the common or tactile sensibility. He exposed the olfactory nerves, and found, that like the hemispheres of the brain, from whence they spring, they are insensible to pressure, pricking, or even laceration. Strong ammonia was dropped upon them without eliciting any signs of feeling. The optic nerve, and its expansion on the retina, participate with the olfactory in this insensibility to stimulants. This was proved by Magendie, in the human subject as well as in animals. In performing the operation of depressing the opaque lens, he repeatedly touched the retina, in two different individuals, without awakening the slightest sensation. The portio mollis, or acoustic nerve, was also touched, pressed, and even torn, without causing pain.

IV. The functions of the ganglia of the great sympathetic nerve, and its intricate plexuses and anastomotic connexions, are matter, at present, of conjecture. Dr. Johnstone, in an Essay on the Use of the Ganglions, published in 1771, has described a few inconclusive experiments on the cardiac nerves. He supposes that "ganglions are the instruments by which the motions of the heart and intestines are rendered uniformly involuntary;"—a notion which Sir Charles Bell has shewn to be totally unsound. The best history of opinions, to which indeed our knowledge reduces itself, will be found in the physiological section of Lobstein's work, *De Nervi Sympathetici Fabrica, Usu, et Morbis.*

In the earliest of his communications to the Royal Society, as well as in his last work on the Nervous System, Sir Charles Bell has maintained the existence of a separate class of nerves, subservient to the regular and the associated actions of respiration. The origins of these nerves "are in a line or series, and from a distinct column of the spinal marrow. Behind the corpus olivare, and anterior to that process, which descends from the cerebellum, called sometimes the corpus restiforme, a convex strip of medullary matter may be observed. From this tract of medullary matter, on the side of the medulla oblongata, arise, in succession from above downwards, the portio dura of the seventh nerve, the glosso-pharyngeus nerve, the nerve of the par vagum, the nervus ad par vagum accessorius, and, as I imagine, the phrenic and the external respiratory nerves." The fourth pair is also received into the same class.

This doctrine of an exclusive system of respiratory nerves, associated in function by virtue of an anatomical relation of their roots, has not, as Sir Charles Bell seems himself aware, received the concurrence of many intelligent physiologists of this country or of the Continent. Mr. Herbert Mayo, in the admirable Essay already referred to, was the first to point out the true relations of the fifth and seventh nerves. He has shewn that the muscles of the face, excepting those already enumerated, which elevate the lower jaw, receive their motive nerves exclusively from the seventh, and consequently that this nerve must govern *all* their motions, voluntary as well as respiratory. But Dr. Alison, in his very elaborate paper "On the Physiological Principle of Sympathy," has cast considerable doubts on the soundness of this part of Sir Charles Bell's arrangement, as respects not only the individual nerves thus classed together, but even the general principle on which the entire system rests. The reasoning of Dr. Alison consists, first, in referring the phenomena of natural and excited respiration to the comprehensive order of sympathetic actions. In these, "the phenomenon observed is, that on an irritation or stimulus being applied to one part of the body, the voluntary muscles of

another, and often distant part, are thrown into action." Now it has been long since fully established by Dr. Whytt, that these associations in function cannot be referred to any connexions, either in *origin* or in course, of the nerves supplying remote organs so sympathizing; and that a *sensation* is the necessary antecedent of the resulting muscular action. Thus it is known that these actions cease in the state of coma—are not excited when the mind is strongly impressed by any other sensation or thought—and that the same muscular contractions may be induced by the irritation of different parts of the body, provided the same sensation be excited. Dr. Alison has, however, failed to shew that the essential acts of inspiration—viz. the contractions of the diaphragm and intercostals—require the intervention of a sensation. Their continuance in the state of coma, and in the experiments of Legallois and Flourens after the entire removal of the brain, and their distinct reference by these two inquirers to the medulla oblongata, which has never been supposed to be the seat of sensation, prove them to be independent of the will and of perception. But this is true only of the essential, not of the associated respiratory phenomena.

Dr. Alison proceeds to shew that there is equal reason for classing almost all the nerves of the brain, and many more of the spinal nerves, with those exclusively named respiratory by Sir Charles Bell. Thus the lingual nerve governs an infinite number of motions strictly associated with respiration: the inferior maxillary "moves the muscles of the lower jaw in the action of sucking,—an action clearly instinctive when first performed by the infant, frequently repeated voluntarily during life, and always in connexion with the act of respiration." Again, the sensitive branches of the fifth pair co-operate in the act of sneezing. But if these nerves be admitted into the system, the fundamental principle of that system—viz. origin in a line or series—is at once violated. Nor is this connexion in origin more than matter of conjecture, as regards two of the most important of the nerves, classed by Sir Charles Bell himself as respiratory—the phrenic and the external respiratory. These two nerves branch from the cervical or regular double-rooted series. Moreover, the circumstance of rising in linear succession is not found to associate nerves in function. "Between the roots of the phrenic nerve and those of the intercostals, there intervene in the same series the origins of the three lowest cervical nerves, and the first dorsal, which go chiefly to the axillary plexus and to the arm, and which are not respiratory nerves."

In recapitulation, the following facts are among the most important that have been fully ascertained in the physiology of the nervous system.

1. One universal type has been followed in the formation of the nervous system in vertebrated animals. The brain of the human fœtus is gradually evolved in the successive months of uterine existence; and these stages of progressive development strictly correspond with permanent states of the adult brain at inferior degrees of the animal scale.

2. These successive increments of cerebral matter are found to be accompanied by parallel advances in the manifestation of the higher instincts and of the mental faculties.

3. That the brain is the material organ of all intellectual states and operations, is proved by observation on comparative development, as well as by experiments on living animals, and by the study of human pathology. But there does not exist any conclusive evidence for referring separate faculties, or moral affections, to distinct portions of brain.

4. Certain irregular movements are produced by injuries of the corpora striata, thalami optici, crura cerebelli, and semicircular canals of the internal ear.

5. The tubercula quadrigemina preside over the motions of the iris, and their integrity seems essential even to the functions of the retina. They are also, according to Flourens, the points at which irritation first begins to excite pain and muscular contractions.

6. The cerebellum appears to exercise some degree of control over the instruments of locomotion; but the precise nature and amount of this influence cannot be distinctly defined.

7. The cerebrum, cerebellum, and medulla oblongata, possess the faculty of acting primordially, or spontaneously, without requiring foreign excitation. The spinal cord and the nerves are not endowed with spontaneity of action, and are therefore termed subordinate parts.

8. The medulla oblongata exercises the office of originating and regulating the motions essential to the act of respiration. By virtue of its continuity with the spinal marrow, it also participates in the functions of that division of nervous matter.

9. The function of the spinal cord is simply that of a *conductor* of motive impulses, from the brain to the nerves supplying the muscles, and of sensitive impressions from the surface of the body to the sensorium commune. These two vital offices reside in distinct portions of the spinal medulla,—the propagation of motion in its anterior columns, the transmission of sensations in its posterior columns. There is no necessary dependence of the

motions of the heart, and the other involuntary muscles, on the spinal marrow.

10. The nerves are comprehended in the four following classes:—I. Nerves simply of motion; II. Of motion and sensation; III. Of three of the senses; IV. The ganglionic system.

I. The nerves of motion are the third, fourth, sixth, portio dura of the seventh, and the ninth. It is not ascertained whether the glosso-pharyngeal and spinal accessory nerves belong to this or to the second class.

II. The function of ministering both to motion and sensation is possessed by the fifth pair of cerebral nerves, and by the spinal nerves, which agree precisely in anatomical composition. The par vagum, however, which is one of the irregular nerves, has also a twofold endowment.

III. This division comprises the first and second pairs, and the portio mollis of the seventh pair. These nerves are insensible to ordinary stimulants, and possess an exclusive sensibility to their respective objects—viz. odorous matter, light, and aërial undulations.

IV. The system of the great sympathetic nerve, and its associated plexuses and ganglia *.

RE-UNION OF FRACTURE OF THE CERVIX FEMORIS WITHIN THE CAPSULE.

To the Editor of the Medical Gazette.

Sir,

As the question of union or non-union of fractures of the cervix femoris, within the capsule, has long been mooted, and seems hardly yet to have been satisfactorily determined, perhaps the annexed case may not be uninteresting to your readers.

In the controversy it is not my object to engage, nor do I think it becoming to place my opinion in opposition to that of others, who, on this particular subject, have enjoyed a more ample field of observation. The narrative is, I hope, penned without bias; and such as it is, I leave it at your disposal.

I am, sir,
Your obedient servant,
Thomas Fawdington.

Manchester, July 31, 1834.

Anne Whitehead, æt. 68, of active habits and good constitution, was thrown down by a bale of goods which fell from a waggon, in June 1828. She could not rise without assistance, and being unable to walk, was immediately conveyed home in a coach. When I first saw her, some hours after the accident, she complained of great pain in the left hip, increased by moving the limb, which was shortened from an inch and a half to two inches; the foot at the same time being everted. The length and position of the limb could easily be restored by extending, and an obscure crepitus perceived by now rotating it; but the moment the extension was relaxed, the limb was drawn into its previous position. Little swelling existed at this time. As, from the evidence stated, it appeared to me there was fracture of the neck of the thigh-bone, and believing that Hagedorn's splint was better calculated than any other to maintain the fractured extremities in apposition, and thus afford a chance of reunion, if such were feasible, I at once adopted his plan, with a slight modification. The patient, however, could not be persuaded to submit to the restraint beyond a fortnight; she complained of the position as extremely irksome, and suffered pain from the pelvic bandage. The extremity also was swollen and œdematous. For these reasons, the semiflexed posture was substituted, by placing the limb on the double-inclined plane; but of this, too, she soon became impatient. The swelling and œdema had now disappeared. Thus driven from such as I was disposed to regard the most efficient resources, the injury was abandoned to the palliative measure of Sir Astley Cooper. The woman continued this for nearly two months, when she began to walk with crutches, afterwards with a stick only; and at the expiration of about nine months from the period of the accident, she was able to move along with a tolerably firm, though irregular step, without any artificial aid whatever.

In May, 1833, the subject of this case died from bronchitis, which afforded us an opportunity of examining the seat of the injury, of which the following contains a brief description.

When the body was laid on the table, the knee and foot on the left side were everted, and the limb measured, in length, not quite an inch and a half

* Report of the Third Meeting of the British Association for the Advancement of Science; held at Cambridge in 1833.

less than the right. On reflecting the muscles, the capsular ligament of the hip-joint appeared thickened and irregular on its external surface, and the trochanter major situated nearer to the dorsum ilii than usual: with these exceptions, nothing remarkable was detected until the capsule was opened. The neck of the femur, which seemed buried in the acetabulum, was evidently roughened and projecting forward; and here the reflected portion of the synovial membrane was adherent, but so slightly that with the handle of the scalpel the adhesions were easily removed. The bone was now taken from its socket, and brought away, along with its fellow, for more accurate inspection; it presented the following appearances:— anteriorly, the prominent part of the cervix projected considerably beyond the level of the base of the trochanter major, in consequence of a deposition of new bone, which extended from the latter point to within less than a quarter of an inch of the circumference of the head, yet strictly within the capsular ligament; and here the synovial membrane was opaque, thicker than natural, and flocculent on its surface, from the separation of the adhesions before-mentioned. Posteriorly, the cervix was greatly hollowed, though tolerably equal and smooth, and the depression in this aspect was continuous with the trochanteric fossa. The head of the bone, which had assumed a peculiar obliquity, overlapped, in an unnatural degree, the neck, so as to contribute to the hollow referred to; and its articulatory margin was sharp and abrupt from ossific deposition, which extended forward into the mass of new bone situated on the forepart of the cervix.

The upper part of the head was sunk to the level of the trochanter major, and its lower part was in close approximation with the trochanter minor; while the axis of the head and neck had changed its relation to that of the shaft of the femur in a remarkable manner: instead of having these, respectively, nearly on the same plane, the cervix and head, in relation to the trochanter, formed a line inclining considerably backward; so that they appeared *twisted* from their natural direction. Such, indeed, was the obliquity thus produced, that while the space existing between the articular edge of the head and the base of the trochanter major *behind*, measured barely three-eighths of an inch, *before* it extended fully to one inch and three-quarters. After due maceration, a section was carefully made, which disclosed a line of union corresponding with that of the exterior, thick and dense towards the circumference, and tapering to the centre, where the cancellated structure had become again somewhat developed. The whole of this part, however, was firmer in texture than the adjacent, as well as the interior of the cervix from the opposite side, which had been removed for the sake of comparison. The right femur presented, externally, a perfectly natural appearance *in all its relations*. When prepared and divided in a similar way, the contrast with its fellow was remarkable; the cancelli were free and open, and the external compact texture comparatively unsubstantial and delicate.

My belief that the foregoing case represents an instance of re-union of fracture of the cervix femoris within the capsule, rests upon the following grounds:—

1st. The mode and immediate results of the accident, together with the progress of the case.

2ndly. The state of the capsular ligament and synovial membrane.

3dly. The shortening of the neck of the bone, the irregularity of its surface, the change of relation of its axis to the shaft, and the depression of its head.

4thly. The similitude of appearance which is apparent in the section, with what exists in other bones after repair of fracture, of even shorter duration; of which a careful comparison has been made of various specimens in my collection.

5thly. The contrast which it presents to the opposite femur; which would seem to indicate that no *general* agency, especially the effects of old age, had been in operation to produce the change described.

Thus I leave the case, and shall be most happy to shew the specimen to any of my professional brethren who may feel an interest in examining it.

MALFORMATION OF THE GREAT ARTERIES OF THE HEART.

DESCRIBED BY F. TIEDEMANN,

In the Zeitschrift für Physiologie, B. IV. H. ii. S. 287.—*Translated and Abridged*

BY DR. ARROWSMITH,

Of Coventry.

THE malformation of the great vessels of the heart about to be described, occurred in a male child which was born January 27, 1831, in the Lying-in Hospital at Stuttgart, and died nine days afterwards.

The heart in its external conformation, in the number, form, and arrangement of its cavities, presented no deviation from the natural structure; it was larger, however, than is usual in children of that age. The superior and inferior vena cava opened into the right auricle, as did also the coronary vein. The left auricle received the four pulmonary veins. From the arterial part of the heart arose a single very large trunk, which represented both the aorta and pulmonary artery. After giving off the coronary arteries, there arose from the right side of this great trunk a large artery (1), which made a slight curve. From this artery arose the left carotid (2), the right carotid (3), and the right subclavian (4). From the posterior part of the great arterial trunk the left (5) and right (6) pulmonary arteries originated, which were subdivided and distributed in the usual manner. A little above, the main trunk became very much contracted (7), but immediately after resumed nearly its former calibre, and represented the descending aorta (8). From the arch of the aorta the left subclavian artery (9) arose.

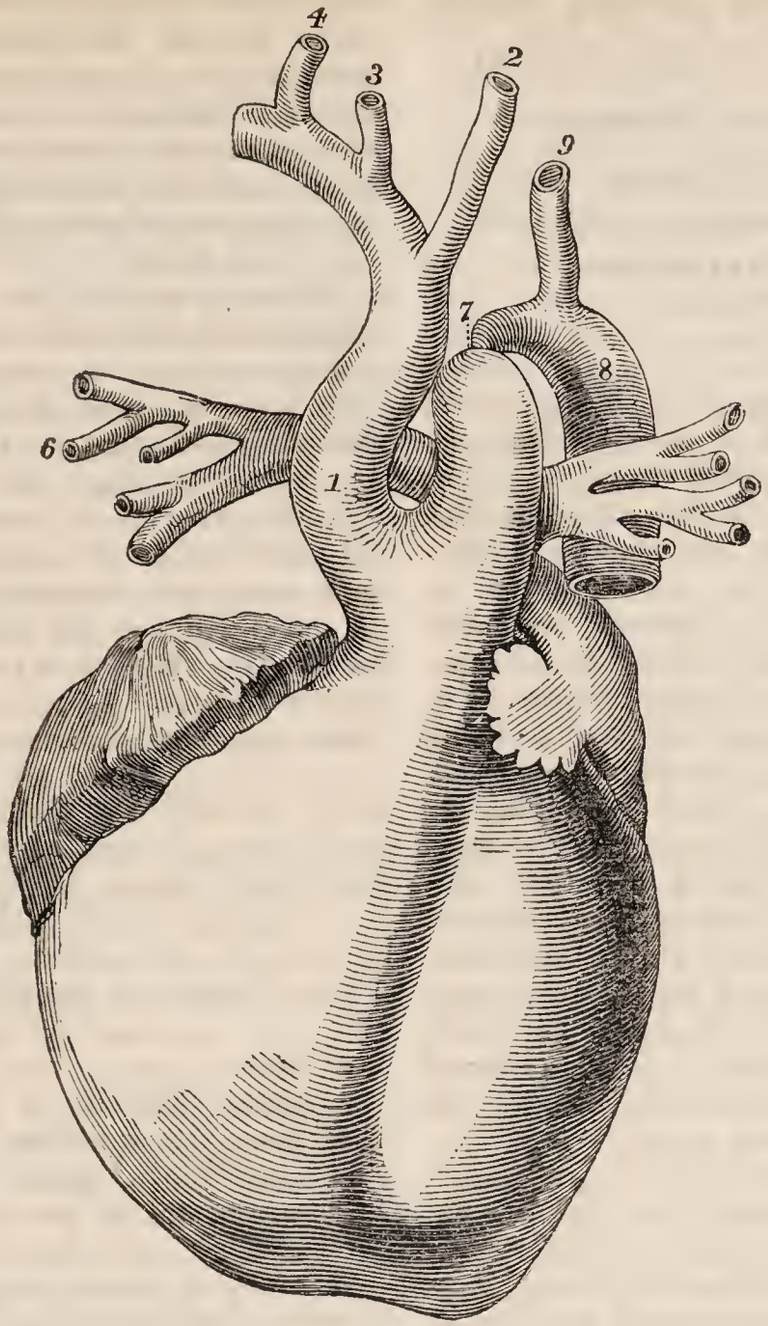
On examining the internal structure of the heart, the foramen ovale was found pervious; the right auricle communicated with the right ventricle, and the left auricle with the left ventricle. The ventricular septum was perforated at its upper part, so as to allow a communication between these two cavities. The great arterial trunk took its origin chiefly from the right ventricle, but there was a small communication of its orifice with the left ventricle also. Thus the two ventricles emptied themselves into the single arterial trunk, where the arterial and venous blood became mixed. Tiedemann thought it probable, from this circumstance, that the child had experienced the symptoms of the morbus cœruleus; he therefore made inquiries in Stuttgart concerning the symptoms during life, and the manner of its death.

From the diary of the hospital it appeared that the mother of the child was a large woman, of strong constitution, twenty-eight years of age, and had had children previously. This child was born after an easy labour; it weighed at birth 6 lbs. 6 oz., and measured 17½ Rhenish inches in length. It appeared well nourished, but wasted to the time of its death, on the ninth day, when the body weighed 6 lbs. It took the breast feebly, was generally still and quiet, but now and then uttered a shrill cry. The warmth of the body was very sensibly less than that of other children of the same age; the breathing was always short, but there was never any blueness of the surface. The evacuations were natural; but two days before death an enema was administered, on account of costiveness. For the last two days of its life there were convulsive motions of the eyes, and some white froth about the mouth; and two hours before death there were convulsive motions of the arms.

A careful *post-mortem* examination discovered a strong sanguineous congestion of the membranes of the brain and of the plexus choroides. A thick and firm layer of lymph, partly bloody, covered the spinal cord; and the substance of the latter was softened. The cavity of the pericardium and of the left thorax contained much clear serum.

"It is clear," says Tiedemann, "from the above statement, that the death of the child was not owing to the malformation, but to the inflammatory state of the spinal cord, &c.; and that, if the symptoms of cyanosis, only a low degree of animal heat, and muscular weakness, were present, the blue colour was wanting."

Note by the Translator.—Contrary to the custom of Tiedemann, and of the German writers in general, there are no comparisons made, in the original paper, of this case with other recorded varieties of malformations of the heart and great vessels. I have had the curiosity to



look into the authorities on the subject, so far as they are accessible to me, and have reason to believe that the present specimen of malformation is unique, not having been able to meet with any example of origin of the pulmonary artery from the aorta, excepting in the case of single heart. In the present instance, the heart, on the contrary, was double; and, with the exception of the previous state of the foramen ovale (which is by no means uncommon), and the perforation in the upper part of the septum ventriculorum, it was perfect in its structure. The case belongs to that variety of malformation which is referrible to Meckel's law of an "arrest of development," the principle of which has been made known to the English reader in the elaborate dissertation of Dr. Paget*. It appears that at an early period of fœtal development, the bulb of the aorta is the only arterial vessel continuous with the heart; and that at a somewhat later period, the bulb divides itself, so as to form the aorta and pulmonary artery. This division takes place about the time of the formation of the septum of the ventricles, for the two ventricles are originally a single cavity: hence it is easy to see, in the event of an "arrest of development," the natural connexion between deficiency of the septum ventriculorum and malposition or unity of the great arterial trunk.

After having noticed the anatomical imperfections of the heart in the case above described, Tiedemann concluded that the child must have suffered from the morbus cœruleus. A comparison, however, of the malformation in this case, with the conclusions obtained by Farre, Louis, and others, would have

* Edinb. Med. and Surg. Journ. xxxvi. 304.

rendered it more than probable that the child had not exhibited any blueness of of the surface; nor should we have anticipated much, if any, diminution of animal heat. The circulation through the pulmonary arteries and lungs was perfect; hence the usual quantity of blood was aërated: and though, from the structure of the heart, the venous and arterial blood must have commingled, it is only when the malformation admits of the great predominance of the venous blood that the morbus cœruleus appears. It would seem from this case, that where the venous blood circulates through the systemic arterial tubes, in such proportion as not to impart the blue tint to the surface, it may, nevertheless, prevent the maintenance of the due degree of animal heat. This is, however, a rather novel conclusion, and reposes exclusively on the observations made in the present case; for I have not elsewhere met with any example of malformation, accompanied with a deficiency of animal heat, without the coincidence of the blue tint of the skin. In measuring animal heat, the sense of touch is deceptive; thermometrical observations are alone to be relied on; and in the case described by Tiedemann the thermometer was not used.

In support of the opinions now stated, I shall quote a passage or two from Dr. Farre's "Pathological Researches." "If the full proportion of blood," says he, "be circulated through the lungs, although the red blood be subsequently mixed with an equal proportion of black blood in the heart, yet a black colour will not be imparted to the skin." p. 31. "And it would appear that there is an over proportion of black blood which gives the *first sign*—a permanent blue colour, without manifesting, in the infant state, the *second sign*—a coldness of the skin. Although this observation needs farther proof, it is certain that a still greater excess in the proportion of black blood affords both signs in a striking degree." p. 32.

The most singular fact, perhaps, in the present case, is the absence of the ductus arteriosus, or of any provision to divert the current of blood from the lungs during intra-uterine life. We must conclude that the same quantity of blood circulated through the lungs before birth as after. In any similar case, it would be desirable to know the condition of the bronchial arteries; for in the present instance, the blood which served for the nutrition of the lungs through the bronchial arteries, must have permeated their structure much more copiously by the pulmonary ones. Such a circumstance could scarcely be unproductive of some peculiar result. Dr. Paget describes it to be the opinion of Kilian, that the use of the ductus arteriosus Botalli is to carry on the circulation of the blood to the lower extremities during fœtal life; and he thinks this view is strongly supported by some varieties of malformation. The present case, however, shews that the nutrition of the lower parts of the body may be adequately performed by the aorta descendens, without the aid of the ductus arteriosus, or of any analogous provision.

Coventry, Aug. 4, 1834.

FUNCTIONS OF THE FIFTH PAIR, AND THE PORTIO DURA.

To the Editor of the Medical Gazette.

Sir,

Circumstances of a private nature have prevented me from noticing at an earlier period the paper of Mr. Alexander Shaw, inserted in your journal of the 24th of May.

It must be quite apparent to any one who has perused that communication, that it is any thing but a reply to the queries propounded by me. Mr. Shaw, however, refers me to a principle advocated by Sir C. Bell, and which at the present period no one can think of disputing, viz. "that the root or nerve which transmits the mandate of the will *outwards* to the muscles, cannot carry a sensation *inwards* to the sensorium. There must be, for this purpose, two nerves or two roots, each having a distinct connexion with the brain." Now in this principle I see nothing hostile to the views I have taken of the functions of the fifth pair. I find sensation conveyed to the brain through the branches connected with the sentient apparatus, and I perceive the mandate of the will conveyed outwards, to the muscles, by the muscular branches.

With respect to the Gasserian ganglion, I would ask what can be the use of this, or any other ganglion, if it be not that of uniting nerves of opposite

character and function? That the two roots of the fifth are independent of each other in the brain, is pretty obvious; but after they have become united in the Gasserian ganglion, who shall answer for their remaining possessed of distinct and independent properties? Or is it not more probable that a union of function as well as of substance is there effected? It is common to believe that the branches of the inferior maxillary division of the fifth only, are composed of its two roots commingled, and possess the double properties of sensation and motion. To me the distinction is not apparent. I observe the two roots of the fifth commingling their fibres, and forming a ganglion. After this union I observe the combined nerve separating into three divisions, each passing in its own course, each distributed alike to parts endowed with sensation and motion; and yet I am required to believe that one only of these divisions is competent to bestow both these functions, and that the other two are subservient to sensation only!

Mr. Shaw says, "Neither of these gentlemen (Sir C. Bell and Mr. John Shaw) whom he has quoted, ever conceived that the fifth possessed any motor influence unconnected with the organ of mastication." Now mastication is effected by the combined operations of many muscles, and includes not merely those of the jaw, but likewise those of the lips and cheeks; and if the fifth be the nerve of mastication it must then be the motor nerve of the muscles of the lips and cheeks, as well as those of the jaw. How this will accord with Mr. Shaw's account of the superior maxillary branch being only a nerve of sensation, must be left for him to explain. That Sir C. Bell and Mr. John Shaw at one time believed in the motor power of this branch of the fifth, the extracts I have previously quoted from their writings abundantly show.

Mr. Shaw has said a great deal about the fifth being similar to the spinal nerves. This view was originally taken up by Sir C. Bell, and adopted by Mr. John Shaw. The latter gentleman, after stating the characters of the spinal nerves—that "they are all compound nerves, being at the same time muscular nerves, ordering the voluntary motions of the frame, and bestowing sensibility on the surfaces of the body," &c.—proceeds to draw up a summary of the points of agreement between these nerves and the fifth, which he does under the following heads:—

"1. That the head and face, having many parts in every respect similar to the neck, trunk, and limbs, must have corresponding nerves.

"2. That the manner in which the spinal nerves and the fifth arise by double origins, is very similar.

"3. That the two origins of the fifth are united by a ganglion exactly of the same shape and character as those which unite the two origins of the spinal nerves.

"4. That the manner in which the branches of the fifth are distributed, and those of the spinal nerves, is the same.

"And, lastly, that the same kind of connexion exists between the fifth and the sympathetic, as between the latter and the spinal nerves."

He then proceeds to state that the similarity holds good in their morbid affections, and instances hemiplegia. "In this disease, the voluntary power over the limbs, and the sensibility of the side affected, are generally destroyed; but in some cases the voluntary power is lost, and the sensibility continues unimpaired, or *vice versâ*. This variety also occurs on the face," &c. "In experiments on the nerves of the spine and on the fifth, we meet with the same results. If, as in the operation which is now frequently performed on the nerves of the horse's foot, we cut a spinal nerve after the branches are given off to the muscles moving the part, we shall destroy only the sensibility of that part; but, if we cut the nerve nearer to the brain, we shall not only destroy the sensibility, but also the power of motion. The same happens in experiments on the fifth; for if we cut a branch which is principally distributed to the skin of the lips, we shall destroy the sensibility of the part, but impair the power of mastication only in a slight degree: but, if we divide the nerve further back, then we shall not only destroy the sensibility of the skin, as in the first experiment, but also cut off the power by which the jaws are moved. I cut a branch of the fifth upon the face: the sensibility of the corresponding side of the lip was destroyed, but little paralysis ensued, *excepting of certain actions of the orbicularis oris*. I cut the nerve nearer the brain, and at a point previous to its having given off the branches to

the other muscles: then the jaw fell, and *the muscles of that side were powerless**." A more complete train of reasoning than this can hardly be imagined, and borne out equally by facts and experiments. The common motions of the face,—voluntary, manducatory, or whatever other term is most proper,—are here all ascribed to the fifth; while, in his next paper, Mr. John Shaw gives an account of what Sir C. Bell and himself then considered as *superadded nerves*, of which the portio dura is assigned to the face, as the nerve of expression, or of those actions of the face which are in any way connected with the organs of respiration.

In this comparison of the fifth pair with the spinal nerves, we find it stated, that "*the two origins of the fifth are united by a ganglion* exactly of the same shape and character as those which unite the two origins of the spinal nerves." This view of the connexion of the roots of the fifth, it will be seen, is very different from that which Mr. Alexander Shaw has given us. He wishes it to be considered that the two first branches are in no wise connected with the motor root of that nerve, and consequently that they are purely nerves of sensation. Here he is distinctly at issue with Mr. John Shaw; and I would again ask, is it reasonable, nay is it possible, for any one (having no particular theory to maintain) to believe, so intimately connected as the two roots are, where they unite to form the ganglion, that there is no intercommunication of their fibres? Where, then, would be the need of this ganglion? Surely, if the smaller root had been intended to join only with the third division of the nerve, the ganglion would have been placed upon that division, and the supposed purely sensitive divisions would have been unconnected with the ganglion.

The fifth, then, being similar to the spinal nerves, its branches "are *all* compound nerves, being at the same time muscular nerves, ordering the voluntary motions of the frame, and bestowing sensibility on the surfaces of the body." I need not say how entirely I agree with this proposition. Indeed, as I have before stated, I cannot conceive any purpose for which these branches of the fifth can be so profusely distributed to the muscles, if it be not for "ordering the voluntary motions." The experiments alluded to support this view. I have referred so often (and Mr. Alexander Shaw has not attempted an explanation of that phenomenon, although I put it to him as a distinct query) to the experiment of Sir C. Bell, of dividing the superior maxillary branch, where the lip was drawn to the opposite side, that I am unwilling to repeat it, but that I consider it a fact so striking and unanswerable that I cannot refrain from again bringing it forward. The experiments of Magendie on the fifth nerve give similar results; the muscles of the face, the eyelids, and iris, were all paralysed. The phenomena of disease afford further confirmation; since it is common to find, in addition to loss of sensibility in the face and mouth, and loss of taste, a paralytic condition of the muscles of the face. A case of this description was appended to my first paper, in the Gazette of the 8th of March last. Another is quoted in the Edinburgh Medical and Surgical Journal of the current quarter, from the work of the Italian physician, Bellingeri. In both these cases there was distortion of the mouth, the eyelids could not be closed, and there was loss of feeling and of taste; and yet Mr. Alexander Shaw, in his paper of July 19th, says this (speaking of Bellingeri's) was an undoubted case of disease of the portio dura! Here Mr. Shaw has fallen into the very error which he declaims against in M. Bellingeri. Had this case been purely paralysis of the muscles, then Mr. Shaw might, with some plausibility, have set it down for an affection of the portio dura; but in addition to that symptom, we find the following: "The irritation from pungent snuff was less in the right nostril, and there was a scanty flow of tears from the right eye; but when the snuff was drawn into the left nostril, the irritation was more violent, and there was a more profuse discharge of tears; and when the nostrils were irritated by extraneous bodies, sneezing followed if the left was touched, but none if the right. Taste was much impaired and injured on the right side of the tongue, and touch was very much blunted in the integuments of that side of the face*." Now these symptoms,

* Medical and Physical Journal, Dec. 1822.

* Edinburgh Medical and Surgical Journal, July. Page 125.

added to the former, present a very different view of the case; and, in my judgment, prove it to be an undoubted affection of the fifth. In the same journal just quoted from (p. 134), we find a case detailed from the same author, which forms a complete contrast to the one just alluded to. Here there was paralysis of all the muscles of the face on one side, and yet taste and touch were unimpaired. Dissection proved this to be a case of disease of the seventh pair. The distinctive marks, then, between the affections of the two nerves, would seem to be, that in disease of the fifth, taste and touch are impaired, as well as the muscles paralysed; while, in that of the portio dura, there is simply paralysis of the muscles.

In reply to the remarks of Mr. Alexander Shaw, respecting Sir C. Bell's experiments on the branches of the ophthalmic division, I would refer to the words of Mr. John Shaw; where he states, that when he cut a branch of the fifth, which was principally distributed to the skin, sensibility only was impaired; but that when he cut the nerve nearer the brain, the muscles were paralyzed.

A great deal of obscurity has been thrown over this subject, by the attempts of physiologists to explain the paralytic condition of the muscles of the face (after division of the second branch of the fifth), as a result simply of loss of sensation; whilst it is not a little remarkable, they all agree that sensation may be destroyed, and voluntary motion continue. If this view be persevered in, no progress can be made. After cutting the third division of the fifth, we might just as well say sensation was lost, and therefore the muscles of the jaw could not act. Thus Mr. Mayo refers to anæsthesia: "In this disease (he says) the sensation of the extremities is wholly lost, while their muscular power remains;" but after these experiments the muscular power did not remain, the animal being unable to use the lips; and when the experiment was confined to one side, the mouth was twisted to the opposite side.

The twist of the mouth is not apparent, of course, when the nerve of both sides is divided, but the inability to use the lips is sufficiently obvious. On a former occasion I ventured to allude to a case reported in one of Sir C. Bell's lectures, where there was disease of the fifth, in which sensation was destroyed, without loss of muscular motion until some time afterwards. I regret that Mr. Shaw has not grappled with these various facts, since it must always be more satisfactory to have objections answered, and difficulties explained, rather than be compelled to set out on a new course, and to have to answer the objections of others.

The experiments on the portio dura prove that paralysis of the muscles of the face and forehead is also the result of its division, while sensibility remains entire. The branches of the portio dura, and the motor branches of the fifth, are distributed to all these muscles; and it seems that injury or disease of either of these nerves is capable of paralysing them. In what way they act separately,—whether the fifth be the voluntary or manducatory, and the portio dura the involuntary or respiratory nerve of the face, it will, perhaps, be difficult to settle; but I contend that their combined action is essential to the perfect motions of these parts.

It will be recollected that I suggested a somewhat similar hypothesis at the commencement of this controversy: with that I think it is likely to terminate. It was in these words:—"The union of these two nerves over every part of the face is most intimate; they appear to inosculate with each other, so as to form a kind of plexus; and one would be almost tempted to imagine that injury or disease affecting one might produce a corresponding affection of the other, or rather might be attended with impaired function of both, as we find to be the case with the union of the fifth and third, termed the ophthalmic ganglion."

I am, sir,
Your obedient servant,
JOHN WALKER.

Manchester,
August 3, 1834.

A FEW WORDS
ON
MUSCLE, NERVE, AND SENSATION.

Do the lower tribes of Animals feel?

BY DR. DAVID BADHAM,
Radcliffe Travelling Fellow, Oxford, &c. &c.

THE various meanings which men attach to words not yet rigorously defined,

is exceedingly apt to mislead. How often do we find a due attention to this important fact lost sight of in medicine, and the good to be derived from attending to it frustrated by a practical, if not by a formal, denial! With this remark, I would call the particular attention of the reader to the words *muscle* and *nerve*. If you ask three physiologists in succession, what they mean by muscle, they might all answer your query differently. If your physiologist be also a chemist, he will refer you to his table of analysis, and bid you distrust all elementary structure not exactly coinciding with this table, as not belonging to true muscle. To your friend, who has a microscope from Dollond, if you put the same question, some peculiar arrangement of fibres is decisive in his mind on the subject of inquiry; while the anatomist, lecturing on muscular structure, announces that it is only by its aptitude to contract on the application of a stimulus, or by its irritability, that you can know whether a part be muscular or not.

All such descriptions and views of muscle are too limited. Muscle ought to be a broad catholic word, not warped or crippled to meet the preconceived notions of any sect. If we agree in attaching to muscle exclusively the idea of vital movement, then all the views entertained of it hitherto are erroneous; for, not to insist that certain parts of our bodies are motive, but plainly not muscular in the chemical acceptation of the term, what an infinity of animals exist whose parts are so far from yielding the products of muscle, that their semi-solid composition scarcely deserves the more popular name of flesh! "A common fibre," says Professor Grant, in his interesting lectures on Zoology, in the course of publication, "is by no means essential to a muscular system; indeed, we find at last all lineal order lost, and that the flesh in the lowest tribes is a soft, homogeneous, gelatinous, or cellular substance. Thus in the medusa, an animal possessing considerable locomotive powers, and where the parts are sensible to a stimulus, the structure of these parts is found to be cellular, and not fibrous."—"Some polypi," we are also told, by the same authority, "possessing no fibre, either nervous or muscular, that can be detected with a microscope, have motion, and are gifted with tentacula of exquisite sensibility, as proved by their laying hold on live worms let fall against them in the water." It is, moreover, worthy of remark here, that some of the Echinodermæ have a muscular and nervous system simultaneously developed, yet possess only motions similar to those tribes that have none.

It was necessary to make these remarks on muscle, because it is time we should all understand one another. It is desirable to brush away cobwebs before we begin to spin the silk; and should any one here remark that we pay undue importance to the acceptation of a word, just inquire whether the long, drawn battle, respecting the muscularity of the second coat of the arteries, would have been ever engaged in, if men had been as intent upon observing facts as pugnacious in securing imaginary prerogatives to a word.

Nor have physiologists been more accurate or distinct in the functions which they ascribe to nerve than in those which they attribute to muscle. This truth I hope to make apparent, in submitting to the candid reader a brief inquiry respecting the evidence on which sensation in the lower tribes of animals has been supported; which evidence I cannot but esteem as very inconclusive and unsatisfactory; reminding him at the same time, in the words of Seneca, that he who decides a question, of which he has only heard the arguments on one side, is not right, even though his decision should be right—"Æquè tanquam statuerit, haud æquus fuit." The question I would consider is, whether sensation does indeed belong to all animals indifferently, or whether the capacity for pain and pleasure be not rather, though in very different degrees, the inheritance of the higher classes only, varying according to the perfection and development of the cerebral organ, and unknown where this does not exist.

To waive, for argument's sake, an objection which, in the present case, would prove fatal to the asserter, "that all animals, as such, feel;" viz. that he who asserts should attempt to make good his assertion by some process of reasoning, I would begin by the remark, that organic sensibility by no means implies sensation in its common acceptation, since extreme organic sensibility is shown by several classes of animals who have yet no trace of ner-

vous system at all!—a remark quite proper in this place, since the two propositions are constantly confounded, and the very erroneous inference drawn from this confusion, that an animal, in whom the first is declared, necessarily possesses the faculty of the second. Should you ask a person whether he imagines a worm to possess sensibility; if his mood be jocose, he may ask you whether you are an angler; if of graver constitution, he will answer at once affirmatively, and appeal to the contortions of the animal on the fishing-hook, bidding you look there, and be satisfied. Do these brisk motions and lively contortions not quite satisfy you? He bids you reflect on similar contortions displayed under acute human suffering. If you are still obdurate, you may dispute the analogy, may admit the similitude of man to a worm as far as it will go, but not be able to see how the wrigglings of the reptile can be taken for evidence of any thing beyond an irritable organization, put into play by the application of a stimulus; for motion neither is sensation, nor does it imply sensation. Who, for instance, is conscious of the normal motions of that heart which is always beating? of those eye-lids that are all day blinking? of those secreting organs always at work? and of the mechanism of respiration that is never idle? So plainly does motion not imply feeling, that we are informed by Dr. Grant, that in piercing the bodies of poriferous animals with a red-hot needle, no contractile action ensues, though these animals at other times display motion. Even where motion is extremely lively, sensation need not be present; for "the zoophytes possess a power of motion surpassing almost any motions we can produce by any stimuli in the highest forms of animals, which motions we are, in them, wont to ascribe to irritability*."

Hence we may conclude, since animals uninjured and unmutilated display very brisk movements, and shew great irritability, that the mere exhibition of the contortions of the worm, and of other animals whose bodies may have been mutilated, does not render it sure, or even probable, that they suffer pain. When we cut a worm into pieces, and see each part in motion, are we to believe that this reptile feels through every link of his divided body? If so, he has no personal identity; he is not one but many. A cat is, by courtesy, accommodated with nine, but a worm must have a hundred lives!

No kind of motion, therefore—that is, no degree of irritability, or organic susceptibility to a stimulus—proves the animal to possess the superior endowment of sensation; and it may be mentioned among the essential differences between the two, that while organic sensibility may be independent*, in certain cases of nervous influence, sensation, on the other hand, cannot be conceived without a nervous system; and further, I would remark that even this evidence, could it be had, would be incomplete, and that it is imperative to do more than to demonstrate nerves in order to prove sensation; there must be a brain. Our nerves are merely the rail-roads, so to speak, between the brain and the external world; they are not the sentient organ; they do no more than transfer the material which is to be coined into sensation in the great central *officina;* and if we interrupt the arrival of the supplies from without (by dividing the nerve or otherwise), no sensation can take place. If, then, no sensation occur when the communication between the nerve and brain be stopped, how shall sensation be pretended in those animals who have no brain? and how many animals have no brains? It has indeed been pretended, that in the lower tribes of animals ganglia supply the place of brains; but as this assertion has not even a shew of plausibility to defend it, and as it is wholly unsupported by argument, I shall think it amply sufficient to deny it, for the following reasons:—

"A ganglion may be pricked, cut, or torn, without the animal appearing conscious of it, and without any contractions taking place in the muscles." "If all

* Dr. Grant.

* It is seen to be so independent, that not only the lower tribes of animals, but many plants too, perform the functions of secretion, absorption, excretion, &c., and shew irritability, which have been called nervous, and supposed to be directly under nervous control; but in these cases, to assume the performance of these functions as evidence of the question, would be a *petitio principii*, and there is no other. We suffer ourselves to be misled here and elsewhere, by the dangerous, but only argument in these cases—of an imperfect analogy.

the cervical and first thoracic ganglia be removed, we do not perceive any sensible and immediate derangement in the functions, even of the parts to which their filaments are distributed." "If the proofs are required on which certain authors would establish their doctrine that the ganglia may be considered as nervous centres (little brains, &c.), we shall be surprised to find that there are none, and their assertion is nothing but a *jeu d'esprit*."—(Magendie.) But now, if in place of no argument (as we say), these writers had every argument in favour of the ganglia conferring sensation, these ganglia only begin to exist in the mollusca; and thus unless something else be set up for these pretended little brains, all animals below these tribes of being are left without an appareil for sensation. In maintaining the necessity of a brain, in order to confer sensation, it is quite immaterial to my argument in what light this organ be viewed; for whether itself be the sentient organ, or the medium of sensation to the mind, it is still the depository or storehouse of impressions; and whether the mind feels in it or through it, sensation cannot but be viewed as a mental function. Without involving ourselves in the intricacies of metaphysics, in considering whether, as some have asserted, all mental operation issues at last in sensation, or in viewing it merely as one of the primitive mental faculties, with the Scotch school, still all metaphysicians, from Locke to Kant, and from Kant to Cousin, consider sensation as implying two things—an organization capable of receiving an impression, and a mind capable of recognising it when received. Sensation, in short, view it as you will, must be a compound faculty, in perfecting which, both mind and body are engaged; and if the influence of the former has been overstated by the metaphysician, it has certainly not been sufficiently considered by the physiologist*.

That all animals do not feel alike, will scarcely be disputed by any: there is, therefore, a progression in the scale of sensation; and where there is a progression, there must have been a beginning. And if it be here objected that the Creator made nothing without design, and that he plainly designed the happiness of his creatures, which cannot consist with insensibility, it may be answered that this statement supposes the objector to be in the secret—a mere *petitio principii*.

Paris, July 1834.

MORBID APPEARANCES FROM POISONING WITH ARSENIC.

To the Editor of the Medical Gazette.

Sir,

In the newspapers of last week I observed accounts of two trials for the crime of poisoning: in both instances, arsenic was suspected to have been the baneful substance taken into the stomach. In reading the reports, I was rather surprised that the medical witnesses made no allusion to the appearances of the colon. I believe Mr. Brodie ascertained, by his experiments with arsenic, that it had a very peculiar effect on the colon, producing great contraction in the diameter of that bowel. I have met with two cases of unequivocal poisoning by arsenic, in both of which it was, in a remarkable manner, contracted throughout its whole extent. I hope those who may have the opportunity of examining, *post mortem*, clear cases of poisoning by arsenic, will not fail to pay attention to

* How much the mind is concerned in sensation, is proved by some being able to imagine a pain till they feel it, or sometimes by not feeling it, though the painful impression be there, from the operation of some powerful moral coercion, or from the attention being preoccupied at the moment with something else. Need we go to the battle-field to seek for proof or illustration; and will not Crabbe's old lady, whose gout was troublesome or the reverse according to her luck at cards, be at once admitted by a candid critic to be as consonant to fact as it is true to nature? How much the mind has to do with sensation, is well illustrated, I think, and physiologically too, by Livy, in his anecdote of M. Scævola, whose right hand was burning, while he witnessed its destruction, "velut *alienato* ab sensu *animo*;" and perhaps still more beautifully in the person of almost every poet or imaginative writer who ever lived. Look at the race of men celebrated for their boundless fancy, and falsely supposed on that account to be the peculiarly favoured of heaven! Yet of this very race it is testified by inconsiderate friends, that they are always querulous, making all around them unhappy:—behold, they say, men who are intolerant of the least pain, and make as bitter lamentation over a corn as was wrung from Philoctetes in his worst moments of suffering! But a medical man would do well to study the philosophy of sensation, and he would find that it was this very susceptibility of mind—this very liveliness of mental perception—that first excited the attention—it may be to a scratch—which the imagination, now rivetted to the spot, really made as painful as it is stated to be by the sufferer.

the colon, that we may ascertain how far the circumstance of contraction may be relied upon as evidence of that kind of poisoning.

I would add two further remarks relative to the appearances of these cases after death. Mr. Tucker, in his evidence upon the trial at Exeter, stated that in cases of poisoning by arsenic taken into the stomach, he should always expect to find some of that substance adhering to the inner coats of the stomach. That this is not generally the case I am not prepared to assert; but it certainly is not universally so. In one of the cases I have mentioned above, on examination, the stomach was found empty, and no particle of arsenic could be discerned upon its mucous membrane, and the only morbid appearances were two small bright red spots on that membrane, near the fundus. The subject of this accident was a child two years old: it had eaten some poison prepared for rats.

In the excellent work of Dr. Paris on Medical Jurisprudence, vol. ii. p. 230, he states it as his opinion that when arsenic is applied to the mucous membrane of the alimentary canal during life there will not be a well-defined line of demarcation; the transition from the diseased to the healthy structure will be gradual, and the limits of each imperceptible. This does, I must confess, appear very probable, *à priori*. But in the other case to which I have alluded, in which, for the purpose of suicide, a large quantity, perhaps an ounce, was taken, the line of demarcation was most strikingly apparent. There was found in the stomach about four ounces of viscid bloody mucus, with nearly a drachm of the white oxide of arsenic: that part of the mucous surface in contact with the fluid appeared of a deep red colour; the remaining portion exhibited its usual hue.

I have confined myself to the statement of facts; if you consider them worthy the notice of the profession, perhaps you will honour them with a place in your valuable journal.

I am, sir,
Your obedient servant,
Joseph Houlton.

Lisson Grove, August 7, 1834.

ON THE EMPLOYMENT OF ALKALINE SALTS,

IN CERTAIN INFLAMMATORY DISEASES, AND OF THE URINARY ORGANS, AS PROPOSED BY MASCAGNI.

To the Editor of the Medical Gazette.

Sir,

In reading the second Gulstonian lecture, lately delivered at the Royal College of Physicians, by Dr. George Burrows, and published in the Medical Gazette for July 19th, my attention was particularly directed to the following question:—"Is it not possible," says the learned Doctor, "that these alkaline remedies will exert their influence at other points of the animal economy, as well as in the kidney, and thus render the soft solids, in different parts, in a state more fit for absorption?" As the following facts seem to me to put out of question this important therapeutical principle, and they appear not generally known, I thought myself justified in sending them to you, for insertion in your valuable journal. In the meantime, I am perfectly convinced that my learned friend Dr. Burrows himself will be pleased in finding that the practical deductions on the internal use of alkalies, drawn by his experiments and observations, were not only on record, but even put out of question, by such a respectable and valuable authority as the following.

The celebrated Mascagni, having observed that those earthy concretions occasionally found after death deposited in different cavities of the body, or adhering to some viscera, and to the interior surface of arteries, were readily dissolved by the carbonate of potash, of soda, and ammonia, was induced to believe they might also have a similar, if not a greater effect, on the living subject.

He first tried on himself a solution of the carbonate of potash, when labouring under an attack of gravel, to which disease he had oftentimes been subject, and found himself very soon relieved by the simple use of that remedy; so much so indeed, as to be at last entirely cured of the complaint. After this favourable result, having also observed how easily coagulable lymph could be dissolved, even in a weak solution of those alka-

line substances, he was naturally induced to think they might be of greater service in various instances of acute inflammatory disease. In individuals who died of pneumonia, and where the lungs were found in a state of complete hepatization, Mascagni had observed not only both the ventricles of the heart filled with black blood, mixed with abundant concretions of an albuminous substance, which plugged up the pulmonary vessels themselves, and prevented any circulation through them: this substance, which was perfectly similar to the buffy coat of the blood drawn from the veins of the same individuals during life, was completely dissolved by those alkaline salts.

Led by these facts, Mascagni suggested their employment to a physician, who obtained by the use of them wonderful effects in an epidemic pulmonary affection, which, in 1800, made great ravages in the district of Chiudino, in Tuscany. Mascagni recommended also the use of the same alkalies in those instances of dropsy which are principally induced by obstruction of lymphatic vessels and glands from depositions of coagulable lymph, in consequence of an inflammatory diathesis.

These facts and observations were published by the celebrated anatomist, in a memoir "*Sull' uso del Carbonato di Potassa, per le renelle, e peripneumonie,*" inserted in the 11th volume of "*Memorie della Società Italiana delle Scienze.* Modena, 1804." The above statement was quoted from a small work, which bears the title of "*Elogio di Mascagni, di* Tommaso Farnese, M.D. Milano, 1816;" and principally from note xviii. p. 108.

Dr. Farnese, who had been a pupil of Mascagni, has repeatedly employed the alkaline treatment; and in his publication gives an account of several cases of disease of the pulmonary and of the urinary organs successfully cured by it, without the use of any other therapeutical means; and remarks, that they may be cured without bloodletting, or with a less quantity of blood being extracted, as Mascagni himself had noticed it before.

The dose employed was from half a drachm to one ounce, dissolved in six ounces of distilled water, to be taken *gradatim*, in the twenty-four hours, with diluent drinks, and increased gradually according to the particular circumstances of each case.

Since these facts came to my knowledge, I have freely employed the carbonate of soda with the same view, and on the same principle, for the course of several years, and with the most favourable result.

In the meantime, sir, I take this opportunity of sending you an article, which shall be the first, in defence of my learned countryman Bellingeri, whose work and opinions on the fifth and seventh pairs of cerebral nerves, has been, in the above-mentioned number of your periodical, so greatly misrepresented by Mr. Shaw, in his article "On the Originality of Sir Charles Bell's Discoveries in the Nervous System."

I am, sir,
Your obedient servant,
GAETANO NEGRI, M.D.

26, Poland-Street, Oxford-Street,
August 9, 1834.

SKETCHES OF THE PRESENT STATE AND FEELINGS OF THE MEDICAL PROFESSION IN THE PROVINCES;

And more especially in Liverpool and its Vicinity.

BY JAMES COLLINS, M.D.

ONE would suppose, by various articles in the Medical Gazette and other publications, that few subjects excite more interest than how far the certificates of our provincial hospitals and schools of medicine ought to be received as qualifications for a degree in surgery. As the subject is a most important one, and seems to admit great difference of opinion, it may, perhaps, be desirable to some of your readers, Mr. Editor, to have some notions, derived from personal and local experience, of how our provincial schools and hospitals work—at least in this neighbourhood; and of the sentiments and views of the practitioners in the provinces on these matters. In attempting to do this, I by no means presume to speak the sentiments of the lecturers, nor of the gentlemen attached to what are termed "the recognized hospitals:" in the decision of this question *they* have a separate and

distinct interest, and therefore very naturally stand up for the glories of provincial teaching, and for all changes that seem to promise grist to their own mill. Hence it is not surprising to see the war that rages between them and their London brethren on this subject; each, in the spirit of the fable of the town besieged, advocating his own interest, and maintaining that there is nothing like leather. But it is to be hoped, that whatever changes may take place, the interest of the profession will be consulted in preference to their's; as they form but a small fraction of it, however respectable, collectively or individually, they may otherwise be: and yet, strange as it may appear, they have as yet been the principal evidence before the Parliamentary Committee appointed to inquire into the state of medicine; at least, as far as I know, no others have been summoned from this quarter.

The general complaint in the provinces (and, I presume, every where else), is, that there are as many doctors as patients; that the profession is overstocked, and hence that very few can now live, much less make money by it; and consequently, that instead of opening additional sluices through the medium of provincial schools, to inundate it still further with competitorsfor practice, some means ought to be devised to adapt, in some degree, the supply of medical men to the wants of the public. Hence they protest against all changes that seem to make things worse instead of better; against the principle of covering the country with institutions to manufacture medical men at little or no expense, and thus making it as easy and as cheap to become one as a mechanic or craftsman. They laugh at the idea of free-trade in medicine as in commerce, as if the cheapness of the doctor constituted his excellency; and ask, why should it be attempted to be applied to medicine and not to divinity and law? What would become of the latter, if every member were allowed to establish a factory of his own, and to dispense with the pupils going to the University, or the Temple, by pulling them through his own machinery? Do you think, if this were the case, that these professions could long keep their ground, or be objects of attraction or emolument?—for every one must admit that he enters a profession either for money or personal qualification, and not from any Quixotic feeling to serve others. The medical men here witness with pain and humiliation, the effects of this free-trade system in manufacturing surgeons: they see medical schools, of one sort or another, multiplying around them; some with two or three lecturers, or (as the phrase goes) professors; others with more or less; none with a full or regular complement; all rivalling and jealous of each other, and lecturing for nothing, or next to nothing; and yet at the same time pretending, that in doing so they are upholding the rank and dignity of medicine, though they are doing every thing in their power to degrade it, by this wholesale system of free teaching. Would you believe, sir, that we have a school of medicine at the Infirmary, one at the Royal Institution, one in Seel-Street, and one or two more building, the speculations of private individuals? Besides these, the Dispensaries and the Northern Hospital intend, this season or the next, to open schools of their own. They have already held out to the subscribers prospects in this respect, and thereby stimulated their zeal to contribute to their funds. Indeed, this mode of appeal is latterly becoming a very popular and plausible way of raising the wind: the subscribers are tickled with the idea of converting our hospitals and dispensaries into schools of medicine, and making them thereby more subservient, as it were, to science and humanity. Thus, if all these institutions succeed, we shall have seven or eight at work at the same time, in making medical men for nothing, or next to nothing. At present there are lectures on surgery given at the Infirmary, by one of its surgeons, free to the pupils who attend; so were the lectures on the practice of physic, delivered in the same institution; and so are, I am told, the lectures on materia medica, now going on at the house of one of its consulting physicians. If this is not making the profession cheap and easy of access, I know not what will: they are all anxious for a class, and, as they cannot get one to pay, they admit them as they come, calculating for remuneration on the indirect advantages of increasing their practice through the friends and interest of the pupils. The surgeons' apprentices, the chemists' lads, the young merchant, the amateur students, all who have curiosity or mo-

tive to attend, may occasionally be seen, scot-free, in the lecture-room; all that is expected in return, is often nothing more than that they should trumpet the talents and dexterity of the lecturer, and give him a good word and a patient when they can.

Others, again, stick their advertisements of lecturing in the medical library, the shops, the newspapers, and other places of notoriety, without giving or intending a lecture. In fact, this custom is getting very much into vogue: I wish it were more general, as, however quackish it may be, it can never injure the profession like the free-school system. The lecturers, when reproached with admitting persons to their lectures who never pay, though able to pay, say they do no harm, as they never give certificates of attendance without being paid. This may be so, for aught I know; but there was one gentleman, now deceased, in the habit of giving certificates of attendance on lectures he never delivered: he differed from others, inasmuch as he got paid for lectures he never gave. Even in his last sickness, a few hours before his death, for the sake of the money, and to oblige a friend, he put his name to one of those fictitious certificates; and yet he was a *respectable* man, a graduate of Cambridge, and a man of very considerable property. But, even supposing that no certificates are given without payment, yet harm is done to the profession by not requiring it from the pupils attending the classes. Persons are thus qualified, if they have the courage or impudence to dispense with the license of the College, as many of them do, to turn general practitioner or an advertising doctor, or to enter our merchant service as surgeon. Chemists and druggists are thus taught to prescribe with a little more tact than they otherwise would do, at the expense of the regular practitioner.

A young man is now in very extensive practice among the lower and poorer classes in the north end of this town,—in fact, the Æsculapius of that district,—whose only medical education has been attendance on one or two of these courses of lectures. He is not a solitary instance. I allude to him merely to illustrate, by his example, some of the results of our provincial teaching.

Now, I ask, in the name of common sense, is what we complain of in the provinces,—namely, the superabundance of competitors for practice,—likely to be remedied or checked by this system? Does it add to the interest or respectability of the profession, to cram into it persons who never pay for their education? Is there not something in this disreputable to the teachers and the pupils? Would it not be well that those who have the power should check the system, and fix some fair and moderate scale for the prices of lectures, and require proof that they were delivered, and attended, and paid for at the time of delivery? There cannot be too much vigilance in this respect.

Some think that no certificates of lectures ought to be received, except given in some legally constituted college, not from any feeling that the private lectures may not be in some cases as good,—as intellect is not measured by latitude and longitude,—but in order to diminish the facilities of entering the profession at little or no expense: and they think that if no other good resulted but the mere additional expense and trouble, from obliging pupils to receive some fraction of their education in London or the other capitals, that it would even on this account be desirable to compel them. But every one knows that enlarged views and liberal sentiments are more likely to be cultivated by doing so, than by confining them to the narrow sphere of a provincial school; and, as for the cant of their morals being endangered by the vices of the capital, I can only say that our Liverpool lads are not so innocent as the saints in Bristol wish to make us suppose theirs to be. By these means and other causes the profession is rapidly falling, and approximating the rank and pay of the lower classes.

Now for the hospitals.

The great object of ambition here among medical men, next to extensive practice, is to be appointed physician or surgeon to the Infirmary or Lunatic Asylum. Once there, we consider the road to practice and local reputation fairly opened before us. But it is no easy matter to get there, as all our medical charities form, as it were, a circle, and work within one another; for there was no chance, until lately, of reaching the infirmary, except one began practice with the dispensaries. The system that prevailed was to advance according to

seniority, and without opposition, when vacancies occurred, the medical men of the dispensaries to what they termed the higher charities, from it to the workhouse and fever hospital, and then in rotation to the lunatic asylum and infirmary. All others were excluded, and, by tacit consent, placed among the common herd of practitioners. It was true, now and again, a solitary exception took place, when disunion or discontent arose in the enemy's camp. It was to a fortuitous circumstance of this sort that the present professor of medical jurisprudence in the University of Edinburgh owed his appointment. For years he was excluded, and for ever would, were it not for this, (however respectable his character and merits), because, forsooth, he did not begin his practice here as physician to one of the dispensaries. This was a preliminary and indispensable step to be eligible, with any chance of success. Thus an ingenious and exclusive system was formed to advance those who had sufficient interest and patronage to be attached to the dispensaries, to the higher charities—the lunatic asylum and infirmary, and to keep all others out from being their competitors; for, generally speaking, the interest that pushed you into one carried you through the others in your turn; in fact, the rights and privileges of our freemen were not more corrupt or exclusive, nor their system of electing their friend better contrived. The doctrines propagated by the medical men, and until lately in a great degree believed by the public, in support of this system of rotation and exclusion, were, that this system was necessary in order to rear a body of experienced medical men worthy of the great and growing commerce of this town; and that that experience could only be had in the public charities; and therefore it was incumbent to begin with the dispensaries, in order to get or gain some little experience for practice: that this experience was still more increased by being attached to the fever hospital and workhouse, as uniting the advantages of both; but that, being attached to the infirmary was the strongest attestation of its existence, in all its fullness and perfection, in the medical men of that institution, as possessing the accumulated experience of all the charities. The idea was a novel and a useful one for those who reached the top of this ascending scale, as it bound all the medical men of our public institutions firmly together; gave them a common interest in upholding each other, and paying the homage of admiration to the skill and experience of those who have passed through the rotation; each calculating that the honour he rendered to others would, in his turn, be rendered to himself, when his day of promotion arrived; and that, at all events, by acting in concert, and confining to themselves the claims of *experience*, they would thereby narrow the field of competition, in excluding 17-20ths of the practitioners of the town, who did not belong to the charities, from the chances of a professional reputation, and thereby raise their own fortunes on their ruin.

You may easily perceive, therefore, that it was a matter of great importance to a young man, when he settled here, to become attached to one of the dispensaries; and that frequent contests, ill blood, and underhand means, took place to put in friends, and keep out enemies. When the high church or corporation party interfere, as they usually do in one shape or another, no interest can withstand them: their power here is as predominant as in the elections of the borough, all giving way before them. Hence it is a standing rule with all who aspire to the honour of the infirmary, to curry favour with both; first, with the subordinates, and thus by degrees to reach the principals. The usual plan is to laud both to the skies, to support their views and politics through thick and thin; and to swear, if necessary, that our established clergy and corporation are the most humane, liberal, and enlightened body in the world; to fight their battles in public and in private, and to seem to have no other wish or feeling than theirs. Hence, you may rest assured that those who usually reach the infirmary are true blue—the corporation colour. This interest once gained, that of the clergy necessarily follows, as they depend on the former for patronage and support; and therefore it is not difficult to explain how attached the church is to the corporation, and the medical men to both: how, like Castor and Pollux, they all seem only to live and die for

each other. Various means were contrived from time to time to keep up this advantage of being attached to the public institutions, some of them so original and peculiar, that I think I may mention one or two in illustration. A medical society was formed, consisting almost exclusively of gentlemen attached to them. It did not meet to discuss medical or scientific subjects; in fact, neither these nor literature came within the range of its constitution; they only met to dine together once a month, each taking the expense of the banquet in his turn, at his own house; there they met, to eat, drink, and be merry; they were bound, not by any formal rules, but by implied custom and etiquette, to support and call in each other when and where they could; to vaunt and circulate their mutual and reciprocal merits; and to keep up the credit and reputation of the public institutions, and consequently themselves. But from one cause or another these dinners died away, after an existence of ten or fifteen years, though at the time of their dissolution apparently in full and vigorous health. The dinner, it is said, became too expensive, and the returns of gain, or professional advantages, too remote and contingent. Some got tired, others disgusted—all indifferent; and thus a society of many years' duration gave way without any apparent cause but the badness of the times and the indifference of the public.

Another society soon rose upon its ruins; religion, fact, party and political prejudices, were enlisted to give it birth and support. Circulars were distributed among all classes, with the names of most of the medical men attached, or who were attached to the public institutions, and of the principal inhabitants of the town and its vicinity, particularly of the clergymen, and saints, as they are termed, of the established church. These circulars lamented the want of a moral, religious, and medical education in our nurses; they stated that the recovery and comforts of the sick were often more dependent on them than on the regular medical attendant, and, therefore, that an institution to educate them in the duties of a christian, and the principles of their calling, was most desirable. Subscriptions were solicited and obtained; a house was taken, and notice given that they were to be educated in the Infirmary, under the care and superintendence of the medical men there; that they were to be taught how to handle a patient, to give drinks, and to remove all offending matter from the sick room; and, above all, how they were to avail themselves of fit and appropriate moments to communicate religious comfort to the patients. The scheme was well received; the religious rejoiced at its anticipated pious results; and the medical men felt the all-important and lucrative returns, in a professional sense, from these new missionaries, and ways and means of raising the wind. The public were cautioned against employing any nurse but such as could produce the diploma, or certificate, of this new school of medicine; they were told, when they had occasion for one, to send to the Nurse Institution, and if they had any complaints of any employed, to make them there. The nurses were also requested from time to time to get their certificates renewed, and to render, in the interim, an account of their mission; and that unless they were punctual and satisfactory in this respect, they were not to have their certificates renewed. The institution continued in existence for some time, but a storm was gathering to destroy it. The tendency of the scheme was too obvious to last long. The nurses were made to depend on the good-will of the few medical men that managed and patronised the institution; and, of course, were bound in conscience and interest to praise them in return. Thus they were to play into each other's hands, and ultimately to throw all others overboard, in order to make room for their patrons. The medical men unattached to the public institutions took the alarm, and denounced the policy and cant, and ultimate designs, of this nursery of nurses. The dissenters joined them; for it seems by its laws all the nurses were to be of the established religion, and that no others were to be admitted or recommended. Hence a powerful re-action set in, and upset the whole scheme, contrary to the expectations and calculations of its founders. Thus this project, like its predecessor the Dinner Society, died, though ushered into life with the greatest patronage, both lay and ecclesiastical, and with every appearance of success and longevity.

Here, I presume, as every where else, a judicious selection in religion and po-

lities has great influence on the success and destiny of medical men. Hence they use the greatest care, and often evince extraordinary tact in this respect. The practice usually is, for those beginning to settle, to remain for some time unattached to any particular sect or party, and to observe in silence how the wind may blow, and how others got on before them. In the interim they are not idle; they are active in extending their acquaintances, and by pliancy of character fitting themselves for whatever may turn up, or for any vacancy in this or that congregation. Whatever people may say of the usual want of religion in medical men, here they are remarkably edifying. Some of them preach, pray, and assist as regular stewards of the vineyard, by word and example. Some read the service of their church in the streets on Sunday, as they pass along in their carriages or gigs, intimating thereby that they have not time for attendance at their church or chapel; others are very conspicuous leaders of class meetings; and many of them pray with their patients as regularly as they visit them, and hold prayer meetings at their own houses for them and others. In fact, few towns present such an instructive and edifying example as Liverpool. Here all sects have their favourite physician and surgeon; church and chapel differ little in this respect. Here every thing connected with religion or morals, bible and missionary meetings, temperance and providence societies, tract and religious associations, readily meet the support and countenance of medical men; and as the labourer is worthy of his hire, they have their reward in increased practice and connexion, or occasionally in a happy and lucrative matrimonial hit. It is no uncommon thing to see medical men here pass through all sects before they get one that answers. Some have begun their career as unitarians, then turned presbyterians, independents, methodists—in fact every thing before they finally settled down into their present character of high church folk; and I am satisfied would as easily revert again to the point whence they started, had they an interest in doing so. The great advantage to be had in being occasionally a member of different sects, is that acquaintances are made in each, and retained by a little tact and hospitality, no matter into whatever religious denomination you may afterwards pass. Indeed, the success of one of our first surgeons is owing to a calculation and tact of this sort; others have identified themselves at once with the high church and corporation religion of the town, and have generally been the most successful. They embarked at once with the strong party in the days of profligate corporation expenditure and rule, and have kept steady and true to their party and interest. These were generally by birth or relationship connected from the beginning with it, and instinctively saw their interest in sticking to it; but things are wonderfully changing at present. The public are beginning to care very little or nothing about them; they neither accelerate nor retard one's getting into practice, and are beginning to have no other result than bad feelings, mutual jealousy, and distrust.

Liverpool, August 9, 1834.

NITRATE OF SILVER IN FISSURED NIPPLES.

To the Editor of the Medical Gazette.

Sir,

In the number of your excellent periodical for 14th November, 1829, I very briefly called the attention of the profession to the efficacy of the argenti nitras *in substance*, in the treatment of fissured nipples. I have since had further experience of its value in healing those very troublesome ulcerations of the nipple; and I am happy to see that, in your number for the 9th of the present month, Dr. Hannay, of Anderson's University, Glasgow, also bears testimony to its merits. It is, indeed, a safe, speedy, and most efficacious remedy; and though its application is in some instances very painful, the pain is of only a few minutes' duration, and is seldom, if ever, so intense as that caused by the sucking of the child —I am, sir,

Your obedient servant,

Edmund Bowden,

Member of the Royal College of Surgeons, London; and Surgeon to the Chelsea, Brompton, and Belgrave Dispensary.

135, Sloane-Street, Aug. 11, 1834.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

A Demonstration of the Nerves of the Human Body. By JOSEPH SWAN. 4to. 1834.

A MORE admirable work than this, we have not for a long time seen. It is at once a most acceptable boon to practical anatomists, and an exquisite specimen of the arts. The great work, or *opus majus*, as it may now be called, of Mr. Swan, on the Nerves, is of European celebrity. It is, however, from its large size and necessarily expensive nature, not calculated to be in every body's hands: its circulation can only be limited, and perhaps on the whole is more conducive to the fame than the profit of the author. But the compact edition of that work which is now offered to the public will, we think, be found to gratify the wishes of all parties. In its size it is a moderate quarto: it contains every thing that can be desired in a work for immediate reference on the nervous system; and, with all, its amazing cheapness must be a recommendation for it of no inconsiderable weight. Twenty-five splendid plates, engraved in the highest style of art, and accompanied by an abundance of explanatory letter-press—all for the moderate price of a guinea and a half—form an attraction, so far as we can judge, not easily to be resisted.

We sincerely trust that the author's laudable design in bringing out this edition will be accomplished. He apprehended—it seems he was threatened with—piracy, in case he did not endeavour to comply with the general wish, to possess his delineations of the nerves in a more popular form: so that, in order to counteract any knavish invasion of his property, which might at the same time probably injure his reputation, he determined at a considerable sacrifice to undertake the present edition, availing himself of the assistance of the same eminent artists who executed for him his larger work.

It would be idle and superfluous to call the attention of our readers to any particular plate, or plates, in the volume; every professional man will, we have no doubt, soon see the work, and judge for himself. We cannot, however, help noticing the striking excellence and beautiful execution of plates 5, 6, and 13. In the two former we have the principal plexuses of the trunk pourtrayed; and in the latter the facial nerve and branches of the fifth. If a sight of these do not justify our highest commendation, and satisfy even the most fastidious anatomist and dilletante, we are much mistaken in our estimate of pictorial talent, and graphic representation.

The Surgical and Descriptive Anatomy of the Bones, Ligaments, and Joints. By W. H. THOMAS, M.R.C.S.

WE are much pleased with the manner in which Mr. Thomas has executed his task. He has treated in a very interesting way a subject which, in the hands of most of his predecessors, has been but a dry business. By introducing a moderate portion of physiological comment, and calling the attention of his readers to the diseases and treatment of the parts of which he gives a description, he continues to enliven his little work very meritoriously; and we incline to think that it will in consequence be a favourite with students. The brevity which the author consults, evidently with a view to condensation, and perhaps at the same time to lighten the style, sometimes betrays him into inaccuracies of expression, which he would do well to amend in revising his volume. For example, he defines fracture as "the separation of one part of a bone from another, resulting either from external violence or undue muscular action." It can scarcely be necessary to remind him that this confounds fracture with a *wound* of the bone. Chaussier's excellent definition of the term is a model of perspicuity: according to him, as we recollect, fracture is "the rupture of a bone caused by a blow, fall, effort, or violence of any kind, which overcomes the flexibility proper to the organ." The opening chapter, which seems to have originally formed the substance of an introductory lecture, contains a variety of valuable practical remarks. We wish Mr. Thomas had given us an index, or complete list of the parts which he describes: it would be an adjunct of no small importance to a work of this sort, abounding with technical terms. But, on the whole, we cannot hesitate to recommend the work, as likely to be a very useful manual for medical students.

MEDICAL GAZETTE.

Saturday, August 16, 1834.

" Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

HYGEISTS AND OTHER QUACKS.

THE pill which the " Hygeists" have recently been made to swallow at York, has proved very bitter to them. One of the worthy agents in this system of imposture has been sentenced to six months' imprisonment for manslaughter, and the consequence is, that the whole firm of pill-rollers is in alarm at the probable effects of this " untoward" event. " The fears and apprehensions of many persons (they inform us) are so unaccountably excited and worked upon by such an occurrence as the present, that many of them would not take another dose of Morison's pills, though they have before reaped the greatest benefit from them."

We are heartily glad to hear that so rational an effect has been produced by the late example of manslaughter, produced by this " universal" nostrum; and we refer again to the subject, because we have reason to believe that some benefit has accrued from our former notice of it, the effect produced by which we are anxious to confirm and extend. Indeed, we think that medical men are too remiss on such matters—too delicate in expressing their opinion to their patients. " *Si populus vult decipi, decipiatur*" is their maxim, and under its influence the nuisance is permitted to flourish. Mankind in general have certainly a strong disposition to be credulous on subjects which they do not comprehend; and their gullibility is nearly in proportion to their ignorance. But the more they are enlightened, the less prone they are to become the objects of this, or of other kinds of imposition. It is said that quackery flourishes most among the higher classes of society; and this to a certain extent is true; but then it results mainly from the influence which opulence and fashion exert in this country; combined, however, with the fact, that the ignorance on subjects connected with medical science is quite as great among the higher as among the lower grades of life. If, however, we look to men whose minds are even moderately enlightened, very few, indeed, among them will be found the dupes of quackery. St. John Long numbered many noble and aristocratic personages among the simpletons from whose flesh he extracted mercury (see Lord Ingestrie's letter), and from whose purses he extracted gold; but except one superannuated and imbecile physician, we are not aware that any persons of scientific education were to be found among them. Still the number of persons in these kingdoms who are the dupes, and who even fall victims to the pernicious arts of empirics, is unquestionably very large—larger, as it appears to us, than it would be, if the members of our profession were sufficiently active in exposing the impostors, and supporting at once the welfare of the community and their own particular interests. In an attempt to do this, in the present instance, they will be greatly assisted by the illiterate, but not unamusing, manifestos which emanate from the " British College of Health," and which, we presume, proceed from the pen of the great " Hygeist" himself. We ought, perhaps, *in limine*, to explain what this college is—or rather to state that it is not, as many have supposed it to be, the same as the Collegium Wakleyanum, or London College of Medicine; but though, like this, it is a private piece of humbug, yet it differs in that it is a much more important and successful speculation. The " British College" is a building in the New Road, not far from the London University; and as this became an *University*

by the assumption of the designation, so that became a *College* by having the word painted in large letters upon the wall. From this manufactory of "Health" emanate hundreds of thousands of pills and pamphlets, which are swallowed and read, all over the kingdom, with astonishing avidity; and, indeed, considering that the pills are actively purgative, the simultaneous issue of the pamphlets is, perhaps, not without its use. But as these documents might be suspected, if put forth in the name of the party deriving direct emolument from the sale, so the ingenious device is adopted of giving them the appearance of emanating from an association or society of learned persons; and as the "magic of a name" is well known, so they rejoice in the imposing appellation of *Hygeists!* The last production of this grave body, —the more interesting to the imagination, because it is invisible and secret as the wonders which it works—relates to the case of the unfortunate man at York, who lost his life from the use of Morison's pills. It is an excellent specimen of the kind of language adopted, and may serve as a clue to the class of persons whom the parties have the cunning to entrap. That it should contain impudent boasting and gross misstatements, was to be expected: but we confess we were astonished to find it so rich in all the attributes of absurdity and bathos as it has proved,—being, as it is, sans truth, sans talent, sans grammar—sans every thing. It is amusing, however, and we shall insert a portion of it,—as it may serve, at some future period, to show the curious in such matters the style in which quackery was wont to address her votaries in the enlightened æra of the nineteenth century.

"People of England,

"An extraordinary case has taken place at York. A young man, ill of malignant small-pox, has died—*although* he had been treated by Morison's Universal Medicine, of the British College of Health. But, mark ye! not left to their medicines only, as he should have been; his friends and relations becoming impatient, a doctor was called in, who administered some drugs and stimulants; after which the patient soon sunk. * * *

"The first consideration is, in this case, whether the patient died from the effects of Morison's pills, or from the drugs administered by the doctor. He might have appeared very ill—naturally to be supposed, under such a malignant disease, and the operation of the medicine; but it is well known to all hygeists, that patients soon rally from such a state, if let alone, and nothing done to interrupt the operation of nature; and all stimulants, opiates, chemical and mineral preparations, are well known to hygeists to have that effect, namely, to check nature in her salutary crisis. If this doctor found the patient so ill, why did he not leave him alone, and decline prescribing for him at all? for, left as the case now is, Omniscience alone can discover the true cause of death—not *infallible* [*sic*] man.

"The second consideration is, the evidence of the doctor; who, from the analization, declared the pills a composition of various ingredients, of which gamboge is one. Experience and practice are superior to theory, or individual opinion. Now experience and practice have shewn, that those persons who make use of Morison's pills in *largest doses, and the longer persevered with, have invariably reaped most benefit.*—Does this look like deleterious and poisonous?

"Divinity, politics, and medicine, are the natural sciences which most occupy mankind. In religion, have we not had the great reform from the Church of Rome, led on by Luther, which has branched into innumerable sects? In politics, how various the opinions of mankind? In medicine alone, or the treatment of one's own body, a monstrous monopoly, or despotism, is erected, which deprives every one of the liberty of treating their own bodies as they choose, or to advise their friends how to do so. We come into this world slaves to the doctors, and we cannot die without them, but by subjecting our friends and advisers to a criminal prosecution. * * *

"*The Hygeists have offered to take any large quantity of the pills before the courts*

of justice, to convince the world of their innocence. Will the doctors submit to the same ordeal, by taking their deleterious drugs in the same proportion? Here we are at issue. It is a feeling natural to all mankind, to recommend to others what they have derived benefit from themselves: this feeling is the foundation of the British College of Health. Are the laws of the country to annihilate this beneficial propensity of the human mind? * * *

"People of England!—the Hygeian agent at York has been found guilty of accelerating the death of his patient, and sentenced to six months' imprisonment! * * *

"But the Hygeists are bound to announce to you, that it is their belief that the young man who died at York did not take doses sufficiently large and energetic to meet the urgency of the case, and carry downwards the small-pox inflammation; and that if Morison's pills do not cure, no other medicine on earth will; and infallibility only belongs to One—to GOD! * *

"From the extensive consumption of Morison's pills, an easy calculation may be made that a million of persons, at least, make use of them annually; and this they may find corroborated by inquiring at the Stamp-office, Somerset-House.

"The Hygeists have repeatedly given proof of curing those given over by the doctors; but failure always attends doctors being called in after Hygeists, and in such cases blame is laid to the latter.

"British College of Health,
25th July, 1834."

There is one very important passage in this precious farrago; namely, that wherein the "Hygeists" offer to swallow "any quantity" of pills. This reminds us of a singular ordeal practised among the Siamese. In order to discover the justice of a cause, civil or criminal, they use certain consecrated purgative pills, which the contending parties are made to swallow; he who *retains* them longest gains the cause*. We recommend that the above offer be accepted, with some modification, and that somebody skilled in trials of this sort may be selected, to see that the "universal pills" are fairly swallowed, boxful after boxful; proper judges being at the same time appointed, to decide when the Hygeist becomes unable any longer to *retain* them. We venture to throw out this hint for the consideration of Mr. Wakley; he pinned Chabert, the fire-eater and poison-drinker, to his promise, and sealed the fate of that impostor, who, poor devil! never was so roasted before, even in his own oven. Let our worthy contemporary now insist on cramming two or three dozen of Morison's pills down the "Hygeist's" own throat, and he will immortalize himself, and purge the land of a greater evil to regular practitioners than all the "hole and corner" abuses, whether real or imaginary, which he has been so active in denouncing. The project is quite worthy of his peculiar talents; and if he has himself any bowels, the suggestion will not be lost upon him.

Let us observe one other point in the "Hygeists'" address. They refer to the stamp-office, and the reference is but too well founded. The sale of stamps for patent medicines is immense; and, however discreditable to the legislature, we fear it is too profitable to be readily abandoned. And this leads us to remark on another circumstance connected with the impositions of quacks,—namely, the unwillingness of the conductors of newspapers to expose them. In order to try them in this respect, we lately sent to the principal papers a short account of the composition of Morison's pills, and some illustrations of their bad effects, especially at this season. The *Times* and *Albion* were the only journals which gave insertion to the article; and the reason of this is obvious. The proprietors derive so much profit from the advertisements of quacks, that they fear to do any thing which may offend them. Hence the instances of "won-

* *Vide* D'Israeli's Curiosities of Literature.

derful cures," which we so frequently see, and which are regularly manufactured for the purpose, like any other imaginative writing. Hence, too, the difficulty of getting insertion for any counter-statement, though, God knows, our journalists are ready enough to insert any thing which tells against the regular faculty.

The indirect protection afforded to empiricism by the government, and the support of the daily press, especially in the provincial towns, are two of the great means which prevent the growing knowledge of the present age from overcoming, so quickly as it would otherwise do, the baneful influence of unprincipled pretenders on the credulity of mankind.

CORONERS' COURTS.

The County Coroners' Bill, of which we gave some account last week, has been thrown out. The objection of the Lords to making the coroner's court, under all circumstances, an open one, has been fatal to the measure. It must be granted, that there is much to be said on both sides. The publicity so strongly contended for by the Commons, might on many occasions prove only an impediment to the ends of justice, and often highly injurious to public morals. Matters have frequently to be investigated at coroners' inquests, which common decency would require to be examined into with closed doors. On the other hand, the allowance of so much discretion to the sort of persons who usually preside at those investigations, might be of very questionable propriety. The judges in the superior courts are perhaps not improperly vested with such a power; but we cannot satisfy ourselves that bribery and interested motives might not operate dangerously on persons like our present race of coroners. We regret, however, that the Bill has been rejected on this debateable point, which is really of minor importance; it unfortunately leaves the profession, with respect to inquests, still in that oppressed and degrading state which cannot be too much deprecated.

PARLIAMENTARY MEDICAL COMMITTEE.

Mr. Warburton, on Wednesday night, is said to have presented the *Report* of the Select Committee to the House of Commons. We think there must be some mistake here: it is impossible that a *report* could be already drawn up, *unless it was done before all the evidence was heard;* for witnesses were examined even up to a late hour on Wednesday.

EXPENSE OF THE LECTURERS' MEMORIAL.

We understand that the expense incurred by the metropolitan teachers, in opposing the grant of a charter to the London University, with the exclusive privilege of conferring degrees, amounts to the enormous sum of 440*l.* 15*s* 6*d.*! As the solicitor employed was the brother of Mr. Lawrence (one of the subcommittee appointed to conduct the business), it is to be presumed that he did not go into any unnecessary expense: but we confess we think, as the amount so very much exceeds what had been anticipated, it is no more than due to all concerned that this should be made apparent.

CLINICAL OBSERVATIONS ON A PARTICULAR KIND OF FIBRO-CELLULAR ENCYSTED TUMORS.

By Baron Dupuytren.

From the "Leçons Orales," published periodically, under the Baron's inspection.

The remarks which we have to make to-day shall be devoted to the subject of those fibro-cellular encysted tumors, so vaguely treated of by authors, and which have been improperly attributed to certain affections of the nerves. Every accidental membranous production, in the shape of a shut sac, inclosing a foreign substance, and developed in the interior of the body by some morbific action, is

called a *cyst*. There are two grand divisions of cysts; one including all those which become organized around a liquid or solid foreign body; the other, such as are formed spontaneously, and previously to the substance contained within them.

Among the substances which may form the nuclei of the first sort of cysts are effused blood, grains of shot, bullets, urinary calculi, fœtuses developed in the fallopian tubes or ovaries, and hydatids. In the second sort, which exist before the matter contained in them, we reckon serous cysts, synovial, melicerie, steatomatous, atheromatous, fatty, mucous, gelatiniform, and a small hydatid tumor, described by ourselves, and which has hitherto been observed almost solely at the articulation of the wrist, on the palmar side; sometimes, though very rarely, in the neighbourhood of the articulation of the instep; but always in connexion with synovial membranes or tendons.

There is a third class in which are ranged fibrous productions, characterized by a dense, whitish, and rather tough tissue,—most frequently exhibiting a linear structure, and many of which are composed of membranous pouches of the fibrous or fibro-cellular accidental kind. It it with this class that the little encysted tumors which we are about to describe appear to have the greatest relationship. They cannot be confounded with either of the preceding classes, in their nature, shape, seat, or results; for they are fibro-cellular, nearly round in their form, seldom larger than a pea, situated for the most part beneath the skin, along the limbs, and terminating in a cancerous softening. At first sight it would not seem very obvious to consider this little tumor, which the eye can scarcely distinguish, as the cause of violent pain, and as the origin of one of the most grievous afflictions of the body—cancer; yet observation warrants us in forming that opinion; and such is the conclusion at which we have deliberately arrived.

Several authors have described pretty accurately the nature of these tumors, but they have supposed them to be found in the tissue of the nerves, or in the course of those organs more particularly. Thus, Antoine Petit, in his essay on Pain, after stating that the ramifying extremities of the nerves are more sensible than their trunks, says,—"The nervous ganglia are very little understood: we see them in the shape of small bodies about the size of a bean, very hard, moveable, colourless,—making their appearance in places which have been injured, and often without any apparent cause; giving rise to cruel pain on the slightest touch, or when smartly moved, or as time may affect them. No application relieves them, and they are cured by extirpation alone. Dissection displays a white tubercle, enveloped in a fibrous membrane, commonly attached to the skin, liberally surrounded by cellular tissue, which is observed to be connected with nervous filaments, of which it is the termination and development. Most of those on which I operated were in the legs; one only was in the arm."

Cheselden, in his Anatomy, after describing the structure of the skin, adds, that he had twice seen beneath the cutaneous integument of the tibia a small tumor about the size of a pea, excessively sensible and hard: from the pain in both instances it was supposed to be of a cancerous nature; but it was cured by extirpation.

Camper follows Cheselden in describing this malady, in his Anatomico-Pathological Demonstrations, book the first:—"It is not unusual," says he, "to notice in the cutaneous nerves hard little tubercles, which are true ganglia, though they do not exceed a pea in size: day and night they occasion lancinating acute pain, and they must be removed with the scalpel. I have frequently met with them in the human subject: they are white internally, elastic, as hard as cartilage, and seated in the tunic of the nerves."

Chaussier, in his Synoptical Table of Neuralgia, thus speaks of them:—"Nervous tubercles or ganglia are seldom larger than a bean, often smaller; oblong, flatted, hard, cartilaginous, whitish, sometimes brownish on their surface, and within. Enveloped in a fibrous membrane, moveable in the cellular tissue, they seem to be attached merely by nervous filaments. The pain which accompanies them is sharp, more or less permanent, and renewed at intervals by pressing on the tumor, by moving the part in any manner, and often without any apparent cause. They are seen most frequently in the leg, but are observed sometimes in the back. They are found in the substance of the skin, or in the cellular tissue, in the track of a nerve. The pains to which they give rise radiate from the tumor as a centre, and extend to a greater or less distance, according to the distribution and connexion of the affected nerve. Excision is the only remedy." Finally, in a Dissertation on local affections, defended, in 1822, before the faculty of Paris, the author says, in speaking of these little tumors, which he calls, as the English do, subcutaneous painful tubercles,—"They are developed beneath the skin; they are ordinarily surrounded by cellular tissue, and not adherent, except by nervous filaments. In other instances, they are situated in the body of a nerve, the filaments of which are distended and spread round them."

Thus we find all these authors speaking of the nervous nature of these tumors, without founding their assertion on any positive fact. Some of them, no doubt, say they have remarked on the surface one or two nervous filaments after extirpation; but they give no anatomical proofs.

By this rapid glance at what has been said on the subject hitherto, you see that the history of fibrous encysted tumors is far from being complete. But repeated and numerous observations have convinced me that they have nothing whatever to do with the nerves. I have dissected several of them with the greatest care in the dead body; and, the better to assure myself of their nature, I have sometimes, in removing them from hardy persons, taken along with them a sufficiently large quantity of cellular tissue, and I have never found the smallest nervous filament adhering to their surface. Their structure is evidently fibro-cellular, a little albuminous, and in time they become scirrhous.

Nor are these tumors, as authors have said, confined to the limbs; I have seen them in the breast. They have the form of grains of wheat, coffee, or peas, and are sometimes oblong; they are also lenticular, flatted, and never larger than a bean; smooth on their exterior, opaque, and hard. If suffered to fall from a certain height on an even and resisting surface, they rebound as an elastic body. Their structure is homogeneous, of a cloudy white colour, without any vestige of cavities or cells; their consistence is fibrous, fibro-cartilaginous, or cartilaginous simply. If the nail be pressed into their substance, a slight crackling is heard: their envelope is opaque, dense, and fibro-cellular, forming a true cyst, which opposes their further growth, giving rise, as it would seem, to the sharp pain felt by the sufferers.

These tumors have never been found affected with inflammation, nor even with redness. The cellular tissue surrounding them presents no unusual appearance. The skin about them is in general sound, without any attachment in the greater number of cases, and preserving its colour: it is, however, occasionally altered in texture, violet, and strongly adherent to their surface, so as to render them immoveable. No nervous filament can be found in their substance, any more than on their exterior: they are, in fact, quite independent of the nerves. Here is an illustrative case.

Case I.—A woman came to show herself to me, complaining of dreadful pains in the cheek, which had distressed her for several years; they were supposed by some to be rheumatic, by others to be owing to sub-orbital neuralgia. Leeches, bleedings, vesicatories, and Meglin's pills, had been tried without benefit. One of her medical advisers had been so confident that the sub-orbital nerve was affected, that he divided it at its egress; but this, instead of doing any good, only aggravated the pain, which had been insupportable when we saw the patient for the first time. Tracing the seat of the pain with the fingers, we felt a small hard tumor, moveable beneath the skin, which was not altered in colour. Pressure on it excited the severest pain. I extirpated the tumor, and the moment I did so the patient was relieved. She has remained quite well ever since.

It is evident that had the tumor in this case been connected with any nervous filament belonging to that branch of the fifth pair which had been cut, the division of the nerve would have removed the pain at once; whereas it only continued the more obstinately, and was relieved solely by the removal of the tumor.

The description given in books of the first stage of cancer, or scirrhus, exactly suits the circumstances of the tumors in question. M. Cruveilhier, in his Pathological Anatomy, says, in treating of scirrhus, that it is formed of fibrous or cellular tissue, penetrated with albumen. These tumors, moreover, become softened; like scirrhus, too, they are painful in the greatest number of cases, while they remain indolent in others.

Case II.—A woman, about 70 years of age, had a small tubercle of the shape and size of a flatted pea, superficially situated beneath the skin, a little above the inner front of the right knee. It was circumscribed and very moveable; and the skin above it was in nowise altered. The patient said that the pains caused by the part were excessive, and made her life burdensome to her. Eighteen years had elapsed since she first noticed the presence of the little tumor; and its volume had not in all that time been increased. It was only during the last eighteen months that she suffered from it. The part was removed, and the pains ceased immediately.

Had this tumor been formed in the track of a nerve, or in its substance, would it have remained insensible for nearly seventeen years? The case is well suited to show the correctness of our opinion on the subject; but there are others equally conclusive.

Case III.—A woman, aged 59, had a small tumor situated immediately beneath the integuments of the anterior of the forearm, just in front of the radius, and about three inches above the wrist. This tumor, which was moderately moveable, and

about the size of a large pea, felt considerably hard to the touch, and exceedingly sensible. The patient, however, only suffered when pressure was made on the part; and when that pressure happened to be made, the pain spread from the part towards the trunk, not towards the fingers. It grew larger insensibly during seven years, and then was stationary for a year. Extirpation was practised on it, by dividing the skin over its centre; and it presented all the characters of an encysted tumor.

The slow and chronic progress of these bodies is explained by their firmness, and the nature of their envelope; and their tendency to *ramollissement*, after a longer or shorter period, affords a new proof of their scirrhous nature. On their undergoing the process of degeneration, even should they be removed, the disorder spreads to the neighbouring lymphatic glands. I removed one, which had been already softened, from the upper part of the arm. In the course of some time the glands in the axilla enlarged, and the complaint was renewed.

The age and sex of the patient seem to have an influence on the development of these tumors. Women are more subject to them than men; and they are most frequently met with in persons of from five-and-thirty to sixty years of age. Their existence is most commonly attributed to blows, or falls, received on the parts affected. In some instances they seem to have been produced by punctures.

Case IV.—A shoemaker pricked his finger with his awl. Not long after he felt a sharp pain in the part, and noticed the growth there of a small tumor. In the course of seven years he suffered more and more acute paroxysms. Caustic was applied in vain; but extirpation was successfully practised; and the patient suffered no more inconvenience. The tubercle was small, hard, of cartilaginous firmness, and contained in a cyst.

On some occasions these tumors have been observed to arise under the influence of rheumatic affection, and to disappear as soon as the person affected is relieved from the principal malady.

Case V.—A medical student slept in a bed which was laid in a damp alcove. In a short time after, he suffered from an attack of inflammation in the joints of his great toe; and presently there grew beneath the skin which covers the internal saphena and the nerve, a hard tumor, about the size of a grain of wheat, which, whenever it was touched, caused a pain like that of an electric shock. The pupil having procured a better sleeping place, was in the course of a few days cured of the tubercle and the neuralgia.

In general the occasional causes of these tumors are very obscure, and most frequently their origin cannot be discovered. Fibrous encysted tumors usually grow in the extremities, and especially in the lower ones. They have also been noticed in the back, the scrotum, the face, and the breast. Seldom are they found otherwise than solitary; when several exist together, they are perfectly distinct from one another. Pain is most usually felt in the affected part long before any enlargement or tumor is observed. Presently the least rubbing from the clothes, or the slightest pressure on the skin, gives rise to darting pains. At the end of a period, which is generally very long, they are detected beneath the integuments, which they sometimes elevate, and then they are readily perceived. Most frequently they are moveable, hard, and the least pressure on them is insupportable. The skin preserves its natural colour in the greatest number of cases. The pains return at irregular intervals in most instances, and are sharp and darting, as in cancer. Those occasioned by pressure are like the effects of an electric shock; they often radiate from the tumor, but this is when they are situated in the neighbourhood of a considerable nervous trunk, and act mechanically. At other times the anguish is continual, and allows the patient little rest; and from want of sleep the general health suffers. When the pains affect the lower limbs, they hinder and positively prevent locomotion. Some irritable individuals during the paroxysms are affected with regular convulsive spasms. I was consulted on one occasion by a young woman who had for a long time a tubercle, about the size of a pea, on the upper and back part of her thigh. She suffered dreadfully (*horriblement*) from the time it made its first appearance; the least pressure on it threw her into convulsions. It was removed, and from that moment all her pains ceased. In numerous cases they remain indolent, and are even unaffected by pressure, during many years.

Diagnosis.—When fibro-cellular encysted tumors are so small as to be invisible, the pains to which they give rise have often been confounded with those from rheumatic or neuralgic affections; and on the latter supposition patients have been tortured in vain with leeches, flying vesicatories, and even more violent remedies. The two women whose cases I shall presently notice, suffered leechings and blisterings along their limbs, although the tumors were very perceptible both to sight and touch.

In neuralgic cases, the pains are sharp, and extend all along the nerves affected; they return generally at stated and regular periods—hourly, daily, or weekly—and pressure has no effect in producing them. But the pains from fibro-cellular encysted tumors do not recur at regular intervals: they are sometimes continual; they do not always extend in all directions; pressure renders them intolerable; and by pressure alone, very frequently, the patients are warned of their existence. It is seldom that several hours elapse without a paroxysm.

The fact of their being called ganglia by some authors, might occasion their being confounded with those tumors which grow in the sheath of tendons, most frequently at the wrist, and which have been called by the same name. But the indolence of the latter—their situation—their mobility during the action of the muscles—their immobility beneath the skin—the existence of a cavity in them, lined with a synovial membrane, and filled with a liquid like that which lubricates the joints—all these characters are fully sufficient to enable us to avoid an error, which, after all, would perhaps be of no great consequence.

Small *lipomas* have sometimes been seen, which, after undergoing carcinomatous alteration, give rise to very severe pains. But their softness, and especially the cellular structure which they present, containing a yellowish fatty matter, like lard at certain points, fibrous at others, serve to afford us the means of diagnosis. MM. Sanson and Begin, in their last edition of Sabatier's *Médecine Operatoire*, mention the case of a woman who was affected with such a lipoma: it occasioned her such pains as materially injured her health.

Finally, it might, perhaps, be more easy to confound these fibro-cellular encysted tubercles with those tumors which affect the nervous tissue, and are called *neuromas*. The latter, however, possess a cavity filled by a substance more or less liquid; whilst the former have neither cavity or cells. Neuromas are capable of attaining a considerable bulk; fibro-cellular encysted tumors, on the other hand, acquire but little volume. Neuromas most usually exist in the great nervous trunks; the others are almost always subcutaneous, and remote from the large nerves: the former are commonly numerous in one place, the latter are in general solitary.

Prognosis.—Where the tumor is moveable, the skin covering it natural in its hue, the situation remote from important organs, such as a vessel or nerve of some size—where, in short, the tumor is simply subcutaneous—the prognosis is favourable. If, on the contrary, it be immoveable, adhering to the skin, which has become violet-coloured, and it begins to soften, we cannot expect a happy issue: for the malady has a tendency, as we have said, under these circumstances, to propagate itself to the lymphatic glands in the vicinity, and the patients soon present all the symptoms which belong to the cancerous diathesis.

Treatment. — Caustics have sometimes been used, with a view to destroy these tumors; but they only favour the softening, while they do not completely remove the disorder. The practice in some rare cases would seem to justify the application of narcotics, in treating some individuals who dread the touch of the knife. A female, sixty years of age, had one of these tubercles in the hinder and inner part of the knee. She would never consent to any operation, notwithstanding the severity of her sufferings. By the steady application of narcotics to the part affected, the pains were assuaged, and they have not since returned.

The surest method, however, and the readiest and the least painful, is extirpation. When these tumors are very small, a simple longitudinal incision, made along the spot which they occupy, will be sufficient. If they be somewhat more bulky—the size of a large pea, for instance—a T shaped incision may be necessary. In either case, we must seize the tumor, after exposing it, with a double hook, draw it to the surface, and then, with a bistoury, separate it from the cellular tissue which attaches it to the surrounding parts. The lips of the wound are then brought together, and kept in contact by means of a little sticking-plaister. If the skin above the tumor be bluish and adherent, both must be removed together. If the tumor be already softened, we must take care how we meddle with it.

I shall conclude this lecture by relating a couple of cases illustrative of the principles already laid down.

Case VI. — Marie Hareng, aged 59, married, and occupied with daily labour, came to the Hôtel Dieu on the 13th October, 1828. She complained of pains, which were sharp and continual, with exacerbations at irregular intervals. Her constitution was good, and she could only trace the existence of this disorder to eighteen months back. Being obliged to expose herself to much cold and moisture, she thought she had contracted a rheumatism; for this supposed malady she had undergone all the methods of treatment which are usually employed. The pains persisted, and were aggravated by the least fatigue. They presented two principal features:—1, Their continual seve-

rity; and 2. their repeated attaeks, reeurring about four times in the four-and-twenty hours, and continuing at each for from a few minutes to an hour. These crises might be brought on by pressure on the parts, or by a blow on a tumor which was situated on the right inner and upper part of the thigh. They consisted in darting and numbing pains, directed from the upper part of the thigh towards the knee. So severe were they, that the woman could not answer when spoken to; she was agitated, she uttered cries, and said she felt the parts torn from her where the pains were situated. On examining the patient, I presently discovered that there was no rheumatism in the case, but that there was present a fibrous subcutaneous body. I removed it on the 20th of October, by making a T ineision over it, when the tubercle was found embedded in fat: it was white, and readily removed with a bistoury. The pains immediately ceased, and the wound was dressed with a little sticking-plaister. The patient left the hospital on the 8th of November, radically cured.

Case VII.—An old soldier, otherwise in excellent health, came to eonsult me, in February last, about a small tumor which he had on the outer and upper part of the right leg, just over the articulation of the tibia with the fibula. He could not reeolleet the origin of the tumor; it was only of a few months' standing; but the pains were of the most acute character: they darted from the tumor towards the neighbouring parts. Having no doubt about the nature of the ease, I removed the morbid part by exeision. The pains ceased, and great was the astonishment of the soldier when he saw what a little substanee had proved such an enemy to his peace.

Case VIII.—Madame P——, the wife of a wine-merehant, suffered exeruciating pains in her right leg for about three years. They came on three or four times a-day, and at last every recurrence of them was attended with syncope. Much medical advice was followed, and all imaginable methods of treatment were employed, in vain. I was at length consulted; when I found on the middle and anterior portion of the leg, on the very shin-bone, a small fibrous tumor, of about the bulk of the kernel of a cherry. I cut down upon it, and pressed it out. It was of a fibrous structure, and enveloped in a fibro-cellular cyst. The pains vanished on the moment. An erysipelas oecurred about the little wound, but yielded to a few laxatives. The lady was quite well in ten days, and has never had the least recurrence of the disorder since.

SURGICAL REPORTS FROM THE LONDON DISPENSARY.

By R. R. Robinson, Esq.
Surgeon to that Institution.

The eases which I am about to describe, I have divided into three elasses.

The first includes affections of the vertebra, in which the disease has been arrested, or death has ensued before the formation of lumbar abscess.

The second includes cases where disease of the spine was met with, in combination with lumbar abscess.

The third eontains two cases of lumbar abscess, in one of which it is *clear* that disease of the vertebræ did, and in the other it is *probable* that it did not, exist.

Case I.—*Disease of the Lumbar Vertebra arrested by Issues, and Position.*

John Farmer, æt. 16½, admitted 28th November; a strumous boy, subject to cough; family consumptive; pain in the back upwards of six years, increased on turning and upon pressure on the loins; has overstrained himself two or three times recently, after which he has always been worse. Has been bled and cupped with relief. His occupation compels him to stoop much. More pain in the loins lately; increased on turning, sitting upright, or lying on his back; most easy when reclining on one side. On tracing the spine, the spinous process of the first lumbar vertebra is much more depressed than the others, and very tender to the touch. A sponge dipped in hot water produces pain here, which it does not elsewhere. The posterior part of the body of this vertebra appears to have been destroyed, and carious. He frequently trips in walking. No palsy, wasting, or numbness of the legs.

Fiat fontana singulis lumbis. P. P. gr. xv. a. a. Horizontal position.

December 10.—Sloughs separated on the 8th. Can move better; freer from pain.

January 28th.—Scarcely any pain since the issue was implanted; can walk without pain, and does so more steadily. Pressure on the diseased vertebra scarcely felt, unless very firm. The issue was very painful and inflamed, from the cold weather, but has become easy by poultice.

Rep. P. P. p. r. n.

March 23d.—Pain entirely gone; pressure produces no uneasiness; can turn without inconvenience in any position; has been able to sit up the whole day for

some time; eame to the Dispensary today; he looks pale, but feels quite well; does not trip; walks firmly; the depression of eourse remains. Diseharged cured.

Remarks.—In this ease the disease had rather an uneommon seat, as it appears to have affeeted the spinous and transverse proeesses, and the bony ring surrounding the medulla spinalis, instead of the body of the bone—the part most eommonly attacked. Counter-irritation and position were very effeetual in arresting the morbid action going on at the time they were eommeneed. The influence of eold in augmenting the pain of an uleer, was also here eonspieuous; an influenee always unpleasant, and sometimes serious.

Case II.—*Disease of the Vertebræ arrested by Position and Issue—Return subsequently—Strumous Ophthalmia.*

Anne Shepperd, æt. 12, admitted May 8th. A healthy-looking girl, naturally weak in her legs, and very liable to trip, (her father is the subjeet of posterior eurvature of the spine). Fell down and bruised her baek last September, after whieh a eonsiderable curve was observed in the baek, but no notiee was taken of it till lately. She eomplains of pain in both thighs, more espeeially the right, and some weakness in walking. On examination, the spinous proeesses of the eighth and ninth dorsal vertebræ are very mueh more prominent than the others; there is also a lateral eurvature, and compression of the ribs laterally. On pressure of the spine, pain is felt every where but in the cervical region; the greatest pain, however, is in the eourse of the lumbar vertebræ, espeeially the projecting ones. A warm sponge produees the same effeet.

Cal. gr. ij.; P. Rhei, gr. xv. alt. auroris. Sodæ Sub. gr. x.; Quin. Sulph, gr.i. ter die. Rest.

June 2d.—Easier sinee she has kept her bed; still pain upon pressing the spine.

Fiat fontana sing. Lumbis.

July 19th. — Issues have diseharged freely; no pain on pressing the spine, exeept for two inches below the projeeting spinous proeesses. Slight strumous ophthalmia of right eye, with pustule at the edge of the eornea.

Lotio frigida. Rep. Med.

August 12th.—Conjunetivitis and pustule soon subdued. Pain entirely gone from the baek; stronger, ean stand without feeling weak, and walk without tripping. Issues withdrawn.

January 5th, 1834.—Has been pretty well until the last few weeks, when she again began to trip in walking; and as she has latterly entirely lost the power of her lower extremities, she is going into a hospital.

Remarks.—This is a much worse ease than the preeeding, in that the disease appears to have been eonfined to a part of one vertebra; in this, several have been implieated; eonsequently, eonsiderable eurvature, both posterior and lateral, has been produeed. The father of this girl being the subjeet of posterior eurvature, shews a peculiarity of eonstitution which may be eonsidered hereditary; without whieh, perhaps, she might altogether have eseaped this terrrible malady.

There can be no doubt that the lateral eurvature had existed long before any spinal disease was deteeted. I think it also more than probable that the disease produetive of posterior eurvature was in existence *long* before its being accidentally diseovered, as the prominenee in the baek was the first symptom whieh indueed the parents to apply for adviee. The insidious eommeneement of diseases of the spine, and the total absenee of pain, often prevent them from being known until they have gone too far to be remedied. I lately witnessed a ease of eonsiderable posterior eurvature in the upper part of the spine, in a boy who had been lying on his baek for a period of eight months, for serofulous disease of the left knee-joint; for whieh iodine had been extensively used.

I beg, therefore, to add my testimony to that of others showing the good effeets of a warm sponge, not only in detecting spinal disease in the early stages, when few, if any, marked symptoms would lead to its deteetion, but also in determining its extent and situation.

Case III.—*Diseased Spine and Intervertebral Substance—Tubercles of the Liver, Lungs, &c.*

Joseph Davies, æt. 2½, admitted 22d November. A strumous child; has never walked; unwell sinee five months old, with eough, dyspnœa, and gradual emaciation: latterly the emaeiation has become extreme. Look exsanguine; head large; abdomen tumid; a slight eurvature of the spine posteriorly, in the situation of the tenth and eleventh dorsal vertebræ. The lower limbs eompletely shrivelled and paralytie.

November 25th.—The eough and dyspnœa inereased, and it died yesterday.

Dissection.—No fat; lungs completely studded with miliary tubereles throughout; a few small vomieæ in different parts; heart healthy. A few similar hard tubereles, with two or three vomieæ eontaining a green mueo-bilious looking substanee, in the right and left lobes of the liver, which, examined through a magnifier, appeared

yellow, gelatinous, and distinctly vesicular. Mesenteric glands and other abdominal viscera healthy. The spinous processes of the two last dorsal vertebræ curved; the body of the eleventh dorsal softened, much compressed in the centre, but not scabrous. The vertebræ above and below, though approaching this state, were considerably firmer; the bodies of these vertebræ very vascular. On making a section of the curved portion of the spine, the intervertebral cartilages were found gelatinous, and exceedingly soft. Medulla spinalis very soft; its membranes very vascular and somewhat thick; a considerable quantity of serous fluid in the theca vertebralis.

Remarks.—In this case there appears to have been increased vascularity of the substance of the bone, with decided softening and deficiency of ossific matter; so that it partook somewhat of the character of mollities ossium. The inflammation had extended to the spine, and produced softening of it; effusion into, and thickening of, the theca vertebralis. It was decidedly a scrofulous disease, which the presence of tubercles in the lungs and liver tends to confirm.

The appearance of the tubercle under the magnifying glass, seems to corroborate the vesicular origin of this substance.

[To be continued.]

WESTMINSTER HOSPITAL.

To the Editor of the Medical Gazette.

Sir,

Should you deem the following cases, out of the many interesting ones which it has lately fallen to my lot to witness under the physicians of this institution, worthy the pages of your valuable journal, an early insertion would oblige

Yours obediently,

Jesse Leach,
Clinical Assistant.

Westminster Hospital,
Aug. 7, 1834.

A Case of Periostitis from Rheumatism.

Elizabeth Long, æt. 32, married, and mother of five children, was admitted into Queen's Ward, under the care of Dr. Roe, on the 4th of June, 1834. Her complexion was dark, her general appearance unhealthy, and her frame emaciated. She complained of great tenderness and swelling of several of her bones, with severe pain at night, which prevented her from sleep. The outer and inferior parts of the left humerus and right femur were much swollen. The posterior part of the head of the left tibia, and a portion of the fibula, were also swelled, and very painful on pressure; but there was no discoloration or redness of the skin in these situations. The uvula and soft palate are absent, and her voice is much affected by it. The skin is cold: her pulse is very feeble, 72; respiration natural; tongue clean; bowels open; appetite not very good; urine high coloured, with furfuraceous deposits. The catamenia have been absent during the period of her illness, twenty-two months; and at the time they should appear she suffers greatly from severe pains in the back and loins, which she compares to the pangs of labour.. She says that at nine years of age she had scarlet fever, which raged with great mortality in the village in which she lived; and from ulceration of her throat, which accompanied it, she lost her uvula and soft palate. Since that time she has enjoyed very good health, until twenty-two months ago, when, after a hard day's washing, she was suddenly seized, whilst suckling her infant of two months' old, with severe pains about the left elbow, increased by heat, and accompanied by thirst, restlessness, heat of skin, and general fever. She was confined to bed for some weeks, and was told her complaint was rheumatism. About two months after this period the pain removed to the shoulder of the left side, and thence to the joints of the lower extremities. The pains of the joints gradually diminished, and the periosteum of the bones of the left arm, leg, and right thigh, became very painful and tender on pressure, and shortly after much swelled. Her health declined so much from continued suffering ever since that attack, that she came to London from Hertfordshire to seek relief.

Balneum tepidum quotidie.
℞ Decoct. Cinchonæ, ℥viij.
Amm. Carb. ʒiss. Cap. ʒss. ter die.
Meat diet.

June 6th.—She complains that the mixture produces spasms of the fauces, and that she cannot take it. The swollen places of the bones are less painful.

Omit. Mist.
℞ Quin. Sulph. gr. ij.; Ferri Sulphat. gr. ij.; Pulv. Opii., gr. ss.; Cap. ter die.

14th.—The tumors are less painful, but she still suffers from her joints. She has pains in the head and loins, without heat of skin or fever. This being the period when the menses ought to appear, six leeches were applied to the pubes, and she was placed in a hip-bath, which removed

the pain in her back and loins, but no menstrual discharge followed. As she suffered much from weakness, she was ordered

Ferri Carbonatis, 3ss.; Pulv. Cinchon. grs. xx. ter die.
Pulv. Rhei, c. Sodæ Carbon. alvo adstricto. Bath to be continued.

From this period she progressively improved in flesh, strength, and appearance. The pains in the joints became gradually less, together with the swelling; and she left the hospital on the 29th of July, in good health, being freed from every vestige of periosteal enlargement.

Eczema Rubrum, with Iritis.

Mary May, æt. 26, unmarried, was admitted under the care of Dr. Roe, June 12, 1834, in a state of great debility.

Her face was swollen, and covered with yellow incrustations, underneath which the skin is inflamed, and discharges a white ichorous matter, which excoriates the parts over which it passes. Parts of the arms, hands, and legs, present the same appearances. The eruption seems to begin in small transparent vesicles, the serum in which is at first clear, then becomes turbid, and is discharged. She complains of pain, heat, and itching, in the affected parts. The corneæ are surrounded by a zone of red vessels, but the conjunctiva is white elsewhere. The irides are of a muddy colour, very irregular, and adherent to the lens in some points; her sight is much impaired, but she has no pain in her eyes. Her gums are red and spongy; teeth carious; tongue and fauces are very red; breath offensive. Her skin is cold and bloodless in appearance; pulse extremely feeble, 70; bowels open; appetite very bad. She complains of severe pains in all her larger joints, and down the tibia, but without swelling; aggravated at night. She says that ten weeks ago she was seized with pains in all her limbs, particularly the larger joints of the extremities, increased by warmth, and attended by copious perspiration, which confined her to bed three weeks.

In about a month after she had a similar attack, with the addition of pain and tenderness in the course of the tibial spines. Shortly before this attack, she had had sores about the organs of generation, sore throat, and an eruption, the precise character of which is not known. Mercury was administered with slight relief to the pains of her bones, until free salivation was produced, when the symptoms of iritis and the present eruption manifested themselves. The catamenia have been absent two months.

Balneum tepidum; Haust. Cathar. st.

℞ Hyd. Potassæ, 3ss.; Pulv. Iodinæ, gr. j.; Aquæ Dist. ℥viij. Capt. ℥j. ter die.

The eyes were covered with a single fold of linen, wetted continually with warm water.

June 14th.—The eyes are less inflamed, with a great diminution of the dinginess of the iris. Vision, which was before very imperfect, considerably improved; eruption less inflamed, and diminution of itching.—Perstet.

16th.—The state of the eyes better; eruption vanishing, but more constitutional disturbance. Tongue and fauces morbidly red; no pain or tenderness over the abdomen.

17th.—Was seized with a shivering this morning; much agitation, preceded by pain in the head, and bilious vomiting; tenderness over the region of the stomach and bowels. One stool this morning.

Med. omit. Cataplasma sinapis pedibus, et fotus calidus epigastrio.

18th.—Passed a good night, being relieved by the hot fomentations. There is a violent agonizing pain, increased by pressure, over the whole abdomen, and more particularly in the epigastrium, with a constant bilious vomiting; the lips are swollen, dry, and cracked at their angles; the mouth and gums painful; the tip and edges of the tongue evince much gastric irritation; the head is affected, and she is continually moaning from the suffering she is under. Thirst great, with a burning hot skin; pulse 144, quick and feeble. The bowels have been once moved.

Ol. Ric. ℥j. st. Hirud. xl. abdom.

June 19th.—The pain and tenderness are gone, and her countenance has lost that anxious appearance observed yesterday. Pulse 120. The eruption and heat of skin much improved; the cuticle desquamating, leaving the surface underneath not so raw. Tongue moist, with a diminution of the redness; bowels tolerably open.

Rep. Ol. Ricin.

20th.—Symptoms much relieved; bowels freely open; eyes healthy.

July 2d.—Her countenance is now blanched and exsanguineous; pulse quick and soft; veins loaded with a leaden, sluggish circulating fluid. She was now ordered to take a combination of iron and iodine, in order that, whilst the iron improved the tone of the circulation, the iodine might remove the scrofulous diathesis and cuticular disease.

℞ Ioduret. Ferri, gr. ij.; Inf. Armor. ℥j. ft. haust. ter die sumendus.

Under this treatment her appetite is now good; cuticular disease disappeared; the pulse reduced to 90, quick and soft; her cheeks are resuming their wonted youthful hue, and she has suffered a progressive convalescence, and will leave the hospital in a few days.

Dr. Roe, in his clinical lectures on this case, observed, that the extreme debility of the patient, the feebleness of her circulation, and almost bloodless whiteness of her skin, rendered further depletion altogether inadmissible, even for the cure of iritis; that the inflammatory affection of the eyes was of that slow and inactive kind, that the restoration of her general health, with the soothing application of warm water, would have been sufficient to remove it.

Hypertrophy with Dilatation.

John Healey, æt. 13, admitted into Luke's ward, under Dr. Roe, May the 7th, 1834. Countenance pale and anxious, and whole frame emaciated from long illness. The heart's action is very violent, and heard over the whole anterior and lateral parts of the thorax; the contractions of the left ventricle are particularly loud and forcible, and may be distinctly seen and counted betwixt the fifth and seventh ribs on the left side, giving considerable impulse to the stethoscope when placed in that situation. The pulsations of the right ventricle may be distinctly seen and counted in the epigastric region, underneath the inferior extremity of the sternum. The contractions of the auricles may be heard underneath the clavicles, and their sounds are duller than natural. The carotids and labial arteries may be seen pulsating, but no particular noise in their course is heard, or pain complained of. Pulse 120, moderately full, and compressible; respiration hurried and oppressed, but there is no evidence of pulmonary disease, although he is chicken-breasted. His digestive organs are healthy; tongue clean; bowels regular; urine pale, and too copious. The boy states, he has been subjected to much hardship and close confinement in a button-manufactory. It appears that he laboured under acute rheumatism of the shoulder and arms some time ago, and that the present symptoms succeeded. Digitalis was given to him, in conjunction with colchicum, for five days, without any benefit resulting, when he had an attack of hospital fever. Eight leeches were applied to the epigastrium, a purge was given, and he took $\frac{1}{8}$ gr. Ant. Tart. 4tis horis.

June 15th.—The fever has entirely disappeared.

℞ Acid. Hydrocy. ♏j.; Ferri Sulph. gr. j. 4tis horis.

19th.—His ankles swell towards evening; the heart's action is not so violent. The pil. scillæ c. digitali et calomelane, was ordered, and the iron and acid discontinued, and he was ordered to rub in the veratrine ointment *bis die*, without any benefit.

24th.—The heart is still beating forcibly, elevating the thoracic wall; the pulse is rapid and thrilling. The pressure betwixt the fifth, sixth, seventh, and eighth ribs gives great pain; and if the pressure be exercised from the base of the sternum upwards, the same degree of pain is produced, and a sense of immediate suffocation succeeds. He cannot lie at all on the left side. The pulse is rapid and jerking, 110; breathing hurried and oppressed (36 respirations per minute); no headache, and but little fever; passes sleepless nights.

℞ Acid. Prussic. ♏ij.; Vini Colch. ♏x. 4tis horis.

Alternating with—

Ferri Sulph. gr. j.; Opii Pulv. gr. $\frac{1}{4}$, bis die.

July 28th.—The boy's health continues to improve; his heart's action greatly diminished; his breathing natural, and general health good. He left off taking the acid and colchicum on the 26th, and now continues the steel preparation, with opium, four times a day. He can now lie on either side, or on his back, equally well, but still prefers being supported by bolsters. We allow him the open air, and gentle exercise, and animal food.

Remarks.—It is extremely probable that a metastasis, during the period of the rheumatic fever, took place to the pericardium; some adhesions might form from effused coagulable lymph; and that the walls of the heart were thickened, and afterwards dilated, from contiguous inflammation. Had much serous effusion taken place, I presume there would have been more irregular, intermittent, and less impulsive action. I have watched this patient since his entrance into the hospital, and during that period I have not observed a single intermission; but, as I know from a post-mortem, which I performed on a dropsical subject last Saturday, the heart's action is not always so irregular or feeble. In this case the heart's action was very impulsive; pulse full and jerking, even to the hour of his death; and yet there were five ounces of serum in the pericardium, and the heart considerably enlarged.

It would be superfluous to give other detailed histories of the efficacy of iron. Suffice it to say, from the extensive use of

this drug under the physicians of this hospital, I should be fully warranted in saying its use is applicable whether there be organic or functional disease, provided there be no accompanying fever or hardness of pulse with the following symptoms: the body may be extremely emaciated, as in the case of Healy, May, and Long; or the integuments have a puffy, flabby feel; the superficial veins have a leaden sluggish character, evincing great inequality in the venous and arterial systems; the conjunctiva has a pale glistening appearance; the skin is remarkably smooth and delicate; the pulse is quick and soft; the digestive organs are more or less impaired, and the patient has generally a voracious desire for liquids. There is sometimes lethargy and inactivity; and if blood be abstracted, there is a larger proportion of serum, and a small inconsistent crassamentum. In some individuals there is a tendency to congestion of the most predisposed organ, and more especially of the bronchiæ, brain, and liver; and should the timely exhibition of iron in these stages be neglected, anasarca, with a diminution of urinary excretion, follows; and in one instance, as related in the case of Healy, there was extreme emaciation, with an increase of urinary excretion. How far this may go, in the treatment of the first stage of diabetes, before the kidney becomes a mass of disease, I have had no experience.

As a beautiful instance of the tendency to anasarca and congestion of the chest, for want of this remedy, I may mention the case of a house-servant of this institution, who has for months past pleaded incapacity of holding her situation for want of bodily strength —much exercise causing oppression at the chest, attended with a slight cough and stitches; her skin is thin, and possessing the above characters; her habit is full, and muscles flabby, and do not forcibly contract; there is a sparing flow of urine, with a swelling of the ankles towards evening; her appetite is much impaired, and she would gladly shun every other diet but tea. We first ascertained by stethoscopical means, &c. that there was no inflammation present; debarred her of every liquid; allowed her as much animal food as she could take, which at first was rejected; gentle exercise in the open air; and gave her the Pil. Fer. c. c. Inf. Gent. c. three times a-day, the effect of which was that in less than a fortnight her appetite materially improved. She is much thinner, and the integuments are more tense; the muscles have a more forcible contraction, and the urine is voided in larger quantity; she is less lethargic, and a great diminution in the oppression at her chest since she commenced the use of iron. There is one practical fact to be held in remembrance in its exhibition in some constitutions: after giving it for some time, epigastric pain is complained of, with nausea, and a febrile state of constitution, and the stools are black; a few days discontinuance of its use will remedy all these symptoms, and it may then be given again. It is a matter of choice in what form or combination it be given—existing circumstances must alone direct the choice.

MANCHESTER EYE INSTITUTION.

Case of Accidental Cataract—The Retina sound, with Paralysis of the Iris.

[Communicated by Mr. Walker.]

John Partington, æt. 27, admitted an out-patient of the Manchester Eye Institution, July 15, 1834. He stated that he met with an accident about four months since. In the act of firing a gun, the cap flying off, he was struck over the left eye. There was a slight cut over the eye-brow, which healed in two or three days. But little pain followed the injury, and the sight of this eye was not much affected for the first week or two, but it has gradually failed since. He can now discern how many fingers are held up before him, and can see objects dimly around him, when the sound eye is closed.

On examining the organ, the iris is observed to be perfectly motionless, and the pupil very large, and uninfluenced by variations of light. The crystalline lens is opaque throughout, and of a pearly white. In other respects the eye is sound.

This case may be considered instructive, in explaining the distinct and independent functions of the retina and iris; the latter is completely paralysed, whilst the former is evidently unaffected, vision being as good as in ordinary cases of cataract.

CASES OF NEURALGIA,

With Remarks.

By W. A. Gillespie.

It is not, perhaps, sufficiently well known to the members of the medical profession, that tooth-ache, and what is vulgarly called jaw-ache, are frequently of neuralgic character, and of miasmatic origin *. Many cases of this kind are submitted

* M'Culloch, Bell, &c.

to the manual dexterity of some neighbouring mechanic, who extracts tooth after tooth, until at length the disconsolate sufferer, without experiencing the least relief, frequently abandons himself, in despair, to the most excruciating agony. Several cases have come under my observation, in which perfectly sound teeth have been removed, and often several in succession, in consequence of no relief being afforded from the extraction of the first. In addition to this, tooth-ache drops of various kinds, many of which contain mineral acids, are used by the tormented sufferer in profusion, to the eventual destruction of his few remaining instruments of mastication. I am by no means opposed to the extraction of teeth, in proper cases, with due discrimination and reference to collateral circumstances. Carious, useless teeth, should always be removed; but under scarcely any circumstances would I be disposed to remove a sound useful tooth. When odontalgia is produced by slight caries, complete success has been known to follow the mere starting of the tooth from the socket by the instrument, and immediately pushing it back into its natural position, thus destroying its nervous connexion, whilst the tooth adhered firmly to the socket, and answered, in a good degree, the purpose for which it was intended. The following cases exemplify the success of quinine in neuralgic odontalgia. Notwithstanding the labours of many late writers, the pathology of neuralgia is still involved in much obscurity; but I believe no remedy, as a general one, is preferable to the sulphate of quinine. Many cases that were formerly termed rheumatism, ought manifestly to be classed with neuralgia; it is in this way only that we can account for the many high encomiums passed on the Peruvian bark, as a remedy in acute rheumatism, by Drs. Hugh Smith, George Fordyce, Haygarth, &c.

Case I. July 29th.—Mrs. S. had been subject to violent toothache, or jawache, as she termed it, for three months; during this time she had applied to a mechanic, who had extracted at different periods, three sound teeth. No relief being afforded, it was thought proper to have professional aid, and accordingly I was called to the patient a few hours after the extraction of the third tooth. It was one of the large molares of the upper jaw, perfectly sound, and its removal had had no effect on the pain, which was of that lancinating, shooting character, too intolerable almost to be borne, at least not without the greatest fortitude. Upon inquiry, finding the pain to have regular periodical exacerbations, and knowing the patient to be exposed to the miasms of a marsh which had produced in the same season some violent cases of remittent and intermittent fever, I viewed the disease as of miasmatic origin, and prescribed sulphate of quinine in free doses. There was in a short time a suspension of the paroxysm, and the patient, on account of the disagreeable taste of the quinine, which was given in solution, discontinued its use.

August 15th.—The paroxysms of pain had now returned more severe than usual; I was again called to the patient, and again prescribed quinine, which was not taken as directed, and on the 20th I saw her again. I now prepared some pills of quinine, and represented to the patient the absolute necessity of taking them constantly and regularly; and predicted with confidence almost certain relieve from her excruciating torments. She now took the medicine as prescribed regularly, and had the satisfaction of informing me, during the same week, that she had escaped the periodical returns of her distressing complaint, and was now well satisfied that the medicine had controlled it. She has since had no return of it.

Case II. September 28th.—I was called to Mrs. K., residing in the immediate vicinity of the foregoing case, and exposed to the same miasms. She had suffered for two months with tooth-ache, as she termed it, and had had two teeth extracted, both perfectly sound, with no relief; and having heard of the preceding case, she now concluded her's to be a similar one; which I found, on examination, to be the fact. Quinine was prescribed as above, and in a few days entire and perfect relief followed. Fomentations, blisters, and purgatives, had previously been used in this case, with no perceptible relief.

She has since had no return*.

LIGATURE OF THE SUBCLAVIAN ARTERY BELOW THE CLAVICLE.

A young man received a sword-thrust through the folds of the axilla, in a duel. The hæmorrhage was checked by compression, and in eight days the wound was nearly healed; but now unfortunately the bleeding returned, and, although restrained for the time, broke out afresh at different intervals. Professor Blasius, of Hallé, determined therefore to tie the subclavian artery, below the clavicle. The operation was performed on the twentieth day after the accident; and, although no particular difficulty was experienced in any of the steps, the patient had been so exhausted

* American Journ. of Med. Sciences.

by the repeated losses of blood, that he died on the second day after. On dissection, the axillary artery and vein were found uninjured; the source of the bleeding had been from the circumflexa humeri posterior, and circumflexa scapulæ, the wound having penetrated from behind, through the tendon of the latissimus dorsi, upwards and forwards. The subclavian artery, at the point of the ligature, was well secured.

Dr. B. very correctly condemns, in severe terms, the early treatment of this case. Why was the artery not laid bare at once, and a thread passed round it? Dr. B. was called one evening to a young man, who had wounded his hand deeply in the morning; a bungling surgeon, who had seen the patient then, had crammed compresses and other trash into and upon the wound; a certain degree of inflammation had thereby already commenced, when Dr. B. applied the ligature. On the fourth day the vessel had ulcerated; the bleeding returned; and a second operation was necessary. But should the wound heal partially at first, and the hæmorrhage not recur till the 16th, 18th, or 20th day after the accident, when suppuration had been established for some time, not only are the difficulties of securing the injured vessel greatly increased, but also the chances of ulceration of its coats at the site of the ligature, and consequent bleeding. The parts are much changed in their tissue, and are matted together, so that it is often not easy to distinguish between them; and moreover the artery is so glued to its sheath, &c., that it is scarcely possible to isolate it satisfactorily. Still, with all these disadvantages, the tying of the artery is much safer than the employment of any other styptic remedies; our prognosis, however, cannot be so favourable as it would have been after an earlier operation *.

COLLEGE OF SURGEONS—HONORARY MEDAL.

We are happy to learn that the Council of the Royal College of Surgeons have decreed the gold honorary medal to our accomplished correspondent, Mr. George Bennett, F.L.S. &c. for his discoveries in Animal Physiology and observations on Natural History.

Preparing for Publication. — An Essay on the Constitution of Self-Supporting Dispensaries, and their Utility to the Working Classes and to the Medical Profession. By Charles B. Nankivell, Esq. Surgeon to the Coventry Self-Supporting Dispensary.

* American Journal of Medical Sciences.

WEEKLY ACCOUNT OF BURIALS,

From Bills of Mortality, *Aug.* 12, 1834.

Abscess	4	Fever, Typhus	3
Age and Debility	30	Gout	1
Apoplexy	7	Hæmorrhage	4
Asthma	13	Heart, diseased	2
Cancer	5	Hooping-Cough	9
Childbirth	4	Inflammation	29
Cholera	44	Bowels & Stomach	16
Consumption	64	Brain	5
Constipation of the Bowels	1	Lungs and Pleura	1
Convulsions	29	Insanity	2
Croup	3	Liver, diseased	6
Dentition or Teething	9	Measles	11
Dropsy	20	Mortification	7
Dropsy on the Brain	11	Paralysis	1
Dysentery	1	Small-Pox	11
Epilepsy	1	Spasms	4
Erysipelas	3	Thrush	3
Fever	4	Tumor	2
Fever, Scarlet	8	Stillborn	31

Decrease of Burials, as compared with the preceding week . . . 37

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

Aug. 1834.	Thermometer.	Barometer.
Thursday . 7	from 48 to 70	29·86 to 29·84
Friday . . 8	50 72	29·80 29·78
Saturday . 9	51 73	29·84 29·91
Sunday . . 10	49 75	30·01 30·09
Monday . . 11	54 75	30·15 30·16
Tuesday . . 12	54 79	29·99 29·92
Wednesday 13	54 82	29·91 29·96

Wind S.W. till the 11th; since, S. and S.E.

The 7th and 8th cloudy, with frequent heavy showers. The morning of the 9th and 11th cloudy; otherwise generally clear.

Rain fallen 1 inch and ·075 of an inch.

The innumerable meteors which are nightly seen shooting in all directions are worthy of notice.

Charles Henry Adams.

NOTICES.

"Y. Z." We are not aware of any other subscription than the one through which a beautiful monument was erected to the memory of Baillie, in Westminster Abbey, some five or six years since: eight hundred guineas were expended on it. See an account of the bust, inscription, &c. in the Med. Gazette, vol. i. p. 135.

"J. B." The objections of our correspondent to the theory of Atoms have been made over and over again: the atomic theory is very far from being unequivocally established; but J. B., we think, would do well to confine himself to the doctrine of *definite proportions*, regarding which he cannot possibly be so very sceptical.

We cannot insert the hue and cry caution of "J. R."

Dr. Favell's circular is unsuited for our pages.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, AUGUST 23, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XLVI.

COMPLEX LABOURS.

RETENTION OF THE PLACENTA.

The placenta may be unduly retained in utero by three different causes, each acting separately, or two in concert. They are, *first*, atony of the uterus; *secondly*, spasmodic or irregular contraction of the uterine fibres; and *thirdly*, morbid adhesion having taken place between the placental and the uterine surfaces.

Retention from atony of the uterus.—We generally observe that when a want of due and sufficient energy on the part of the uterus prevents the proper contraction of its fibres, for the purpose of expelling the placenta, it occurs in cases where the woman has had a number of children—where the uterus has been acting feebly during the previous stages of the labour—where a long interval has occurred between the expulsion of the head and the passage of the shoulders: after lingering labours also; and in cases where the patient has been delivered by instrumental aid, in which the uterus has become worn out, and the powers of life much depressed. It is equally likely to happen if, when the head is expelled, the attendant has suddenly, forcibly, and improperly, extracted the fœtal body from the uterine cavity, after which reprehensible interference the womb is left in a flabby, relaxed, and torpid state, disinclined to continue its active contractile efforts for the expulsion of the placental mass.

We know that the placenta is in utero by observing, on the application of the hand externally, that the organ is larger than it should be if emptied; by feeling that no part of the placenta, or only a small portion of it, is protruded into the vagina. But do we know why it is retained in the uterus?—Can we tell which of the three causes I have mentioned is in operation? We cannot discriminate *positively*, except under the introduction of the hand into the cavity itself; but our *suspicions* as to the true cause may be strong, and probably correct. We may presume that atony is the cause, if, after the birth of the child, the uterus remains soft, large, and flabby; if there be no after-pains; if, when we take hold of the funis—and this is a good indication—we find that the vein is not full, that it is quite flaccid; because, if the placental mass be squeezed by the uterus contracting upon it, the blood will be forced down from the placenta into the funis, under which action the vein becomes turgid and distended: and we may frequently observe it twist in a trifling degree, or writhe spontaneously, somewhat like an eel, as often as a fresh contraction occurs in the uterine parietes. This twisting is produced by the blood passing gradually downwards, along the vessel, which is seldom straight, but almost invariably following a spiral course.

Where the placenta is retained by atony of the uterine fibres, the blood is, generally speaking, poured out in a copious stream, provided the organ be separated from its previous attachment; because the uterus being uncontracted, its vessels continue large; and their open orifices are not plugged in the least degree, as occurs when the womb has closed itself strongly around the mass retained within its cavity.

Treatment.—What method, then, shall we adopt under this state?—Are we to re-

move the placenta immediately hæmorrhage shows itself; or can we stimulate the uterus to contraction, so as to induce it to throw off the mass without the necessity of so harsh a proceeding? By pressure, friction, and the application of cold, we may frequently excite such efficient action that the placenta will gradually descend into the vagina, and the introduction of the hand be rendered unnecessary. But we must always bear in mind, that these means ought not to be trusted to exclusively and entirely, under a continuance of copious discharge; and that frequently the manual removal of the placenta from the uterine cavity itself, will alone check the flow, and place the patient in a state of safety.

If, under a retention of the placenta from atony of the uterine structure, there be little or no sanguineous appearance, and no disposition to faintness supervene, fifteen or twenty minutes may be allowed to pass without any artificial means being used to solicit the renewal of uterine action. On the expiration of such a period, pressure may be applied to the uterus by the hand placed externally; or gentle friction may be made over the hypogastric region. Should an unusual discharge of blood now take place, cloths dipped in cold vinegar and water may be suddenly laid upon the lower part of the abdomen and the vulva, and the pressure and friction persevered in; and should the discharge continue to an alarming extent, or increase to a profuse hæmorrhage, the removal of the placenta must at once be undertaken. All other considerations must give way to procuring an emptied and *contracted* state of uterus; and that can only with certainty be accomplished by the withdrawal of the placenta. Many a woman has fallen a victim to the timidity of her attendant; many a life has been sacrificed by the trial of trifling means, perfectly inadequate to the production of the grand end proposed—the contraction of the uterine parietes, the evacuation of its cavity, and the perfect closure of its vessels. I should have but little faith in the efficacy of cold water injected into the uterus, while the placenta was retained in the cavity, although strongly recommended by Gooch. It may be useful in floodings, after the placenta is expelled, but even then can by no means generally be resorted to, because the necessary implements may not be at hand. And I should have still less faith in emptying the umbilical vessels of their blood, with the hope of diminishing the size of the placenta, as suggested by some continental physicians; or in injecting the umbilical vein with vinegar, or other astringents, as practised by others: the great objections to all these measures being, that while we are employing them, the blood is gushing from the uterus, and the patient is dying; that we are uncertain whether morbid adhesion may not exist at the same time, in conjunction with deficient energy in the uterine fibres, which may eventually require manual separation; and that the introduction of the hand is the strongest provocative to uterine action of any means we can resort to. The same objections apply to the use of the ergot, which, although it may have the power of exciting contraction in the uterine fibres, requires some time for the establishment of its action; and, if the placenta were adherent, must fail in bringing about its expulsion.

When the necessity, then, for the removal of the placenta is apparent, and we dare no longer trust to more mild and less powerful agents, the operation must be undertaken in the following manner:—

The patient lying on her left side, conveniently near the edge of the bed, we must take off our coat—as in all cases where it becomes necessary to introduce the hand into the uterus;—denude the left arm and grease it; then kneeling down by the bed-side, we bring the fingers into the form of a cone, twist the funis umbilicalis two or three times round the first and second fingers of the right hand, to give us a guide to the placenta, and quietly insinuate the left into the uterus. There is little or no difficulty in passing it through the external parts, vagina and os uteri, if the operation be undertaken within an hour or two of the child's birth; nor is there any difficulty in introducing it fully into the uterine cavity, because the parietes are in a flaccid condition, and the cavity itself is both considerably distended, and readily dilatable.

The removal of a placenta from the uterus, indeed, retained by simple inertia, is one of the easiest operations in surgery; but the condition requiring its adoption is of a highly dangerous character; and the danger will be in proportion to the facility with which the organ admits the hand. The danger, then, is not that we should bruise or lacerate its structure, or dispose it to inflammatory disease, but that we should leave it in an uncontracted state after the withdrawal of the placenta, and consequently subject the woman to a continuance of the hæmorrhage. It certainly far more frequently happens, that the stimulus of the hand causes the uterus to act, and that in contracting it expels the hand and placenta together; and this is a fortunate occurrence; it is to be hailed as the best proof of safety.

Whenever we are compelled to resort

to manual extraction, we must bear in mind, that previously to the introduction of the hand, we cannot tell in what situation we may find the placenta; it may be entirely thrown off from the uterine surface, and lying loose in the cavity; or it may be partly separated, and partly attached; or it may be partially, or through its whole extent, morbidly adherent. For these reasons we must not always calculate on meeting with so easy a case as I have just described; we must not suppose that all we have to do is to introduce the hand and take away the placenta. If we act in this way, we may find the case much more difficult than we expected; we may lose our presence of mind; we may withdraw our hand in doubt and disappointment, cause a serious aggravation of the flooding, and increase the previous peril. Let us, then, before operating, make up our mind to have to encounter the most difficult of all the cases of retained placenta; and should we find it more easily managed than we anticipated, our error, if it be one, is on the right side.

I will suppose that it is partly attached, but not morbidly adherent. We pass the left hand gently into the uterus, guided by the funis; and on its introduction place the right hand between the woman's thighs, to steady the uterine tumor externally; for being much smaller than it was before the birth of the child, the parietes of the abdomen do not support it, but it rolls about in the abdominal cavity, impeding our endeavours at removal. When the hand has fully gained possession of the cavity, we tear the membranes with our fingers, and passing them between the placental and uterine surfaces, run our hand all over the maternal face of the placenta, to be assured that we have got the whole organ within it, grasp the uterus externally with the right; and most likely a contraction will occur; under which we may quietly withdraw our hand, retaining the placenta within its hold. Should, however, this desirable action not supervene, we may keep the hand a short space within the cavity, and endeavour to ensure contraction, by gently moving our fingers, so as to irritate the parietes.

On the entire withdrawal of the mass—whatever may have been the cause obliging us to have recourse to its removal manually—we must never forget to examine whether or not it be entire; for it is possible that, from its softness, a portion may be broken off with the slightest handling, and be left behind. It is possible that adhesion may have taken place between a part of its structure and the uterus itself; that instead of passing the hand over its whole face, we may have broken it, and brought away only a part, leaving the remainder in the uterine cavity. To assure ourselves that we have removed it all, we must lay it upon a napkin with the maternal face upwards. If there be a large portion wanting, we cannot be deceived; we see the mass broken, and we observe the cavity from which a piece has been separated. From the neglect of this very simple proceeding, I have known many cases of great danger occur, and one particularly of which I will give you the outline. A medical friend called me to his assistance on the appearance of violent hæmorrhage, after, as he believed, the placenta was removed. Immediately I placed my hand on the abdomen, I felt satisfied that the whole, or principal part of it, was still within the uterus; but on inquiry, was informed that it had come away on the application of the slightest traction possible. On requesting to inspect it, an utensil was brought which it was supposed contained the placenta. There was the funis entire—there were all the membranes—and there was a large mass that looked like the placenta lying below the membranes. On turning it up, however, no part of the placenta was there. The cord and membranes had slipped away from their attachment to its body, and a large quantity of blood had collected within the membranes and there coagulated, which was mistaken for the placenta itself. If, instead of being satisfied with the appearance of the funis and fœtal membranes, my friend had made his examination, as I have just recommended, the mistake could not have happened; and the cause of the continuance of the hæmorrhage would have been at once apparent. If, then, we find that a large portion of the placenta be wanting—such as a third, or a quarter—let us immediately introduce the hand a second time; for it is much better to remove the disruptured portion than to leave it to be thrown off by nature. This should be done before the uterus is contracted around it; and if much difficulty be experienced, we must desist from our attempts. But if there be only a number of small filaments left, like these pieces adhering to this uterus [presenting a preparation], it would be madness to make any exertion for their removal; since we must put the patient to much pain, run the risk of doing permanent injury, and in the end, most likely, not accomplish our object.

Retention from irregular contraction.—The second cause of retention is the irregular contraction of the uterine fibres. This generally happens after the uterus has acted violently, when the child has been very rapidly expelled, its whole body being projected forth probably by one pain, and

under the same action the organ has contracted strongly around the placenta; or where improper attempts have been made to remove the placenta from the uterine cavity by pulling and jerking at the funis. In this case, then, either all the fibres shorten themselves simultaneously, or some are in a contracted state while others are dilated; instead of the action being regularly progressive from the fundus downwards. Upon this occurrence taking place, two or three strong pains will generally follow each other in rapid succession, soon after the expulsion of the child; and sometimes they are almost as severe as those experienced before the perfection of the birth. If the uterus act strongly in this way, while the placenta does not descend within reach of the finger, if the funis umbilicalis become full and turgid with blood, and if the uterus feel very hard, as well as large, to the hand externally applied, these symptoms are suspicious of the state I am describing. Generally speaking, under these cases there is not such violent hæmorrhage as when atony is the cause of delay; and some time may frequently elapse without there being such a degree of flooding as to induce us to remove the placenta.

Let us, however, not wait longer than the limit before assigned—one hour and a half; and in the meantime we may consider whether we can, by any internal medicines or outward applications, overcome this spasmodic state. External means seem of little service, and of all medicines, opium, perhaps, is the only one which can procure the relaxation sought. It is, indeed, very generally recommended under this state. Opium in moderate quantities, I should not object to; but I have a decided aversion to its employment in large doses; because its influence may be greater than we anticipated—the opposite state to that previously existing may be procured; the uterine powers may be paralyzed; and, although the difficulty in the removal of the placenta may vanish, the contractions necessary for the ultimate safety of the woman may never be resumed. Again: it is very possible that adhesion may co-exist with this irregular action; and if such be the case, the introduction of the hand will eventually be required. Still greater objections apply to the abstraction of blood by the lancet, for the purpose of relaxing this spasmodic contraction — a means which Ingleby tells us has occasionally been resorted to; but which, he states, "will rarely be found admissible, except in the instances of plethoric women and in the absence of hæmorrhage." I should strongly deprecate such a practice while the placenta was detained in utero after the child's birth, even although there might be no flooding; dreading the probability of a copious discharge from the uterus, so long as that organ remained unemptied and uncontracted. Should apoplexy or convulsions, indeed, occur immediately on the child's expulsion, bleeding would be indicated, and it might be highly proper to open a vein, even before any part of the placenta passed into the vagina; but I am now speaking of venesection as a means of overcoming that spasmodic state of the uterus which prevents the placenta descending.

On the other hand, I should equally object to the use of those means which will increase the tone of the uterus—such as ergot of rye. In my own practice, indeed, I am in the habit of relying only on the careful removal of the placenta by manual operation;—the indications being lapse of time on the one hand, and flooding on the other.

Irregular contraction is of various kinds. Sometimes the uterus contracts globularly on the placenta, sometimctimes longitudinally, assuming somewhat the shape of a sugar-loaf; at others, it contracts with a corner, so that in one part or other there is a sac, in which the principal bulk of the placenta is retained; the other portions of the uterus being in a relaxed state. Sometimes it contracts with a sharp ridge anteriorly, something like a hog's back; but this is rare. At others, again, the central fibres of the body of the uterus act powerfully, leaving those of the fundus and neck uncontracted, and the hour-glass state is produced; the placenta being prevented from descending, by the constricted ring formed by the circular fibres of the body. We hear much of hour-glass contraction of the uterus, but my belief is that there is no rarer case in midwifery than the real and true hour-glass contraction, such as I have described. Professor Burns, indeed, states, that "in almost every instance, this contraction takes place; that he scarcely ever introduced his hand into the uterus, in a case of flooding, without meeting with it, whether the placenta had or had not been expelled." Mr. Burns' authority is great on all subjects connected with the obstetric department of medicine, but in this sentiment I can by no means concur; and I am certainly not singular in my opinion. I can scarcely suppose the Professor himself could be mistaken: but I am almost persuaded that the general idea of the occurrence being so frequent, has its origin in error:—that the contraction of the uterus, indeed, is of the globular kind; that its whole cavity is considered the upper chamber; the os uteri being taken for the constriction of the central fibres of the body, and the dilated

vagina,—having in it a coagulum of blood,—for the lower chamber. In many cases I have been told that an hour-glass contraction existed, but, when I came to examine for myself, I found it was of a mere simple globular kind. Out of a great number of cases, indeed, in which I have been called upon to remove the placenta, I do not recollect to have met with more than one instance, which perfectly agreed with my idea, of the true hour-glass contraction.

There is less danger of flooding in irregular contraction than when the uterus is in a state of atony, but the operation of removal is both more dangerous and difficult, because of the resistance necessary to be overcome: and in proportion to the strength of the spasm will be the probability of injury.

Treatment.—Since, then, there is so much more chance of injuring the uterus, it behoves us to be so much the more cautious in our proceedings. If there be no flooding, we may generally wait an hour and a half from the birth of the child; and in the interval, we may administer small doses of laudanum occasionally; but if hæmorrhage come on, we should be acting unwisely to delay the employment of more active means a single minute. We must, then, make up our minds to meet with a certain degree of resistance, and we must overcome it in the softest and most gentle manner. Having taken off our coat and anointed our hand and arm, we kneel by the bed-side and introduce our hand, previously gathered into the form of a cone, fully into the vagina. When we arrive at the os uteri, we must dilate it with the greatest care, using a slow boring motion, and steadying at the same time the uterine tumor with the right hand externally applied. The hand having entered the cavity, must be passed behind the placenta, between its maternal face and the uterus, as before directed; it must be carried over the whole surface of the mass, to ascertain that no part remains adherent, and when we have embraced it all within our grasp, it may be withdrawn. The uterus will most probably act forcibly, on the introduction of the hand into its cavity, and expel it and the placenta together.

You may suppose that cases will occasionally happen in which we cannot introduce the hand for the removal of the placenta after the child's birth. It is possible, certainly, that the uterus may take upon itself such violent contraction immediately, as to offer an insuperable barrier to the passage of the hand; but I never met with a case of this kind when the operation had not been delayed much beyond the limit I have assigned for our passive treatment. I never saw an instance, within a few hours after the birth, in which, by care, tenderness, and perseverance, I could not introduce my hand, and that without injury to the uterine structure, provided the term of gestation were nearly completed. Our obvious indication, if we were foiled, would be to place the patient in some degree under the influence of opium, and take advantage of the earliest opportunity of its action to renew our attempts: for the longer we delay, the more difficulty we shall experience from the permanent contraction which will assuredly take place, and which we have no means, as far as I have been able to judge, of removing.

Another kind of irregular contraction sometimes occurs—the too rapid closure of the ŏs uteri during the passage of the placenta through it; by which action the mass is detained prisoner, lying partly *in utero*, partly *in vagina*. Any attempt to draw it forth will usually be followed by a laceration of the placenta itself, and a cautious dilatation of the orifice is generally required for its removal.

Retention from morbid adhesion.—The last case is the most difficult of all: that in which morbid adhesion takes place,—agglutination between the two surfaces of the uterus and placenta,—in consequence, most probably, of a deposition of coagulable lymph, the produce of a peculiar kind of inflammation which the lining membrane of the uterus has taken upon itself. The adhesion may be of greater or less extent, and of a higher or lower degree of intensity. Sometimes the whole placenta becomes united by adhesion, of which I have known instances; at others, the part adherent may not exceed a sixpence in extent; but the union may be so firm that the unaided efforts of the uterus, however strongly excited, are not sufficient to produce entire separation of the mass. Professor Burns mentions a case in which the placenta was retained four days, and a fatal termination ensued, although the surface morbidly adherent was not larger than a shilling.

As a general principle, the larger is the surface detached from the uterus the more copious will be the hæmorrhage, because the greater is the number of vessels opened; and if the adhesion be entire, the sanguineous appearance will be but very trifling; no blood flowing, indeed, out of the vessels in connexion with the placenta, and all that is lost being afforded by the small arteries which communicated with the deciduous membrane.

Whenever half an hour or an hour has elapsed since the birth, without the appearance of any sanguineous discharge, while at the same time three or four smart uterine contractions have taken place, we may begin to suspect not only that the

placenta is morbidly adherent, but that throughout its whole extent; because, if any part of the placenta were separated, some vessels must be rendered patulous. This is certainly a rare case, but it has happened to me two or three times to meet with it.

The intensity varies in degree as much as the extent: it is sometimes so slight, that, notwithstanding the uterine powers cannot accomplish the expulsion of the mass, yet it may be separated by the hand with the greatest ease; in others, again, the adhesion is so strong that it is impossible to peel the placenta off from its attachment. Here is a case [presenting a preparation] where a portion of the placenta was so firmly attached to the uterine surface, that it could not by any means be removed; nay, I have opened more than one body where a part of the placenta was left adherent to the uterus, and where, on making a longitudinal section of the organs, and examining the cut edges, I could not determine the boundary line between the uterus and the placenta; so that you may imagine the difficulty which must sometimes be experienced in attempting to remove the mass when adherent. This state of morbid union, however, is, in my opinion, not so frequent as is generally supposed: the placenta is more often retained by a want of action, or by irregular contraction in the uterine fibres, than by positive adhesion.

This, indeed, may not be the case in the practice of those who have received a proper medical education, and who have thought the study of the obstetric department of the science worth cultivating; because such men, impressed with the dangers of hurrying the extraction of the body after the head's expulsion, and the impropriety of meddlesome interference, would allow nature to terminate the delivery unaided, and would thus act in the best manner to ensure the most perfect and regular contraction of the womb. But under the hands of women, or persons but partially educated, I have found the cases of more simple retention outweigh in number those of morbid adhesion.

When called upon to separate an adherent placenta, we may find the uterus flabby and uncontracted, or it may have embraced the mass more or less tightly. It is most usual for a contracted state to exist in conjunction with morbid adhesion, because the probability is that the uterus will have made some efforts to expel it, and not being able to protrude it from its cavity, it will have closed upon it. We shall also sometimes meet with it partially extruded from the uterine cavity, and a greater or less portion lying loose in the vagina, and we may trace it passing through the os uteri, and find another part adherent to the uterus within. This state can hardly indeed exist, unless the attachment had been originally much lower than is usual; but when it does occur, it may be the cause of much embarrassment, and, if not understood, of great danger. So long as any portion of the mass remains connected by morbid organization with the uterine surface, so long any attempts to remove it by traction at the cord, or even by pulling at the placenta itself, must be in the highest degree hazardous; and for this reason I have, on a former occasion, cautioned you not to attempt its removal by the agency of the funis, until not only the insertion of the cord can be easily distinguished,—not only the bulk of the placenta can be clearly felt, but its general body can be completely surrounded by the finger, introduced as in a common examination. The management of the case I am supposing must be conducted on exactly the same principles as if the whole organ was shut up within the uterine cavity.

I have said that I consider this morbid adhesion is produced by the formation of a fresh membrane, in consequence of inflammatory action existing in the uterus; and I will now give you my reasons for that opinion. In the first place, we find adhesion of the placenta more frequent among the lower classes than in the higher circles; and this is easily explained upon the greater liability of the poor to such accidents during pregnancy as are likely to induce inflammation in the uterine structure, which may terminate in the agglutination of the two surfaces together.

I have often myself known adhesion of the placenta follow an injury during gestation; and I have frequently inquired of my patient, after having removed an adherent placenta, whether she has suffered pain in the belly during pregnancy, and her reply has very usually been, "Yes, just where your hand was,"—my hand, for the purpose of the separation, having been carried to the part where agglutination had taken place. From observing, then, that the patient, while pregnant, has had a fall, or received a blow; that she has experienced pain, evidently the result of inflammation,—I think there is no doubt that the morbid union is the effect of the same kind of action in the vessels of the uterus as occasions the formation of false membranes in other parts of the body; and I see no reason to believe otherwise. I do not mean to state that adhesion will be met with in every case where pain in the uterus exists during gestation, because that pain may be spasmodic or neuralgic,

and not the effect of inflammatory action: besides, the whole structure of the organ need not be the subject of the disease, even were it inflammatory: the lining membrane may possibly escape: even should the mucous membrane be implicated, the affection may be situated in a part remote from the implantation of the placental mass, and consequently no change can be expected to occur at that particular spot. Again, let us suppose that the very point at which the fœtal organ is attached has become the seat of injury and subsequent inflammation, still it is evident that resolution may occur,—that effusion of lymph need not take place, and that no difficulty may arise in the labour; so that there are a great many chances against the production of the effect I am describing.

My belief, however, that this morbid adhesion is caused by inflammation of the lining membrane of the uterus, is also strengthened by having observed this state occasionally follow accidental hæmorrhages towards the close of pregnancy. Cases are not unfrequently met with in which two or three eruptions of blood having taken place, consequent on some external and easily assignable cause, the hæmorrhage gradually ceases, and does not return; but, under labour, adhesion of the placenta is discovered.

I presume, under such circumstances, that the cessation of the discharge depends upon an agglutination of the placenta with the uterus; nor is the explanation difficult. The two surfaces remain in contact, though not attached, having been separated from each other by some accidental cause; and, to prevent a continuance of bleeding, and to save life, nature makes a strenuous effort; inflammation is set up in the membrane of the womb, by which the placenta is glued to the uterine surface, and thus the open vessels are permanently closed. Such a change is not more extraordinary than many of the contrivances to avert danger, which we observe nature daily practise, and quite in accordance with the mode she generally adopts to repair injuries.

Moreover, I have had occasion to notice, in an early part of the course, that disease sometimes takes place in the placenta itself. Occasionally the mass becomes studded with tubercular formations, like small scirrhous glands: sometimes there are spiculæ or granules of bone strewed, as it were, over the maternal surface, and sometimes the organ becomes almost cartilaginous throughout; at others, unnaturally soft;—these states being frequently connected with adhesion under labour. It is fair to presume, then, that the uterine membrane is excited and irritated by contact with the diseased mass, and that inflammation is the primary, and effusion of lymph the secondary effect.

When all these circumstances are considered in conjunction, there can be little doubt that the morbid change occasioning adhesion of the placenta is to be referred to excitement of the uterine vessels as the immediate agents, and not to the fœtal system.

We may suspect that morbid adhesion exists if after the birth of the child the placenta does not descend, although the uterus continues moderately active; and if, on putting the funis rather on the stretch, and then letting it suddenly go, it springs up with a sort of jerk: but we can only positively detect the true nature of the case when the hand is in the uterus for the purpose of removing the mass.

Treatment.—The removal is to be conducted on exactly the same principles as I before mentioned. The hand is to be carried up to the placenta; we are to seek for an edge which has been separated, and is lying loose; insinuate the fingers cautiously between this and the uterine surface; and by gently moving the hand backwards and forwards with a sawing kind of motion,—keeping the palm towards the placenta, and the knuckles next the uterus,—we continue the separation until we find that we have encompassed the whole of the organ, and that it drops loose into our hand: or if the adhesion be too firm to give way to this mode of proceeding, we may often succeed in removing the whole by cautiously working with each finger separately.

Either of these methods is by far preferable to that recommended by Dr. Hamilton,—expanding the fingers over the fœtal surface, and squeezing the edges towards the centre;—because, if the agglutination be firm, we are very likely to break the placenta, and leave filaments still adherent. The principal, and indeed the only objection to the plan I adopt, is the chance of bruising, scratching, or slightly tearing the uterine membrane with the points of the fingers, the nails, or the knuckles; and no doubt, if the nails be long, pointed, or rough, at their extremities, or the operation be performed hurridly or inconsiderately, such accidents are very likely to happen; but I take it for granted that due caution will be used, and I am myself not aware of ever having inflicted injury under the operation.

The hand should not be withdrawn from the uterus until the entire separation is effected, and we must be most particular in removing every particle of the mass. I know that to get it all away is sometimes impracticable, owing to the strength of the adhesion; but such cases are fortunately rare. I know, also, that we are

told by some authorities of great weight, that should the placenta break under the action of the hand, we are not to use much effort to procure it all, but remove what we can, and leave the remainder. I cannot help thinking that such a doctrine is highly dangerous, by impressing the student with the belief that in many cases its entire separation is impossible, and perhaps by lulling him into a fatal carelessness. It would be my wish, on the contrary, to inculcate the idea that the *whole* can very generally, and ought, even at the expense of some trouble, to be removed; and that we should never feel satisfied that we have done our duty, unless we have used our best endeavours to effect its entire abstraction. The recommendation given by those practitioners who think differently with myself, is founded on the supposition that more danger will accrue by our attempts at separating the strongly-adherent portions than by leaving some filaments behind. From some practical observation, however, I am persuaded that very few states after delivery are fraught with such extreme peril as that in which any portion of the placenta remains adherent to the uterine surface; and I believe, also, both that the uterine membrane is not so liable to serious injury as has been supposed, and that, if injured, it possesses great powers of reparation within itself: and for these reasons I consider it my duty strongly to enforce the practice I have just ventured to advocate. On examining the placenta, also, after its removal, if a large proportion be wanting, I think it better to attempt to remove it immediately, under the employment of the utmost tenderness, than to leave it to be expelled, to putrify, to become perhaps the nucleus for hydatid formations, or to the chance of its absorption.

I have recommended, for the reasons assigned when treating of transverse presentations, that the left hand should be employed in the operation under consideration: other practitioners of experience prefer the right with Merriman; and others, again (as Ingleby), introduce the right if the placenta be attached towards the back part of the organ, and the left if forward; and they judge of the situation of the mass by the direction in which the funis runs upwards into the cavity. I am not satisfied that it is always, or indeed generally, possible to tell whether the placenta be connected anteriorly or posteriorly, by tracing the funis up to the pelvic brim; and under every diversity of attachment, I am myself in the habit of using the left.

You must be already prepared for the observation, that the difficulty in removing an adherent placenta will be dependent upon two causes; partly the contracted state of the uterus in resisting the introduction of the hand, but principally the degree of its adhesion, both in extent and intensity; so that we have a combination of difficulties only to be overcome by the most judicious management, and not to be undertaken except by one who has acquired an intimate acquaintance with the structure of the parts.

Although, when the hæmorrhage is copious, the manual removal of the placenta is the only means to which we can trust for the closure of the uterine cavity, and the suppression of the discharge, still, if the patient be lying under a state of syncope, it would be unwise to empty the uterus until the system had somewhat rallied, lest the organ be left in a flaccid condition, and on the restoration of the heart's action a fresh and more violent eruption should ensue; for it cannot be too strongly or too frequently impressed on your minds, that the removal of the placenta is not our only object, but that our principal attention should be directed to producing a firm and permanent state of contraction in the uterus itself. Gooch, indeed, used to declare, "he could positively assert from experience, that the organ would contract even during syncope;" but he acknowledged its action under such circumstances was feeble. I do not mean to deny the possibility of contraction occurring under a temporary suspension of the vital functions; but I am perfectly persuaded that, in the generality of such cases, we should be disappointed in our expectation; and I am equally convinced, that the safest plan is that I have just advised. The exhibition of stimuli, then, may become necessary to rouse the torpid energies before the operation is undertaken.

Another cause impeding the easy removal of the placenta from the uterus, will be found in a contracted state of the pelvic apertures. The pelvis I hold in my hand belonged to a woman whom I delivered by perforating the skull, in the Workhouse of Bethnal-green. Immediately the child was born, a profuse hæmorrhage took place, not to be arrested by pressure, or the application of cold; and I was induced to pass my hand into the uterus for the withdrawal of the placenta. Having separated it from its attachment,—for it was partially adherent,—and embraced it within my grasp, I found, as you may easily see, that I could not bring my closed hand through the brim, in consequence of its diminished capacity. I was compelled, therefore, to relax my hold, to withdraw my hand open, and trust to the funis for the extraction of the placenta.

A morbid union appears occasionally, though very seldom, to take place between the fœtal membranes and the decidua, or

the uterine surface, at a greater or less distance from the point of placental attachment; and if such an adhesion be to any considerable extent, it may prevent the occlusion of the placenta from the uterine cavity, and consequently be the cause of a retention of the mass. We should not, *à priori*, suppose that agglutination, by the deposition of coagulable lymph, would occur between the uterine membrane and the chorion, from the great dissimilarity of their tissues; but I am persuaded I have known three or four instances in which such an adhesion impeded the descent of the placenta into the vagina. The case must be treated, in every respect, on the principles already laid down, and the separation is generally not difficult to be effected.

It is a remark made by most practical men, that some women seem constitutionally subject to an adhesion of the placenta; so that this cause of danger exists in almost every successive pregnancy; and I have myself known several examples of this unfortunate peculiarity. It becomes, then, a point of some importance to ascertain whether, by any means applied during gestation, the danger might not be obviated. Quietude, rest, regular attention to the action of the bowels, an unstimulating diet, and the occasional abstraction of blood—particularly if the circulation be hurried, or a fixed pain shew itself at the lower part of the abdomen—seem to offer the best chance of success. With regard to bleeding under pregnancy, I am certainly averse to it, unless there exist some grave occasion; and as a principle, do not coincide with those practitioners who have frequent recourse to the lancet to prevent flooding in labour, where the uterus is constitutionally disposed to relax after the birth of the child: because, in such systems, the powers of life are generally weak, the habit is relaxed, and not in a condition to afford the quantity taken. If blood be formed rapidly, indeed — as is not unfrequently the case under pregnancy—the lancet may be advantageously employed for the object in question: and if undue determination to any particular organ exist, bleeding is even more necessary than in the unimpregnated state; for it is a common observation both that inflammatory disease runs on to its termination more speedily during gestation, and that the loss of more blood is required for its removal than in the ordinary condition of the system. I have but a few days since, I think, seen the advantage of the loss of blood in the prevention of adhesion of the placenta. I attended a lady of her fourth child, who, in her second and third pregnancy, was the subject of a fixed pain, but not of an acute character, in the right iliac region, which commenced about the period of quickening, and continued, rising however in situation, more or less, till the term of gestation was nearly completed. In both those labours there existed a morbidly adherent placenta, and considerable hæmorrhage took place. Under the last pregnancy, also, the same kind of pain returned about the same period, and she became apprehensive that the same mischance would attend her labour. Although rather of a cachectic habit, I advised low diet, and the abstraction of a small quantity of blood; and with some difficulty persuaded her to permit it. The pain soon disappeared, did not return, and her labour was in every respect natural. I have little doubt that a low degree of inflammation existed in the uterus itself, which would probably have terminated in the effusion of lymph.

EXPERIMENTS ON THE RELATION WHICH SUBSISTS BETWEEN THE NUMBER OF THE PULSE AND THE VELOCITY OF THE CIRCULATION OF THE BLOOD.

By Professor Hering,
Of the Royal Veterinary School, Stuttgart.

Translated and abridged from Tiedemann's Zeitschrift für Physiologie, b. v. h. i.

By Dr. Arrowsmith.

Several years ago Professor Hering published an experimental essay in the third volume of Tiedemann's Physiological Journal, on the velocity of the circulation of the blood and secretion. In that paper he contented himself, as regards the distribution of the blood, with the very unexpected result of a far greater velocity of the circulation than physiologists had been previously aware of. In horses, for instance, the time required for the circulation of the blood through the anterior half of the body and the pulmonary organs, was found to be 20-25 seconds. Although, in these experiments, he had generally specified the number of the pulse, he did not accurately determine the relation subsisting between the rate of pulsation of the heart and arteries and the velo-

city with which the blood circulated through the body. On these points he instituted the present series of experiments, by means of which the remaining circumstances also, which might influence the results, as the state of health and strength, the respiration, &c. could, at the same time, be more accurately noted.

These experiments, 40 in number, were performed, as in the former series, on horses; as, in consequence of their familiarity with man, impediments on the part of the animal could be more easily obviated. The horses were simply held by a halter or snaffle-bridle in an open place, and all other restraint or disquietude avoided. The details of these experiments, with the remarks on them, occupy thirty-six pages of Tiedemann's Journal; but the abstract now presented to the reader exhibits all the important facts elicited by the inquiry.

It is necessary to premise, that the test employed by Hering to indicate the rate of movement of the mass of blood was the ferro-prussiate of potass. A solution of this substance in water was injected into the jugular vein on one side; and the jugular vein on the opposite side being opened at the same time, the blood, as it flowed, was tested every five seconds; and the moment at which the presence of the ferro-prussiate of potass was denoted in this blood, indicated the period in which the blood performed one circulation through the anterior half of the body and the pulmonary organs.

The Table annexed, which exhibits the results of the experiments, is a translated transcript from the original,—with the addition, however, of the two columns which indicate the state of the pulse and respiration of the animal before the commencement of the experiment. This addition, derived from the detailed observations in the original paper, will, it is presumed, render the tabular results more explicit and complete.

That accelerated pulsation of the heart and arteries is always accompanied by a corresponding increased velocity of the circulation of the blood was, till now, an almost universal opinion; and if we assume, with some modern physiologists, that the heart is the only motive power of the blood, the conclusion would be inevitable.

This opinion, however, if we refer to the experiments, the results of which are exhibited in the Table subjoined, is found to be no longer tenable. According to this Table, the absence of any coincidence between the pulsation (the accelerated pulsation particularly) of the heart and arteries, and the velocity of the motion of the blood, is very striking.

In Professor Hering's former experiments, the normal velocity of the circulation of the blood in healthy horses, with the pulse and respiration tranquil, appeared to be from 20-25 seconds, which period is confirmed in general by the present series of experiments. But if we make allowance for peculiarities of age, size, &c. and extend the period to 30 seconds, then may we confidently say, that in a healthy strong horse, in a state of rest, the blood passes through the (anterior or posterior) half of the body and the pulmonary organs within 20-30 seconds.

Rapid excitement of the vascular system, by the injection into the veins of veratria, alcohol, &c. was found to increase the pulse two or three-fold in some instances, but did not accelerate the circulation of the blood. In like manner, blood-letting accelerated the pulse, but not the movement of the blood; indeed, it rather retarded it. Tetanus, meningitis, and mortification of the intestines, retarded the circulation from 5 to 10 seconds; and the same result was observed in an animal affected with torpor. Respiration has, without doubt, an important influence on the circulation of the blood; but an increased number of inspirations does not bring a corresponding acceleration of the circulation; in some experiments, on the contrary, inspiration retarded it. After the section of the spinal marrow at the foramen magnum (Exp. 7), breathing suddenly ceased, whilst the heart and arteries continued to beat some minutes. The circulation was delayed to 45-50 seconds. After the pulse stopped, the circulation ceased also. In microscopic examination it has been observed, on the other hand, that for some time after apparent death, an irregular motion of the blood was visible in the vessels; and in the detailed account of the 29th Experiment, it is stated that, after the arteries had ceased to beat, the blood continued to flow from the maxillary artery, but without pulsation. [After

No. of Experiments.	General State of the Animal.	No. of the Pulse before the Experiment.	No. of Inspirations before Experiment.	Pulse accelerated by	No. of Inspirations during the Experiment.	No. of Pulse during the Experiment.	Velocity of Circulation of Blood.
							Seconds.
1	Healthy	48	8	*Veratria injected into vein.	Laborious.	64	25—30
2	Ditto	40	10	Ditto	Ditto	120	30—35
3	Extremely weak	80	12	....	12	80	35—40
4	Healthy	48	16	†Ammonia injected into the vein, and by exercise.	..	120-72	50—55
5	Ditto	44	10	‡Sp. of Camph. injected into vein.	60	64, hard	30—35
6	Ditto	56	..	Bloodletting to 10 lbs.	..	80-88	20—25
7	Broken-winded	44	36	Killed by section of the spinal marrow at occipital foramen.	..	Fluttering.	45—50
8	Healthy	32	12	....	12	32	25—30
9	Very weak	40	14	Bleeding to 15 lbs.	28	80	45—50
10	Healthy	34	Tranquil	§ Alcohol injected into vein.	..	64, hard	35—40
11	Very weak	..	..	Exercise	40	92-96	20—25
12	Emaciated	56	Tranquil	....	Tranquil	56-68	35—40
13	Healthy	36	..	Bleeding to 20 lbs.	Accelerated	72	40—45
14	Feeble	..	..	Exertion	20	96	20—25
15	Healthy	40—50	Tranquil	‖ Inj. of Ammonia	Labour'd	96, full	25—30
16	Ditto	36	Ditto	....	Tranquil	36-40	30—35
17	Pleuritis	72	20	....	20	72, contracted	25—30
18	Healthy	48	Natural	....	Tranquil	48-52	15—20
19	Ditto	48	Ditto	....	Ditto	48	25—30
20	Ditto	28	12	....	12	28-36	40—45
21	Broken-winded	36	24	....	24	36-40	15—20
22	Tetanus; broken-winded before; had been bled to 36—40 lbs.				68	60	30—35
23	Inflam. of Brain	..	..	....	Slow	104-112	30—35
24	Feeble	60	12-15	....	12-15	60	15—20
25	Same animal 30 hours after.	..	..	¶ Inj. of Ammonia	30, panting.	120	20—25
26	Colic; mortification.	80	16	....	16 do.	80, small	30—35
27	Healthy	40	12	....	12	40	25—30
28	Ditto	40	12	Bleeding to 14 lbs.	Feeble	80	25—30
29	Weak	64	Tranquil	....	Tranquil	64	20—25
30	Ditto	32	Ditto	....	Ditto	32-36	20—25
31	Very weak	68	Ditto	....	Ditto	68-72	40—45
32	Weak	48	Slow, laboured	....	..	56	30—35
33	Torpor	56	12	....	12, laborious.	Ditto	30—35
34	Same animal killed 4 days after, by injecting warm water into veins.						
35	Weak	40	10-12	....	10-12	40	35—40
36	Ditto	40	Ditto	....	Ditto	Ditto	35—40
37	Weaker	40	Ditto	** Morphia injected into vein.	..	38	35—40
38	Ditto	38	Ditto	†† Inj. of Alcohol	16, laborious.	36-40	50—55
39	Broken-winded	40	20	....	20	40	20—25
40	Ditto	40	30	....	30	Ditto	25—30

The notes referred to in this Table will be found in the next page.

After these and many other similar remarks, Professor Hering observes that the chief results of his numerous experiments is expressed in the following proposition, namely, *That the velocity of the circulation of the blood bears no definite relation to the velocity of the pulse.* This is proved in the two following ways:—

1stly, In the same animal, in four different experiments (35, 36, 37, 38), the pulse being normal in each experiment, the velocity of the circulation of the blood was, in three experiments, 35-40 seconds, and in the fourth, 50-55 seconds.

2dly, By comparing experiments 27 and 28, 30 and 29, 35 and 3, 19 and 15, 8 and 1, 39 and 6, 32 and 23, 16 and 26, and 20 and 13, the pulse will be seen to have been twice as quick in the latter instances as in the former, but the velocity of the circulation was equal in the respective experiments. Even in the experiments in which the pulse was accelerated three times the usual rate, as is seen by comparing 8 and 15, 16 and 2, 30 and 14, and 39 and 25, the velocity of the circulation remained the same. When the pulse is from 30 to 40 in a minute, the blood can perform its circulation in 15-20 seconds (Exp. 21); but it may require 50-55 seconds (Exp. 28): and when the pulse was from 100 to 120, the circulation was performed in 20-25 seconds; but (in Exper. 4) the pulse being the same, from 50 to 55 seconds were required to complete it.

The extremes in the rapidity of the circulation of the blood were, in the foregoing experiments, 15-20 and 50-55 seconds, or nearly triple. About the same relation occurs in the number of pulsations, which, by the means pointed out, could be made to increase threefold. Remarkable slowness of the pulse very seldom occurs in horses, and is found to amount, at the utmost, to one-fourth of the normal number, which is 36-40 strokes in the minute. Acceleration of the circulation of the blood in horses, in more than sixty experiments, was observed but three times, and then only to one-fourth or one-fifth of the normal velocity.

If we set down the normal velocity of the circulation to be 20-30 seconds, then the greatest degree of retardation is but double the usual time; which, in fact, is not important, if we reflect on the difference between a strong healthy animal and one almost exhausted from loss of blood, mortification, &c. The living body, Hering thinks, must therefore possess a means of regulating within certain limits, the vital function of the circulation of the blood.

The organ endowed with that power, says he, is the heart; whose alternate contractions and dilatations may be compared to the pendulum of a clock, which is not, as it appears to be at first view, the cause of motion in the clock, but the means of regulating and rendering uniform its rate of movement: and like as the appended weight, or concealed spring, urges on the mechanism of the clock, so does the vital principle inherent in the blood, as in every other living part, excite it to action. That the proximate cause of the blood's motion is to be discovered in the blood itself, is apparent from former and recent experiments; particularly from those of Döllinger, Oesterreicher, Kaltenbrunner, Baumgäartner, Burkhart, and others: but it would be as incorrect to overlook the influence of the heart, as it was erroneous in the jatromathematic school to represent that organ as the sole cause of the motion of the blood.

The acceleration or retardation of the circulation, depends primarily on the *vis insita* of the blood itself; which power, in disturbances of the circulation, appears rather to diminish than to increase. The accelerated contractions of the heart may be, therefore, regarded as efforts of nature (oftentimes, indeed,

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* 2 drs. of tincture of veratria, with as much distilled water, was injected. The tincture was made from 1 pt. of rad. veratr. and 8 pts. alcohol (Sp. Gr. 0·900).

† 1 dr. of liq. ammon. caust. (0·972 Sp. Gr.) with 1 oz. of water. This quantity was repeated three times.

‡ Half a drachm of camphor dissolved in 1 oz. of rectified spirit of wine.

§ 3 oz. of alcohol (0·900 Sp. Gr.) This quantity was twice repeated.

‖ 1 oz. of liq. ammon. caustic. (0·972 Sp. Gr.) with 1 oz of water.

¶ Of a mixture of 1 oz. of liq. ammon. (as above) and 2 oz. of water, about 1-3d was injected into the vein.

** 3 grs. of pure morphia with a few drops of diluted sulphuric acid, dissolved in 1½ oz. of water.

†† 6 oz. of diluted alcohol, taken from an old anatomical preparation, Sp. Gr. not ascertained, was first injected, but without effect. Afterwards, 2 drs. of spirit of camphor and 1 oz. of alcohol (0·840 Sp. Gr.)

insufficient) to regulate the circulation. Mr. John Bell, long since, announced a similar opinion. "The heart," says he, "is rather the regulator than the prime and efficient cause of the circulation*."

An appropriate analogy; for the existence of a regulating power over the circulation may be found in animal heat, which in like manner is maintained within certain limits by the vital power (*von dem organismus*); and although some of the means by which it is preserved are known to us—as respiration, covering with bad conductors of heat, the increase or diminution of gaseous secretions, &c.—we are, nevertheless, far from having an accurate knowledge of them all.

Finally: whatever view, says Professor Hering, with regard to the forces which occasion the movements of the blood, may receive the homage of physiologists, and whatever interpretation may be put on the results of the preceding experiments, the following may be regarded as matters of fact:—

1. That in the horse, the blood passes from the jugular vein on the one side to the heart, and through the lungs; thence to the heart again; and afterwards, through the anterior half of the greater circulation, to the jugular vein on the opposite side, in an interval of, generally speaking, from twenty to thirty seconds.

2. That the number of the pulse in a given time, furnishes no certain criterion of the velocity of the circulation of the blood.

3. That the velocity of the circulation of the blood remains, in most cases, nearly in its normal state, uninfluenced by acceleration of the pulse (or breathing).

4. That an actual acceleration of the circulation of the blood is a very rare occurrence; and when present, exists only in a slight degree.

5. That the retardation of the circulation, observed in many of these experiments, amounted, in the extremest cases, to about double the average period (20-30 seconds); in general, however, the retardation was much less.

Coventry, August 4, 1834.

* Essay on the Forces which Circulate the Blood, by John Bell. 1819.

ON THE COMPARATIVE MERITS

OF

DR. BELLINGERI'S AND SIR C. BELL'S WRITINGS

AND OPINIONS ON THE FUNCTIONS OF THE FIFTH AND SEVENTH PAIRS OF CEREBRAL NERVES.

To the Editor of the Medical Gazette.

Sir,

No one is more desirous than I am of giving to Sir Charles Bell all the credit he justly deserves for what he has done on the subject of the nervous system. I really believe that, after having read his "Idea of a New Anatomy of the Brain, submitted for the observations of his friends," printed in 1811, although merely for private circulation amongst them, nobody in this country, where he publicly professed the principles therein exposed, can deny to Sir Charles Bell the merit of having opened and traced a new path, leading to the most important inquiries on such an intricate subject as that of the nervous system.

Still it is certain that Sir Charles Bell's "*unpublished tract*" was not, nor is now, known on the Continent, or at any rate in Italy, where Bellingeri's *Dissertatio* (parts 2d and 3d of which contain the Anatomy and Physiology of the Fifth and Seventh Pairs of Cerebral Nerves) was published in 1818, and sent in the following year, 1819, by the author, to the Royal Society of London, where, I have good reason to believe, it remained untouched till last winter.

On the contrary, Sir Charles Bell's first memoir "On the Nerves," &c. was read to the Royal Society the 12th of July, 1821, and printed in the second part of the Philosophical Transactions of the same year. The fifth pair is there considered, in common with those of the spine, as a symmetrical nerve, with double origin, and a ganglion on one of its roots, and endowed with both voluntary motion and sensibility.—(See Philosophical Transactions, 1821, page 404.) "If the fifth nerve, and the portio dura of the seventh (says the author), be both exposed on the face of a living animal, there will not remain the slightest doubt in the mind of the experimenter which of these nerves bestows sensibility. If the nerve of this origi-

nal class be divided, the skin and common substance is deprived of sensibility; but if a nerve not of this class be divided, it in no measure deprives the parts of their sensibility to external impression."—(*Id.* p. 405)

Then, when speaking of the trigeminus, or fifth pair, the author goes on saying, "In all animals that have a stomach, with palpi or tentacula to embrace their food, the rudiments of this nerve may be perceived;" "it is the nerve of taste, and of the salivary glands, of the muscles of the face and jaws, and common sensibility." "A ganglion is formed upon it, near its origin, though some of its filaments pass on without entering into the ganglion. Before passing out of the skull, the nerve splits into three great divisions, which are sent to the face, jaws, and tongue: its branches go minutely into the skin, and enter into all the muscles, and they are especially profuse to the muscles which move the lips upon the teeth." (pp. 409, 410.)

In conclusion, when speaking of the functions of the trigeminus, as illustrated by his experiments, the author says,—"We have seen that when the fifth nerve, the nerve of mastication and sensation, was cut in an ass, the animal could no longer gather his food."

From these quotations it is evident that Sir Charles Bell considered the fifth pair a mixed nerve of sense and voluntary motion, distributed to the face generally, both for *mastication* and *sensation;* but still he never mentioned the smaller portion of the fifth as a peculiar nerve, merely for the voluntary muscular action of the lower jaw, although as such it had been beautifully described by Paletta as far back as the year 1784, and by him called, from its anatomical distribution, *nervus crotaphiticus et buccinatorius;* and which Bellingeri, from its physiology, distinguished it by the name of *nervus masticatorius.*

On the 28th of May, 1829, Sir Charles Bell's last memoir "On the Nerves of the Face," was read before the Royal Society, and printed in their Transactions for the same year. There the author says,—"Confident in the accuracy of my deductions from the anatomy of the fifth nerve, I had attributed to one of its branches a function which belongs to another branch of the same nerve."—(See Sir Charles Bell's last work on the Nervous System, page 94.) In this paper only we meet, for the first time, with an article on the motor, or manducatory portion of the fifth nerve, of which he says,—"Since the publication of my first paper this inquiry has assumed importance, although the principal facts of the anatomy were known to Wrisberg, Santorini, Paletta*, Prochaska, and Soëmmerring; *but in no author is the anatomy of the motor portion of the nerve traced with sufficient minuteness, or regard to the distinct uses of the muscular and sensitive divisions,*" (p. 100.) And farther on:—"The form of the fifth, and its resemblance to the spinal nerves, had struck some of the best continental anatomists; *but as they made no distinctions in the functions of the roots of the spinal nerves, so neither did they imagine any difference in the roots of the fifth nerve, and therefore no consequence resulted from having observed this resemblance. This part of anatomy, together with the whole minute relations of the nerves, was a dead letter, and led to no inference.*" (See p. 103.)

Now Bellingeri gave the most minute and distinct anatomy both of the ganglionic and motor portions of the fifth†, in the second part of his *Dissertatio*, (which is entirely on the anatomy of the nerves of the face), with an interesting tabular form of their respective origins, and distribution from *branch to branches down to the last fibrillæ;* and from the exclusive distribution of *part* of its smaller portion, he called it, as already mentioned, *nervus masticatorius.*

But with the view of giving a more correct idea of Bellingeri's opinions on this particular subject, I shall bring before the public some quotations from his Latin *Dissertatio*, Part III. Art. ii.; the title of which is, "Usus portionis minoris quinti paris." (See p. 176.)

"Portio minor quinti paris diversam omnino originem, iter, et structuram habet a majori ipsius portione, et per proprios surculos in musculos temporalem, massetericum, buccinatorium, pte-

* This is the first time, and the only place in this paper, where we met with this celebrated anatomist's name.—See Mr. A. Shaw's article, in the Med. Gaz. July 19th, p. 563.

† See Part II. of the Edinburgh Medical and Surgical Journal, July 1, 1834.

rygoïdeos externum, et internum, orbicularem insuper labiorum, elevatorem anguli oris, et triangularem menti inseritur. Hanc minorem portionem dixit Paletta, voluntarios exequi motus, et sympathice, vel idiopathice in trismo affici.

"Voluntarie certe et nostro arbitrio movemus musculos cunctos, in quibus inseritur portio minor. Hunc vero voluntarium musculorum motum *a sola minori quinti portione pendêre*, non a septimo, vel faciali nervo, qui alicubi cum ramis ipsius consociatur, demonstratum fuit ex eo quod in carnea substantia musculi temporalis, et in musculis pterygoïdeis præsertim, soli immittantur minoris quinti portionis rami absque ullis nervi facialis propaginibus, et tamen isti musculi voluntatis obediunt imperio. . . . . Spectat igitur portio minor ad nervos vitæ animalis, *et quidem ad nervos motorios; nullibi enim sensibus præest, et habita ratione ipsius officii*, NERVUS MASTICATORIUS esset dicendus. Patet igitur, *quam merito Paletta*, anatomicis ductus rationibus *spectataque diversa origine, structura, et itinere*, dixerit nervum crotaphiticum, et buccinatorium distinctos a quinto pari nervos constituere, namque et physiologicus ipsorum usus demonstrat, a majori quinti portione differre ipsammet portionem minorem." (p. 177.)

"Verum demonstrato, prout sategimus, minorem hanc portionem per se voluntarios exequi motus, inquirendum remanet, cur *in quibusdam circumstantiis* involuntarie, vel ab instinctu moveat musculos, in quibus inseritur. Hoc pariter anatomes explicatur adjumento; vidimus porro in præcedenti dissertatione (*he means the second part, which contains the anatomy of this nerve*), minorem quinti portionem in suo e foramine ovato egressu intime ut plurimum per plexum ganglioformem cum maxillari inferiori connecti, insuper et fere omnes ramos portionis minoris accipere radices, aut filamenta a ramis maxillaris inferioris; igitur portio minor in propriis ramificationibus est nervus compositus *ex propriis filamentis, et a filamentis maxillaris inferioris:* nihil mirum inde, *si voluntarios ut plurimum*, et modo etiam involuntarios exequatur motus, quos tamen producit *non per propria filamenta*, sed per ea, *quæ a maxillari inferiori proveniunt*, quem organicæ vitæ præesse, et involuntarios perficere motus, ex superius dictis, constare dijudico." (See pp. 177 and 178.)

"Sunt igitur musculi, qui mixtos exequuntur motus, modo voluntarios, modo involuntarios, sunt et nervi, sed isti *vel constant e diversorum nervorum filamentis in unicum fasciculum collectis, vel filamenta unicum nervum constituentia diversam habent in encephalo originem.*" (p. 178.)

Here ends the article in Bellingeri's work on the Minor Portion of the Fifth Pair of Cerebral Nerves, and so I shall also conclude my first article, leaving the other parts of this question for a future period.

I am, sir,
Your obedient servant,
G. NEGRI, M.D.

August 9, 1834.

URINARY DEPOSITS.

To the Editor of the Medical Gazette.

SIR,

WE regret exceedingly that, in consequence of our absence from town, we did not see Dr. Prout's notice of our last communication until a week after its appearance in your journal, which circumstance is the reason of our replying to it at so late a period.

The error into which Dr. Prout considers we have fallen, is one of omission, having stated as his opinion, that the pink deposits *consist* of the alkaline purpurates, omitting to notice the presence of urate of ammonia, which Dr. P. considers to be the essential ingredient. That we were aware of the presence of this salt in such deposits (and not only of that in particular, but also of urate of lime, and probably of soda, as well as of free uric acid, and muriate of ammonia, which last we find to be almost universally present, not only in urinary sediments, but in calculi), is clear from the analysis detailed in our last communication. We were, however, more particularly anxious to decide the question as to whether the pink colour which certain urinary sediments possess, depends upon the presence of an alkaline purpurate, or upon a peculiar pink colouring matter, differing in its properties from the purpurates?

Dr. Prout speaks of the crimson, or

pink-coloured deposits being very rare; we have, however, seen five well-marked instances during the last two winters (the tints being almost precisely similar to those of Nos. 1 and 2 of the *pink* deposits figured in Dr. Prout's table of colours, published in his work on Calculous Affections.) The cases were as follows:—

Name.	Ward.	Disease.
1. Perin	12. Job	Hypertrophy of the heart, with bronchitis.
2. Rogers	3. Job	Hepatic ascites.
3. Anderson ..	20. Job	Splenic ascites.
4. ———(?)..	11. Job	Obscure visceral affection.
5. Eliz. Fowler	8. Petersham	Phthisis.

The three first patients were under the care of Dr. Bright; the two last under that of Dr. Addison. The name of the fourth patient (a schoolmaster) has unfortunately slipped our memories.

Dr. Prout, in pages 16 and 17 of his work, states that urate of ammonia, diffused through fresh healthy urine, and mixed with purpurate of ammonia, becomes, during deposition, tinged of a pink colour; but this proves nothing more than that the urate of ammonia acts as a mordaunt to certain kinds of colouring matter: but it must be remembered that urate of ammonia, tinged by the purpurate, does not yield up any of its colour to alcohol; whereas the same lithate, tinged by the colouring matter of the morbid deposits, may be restored to a pale fawn colour by the action of the same menstruum. Dr. Prout positively asserts, "that it is difficult, if not impossible, to prove the *existence* of the purpurate of ammonia by direct experiment;" but we cannot help flattering ourselves that the experiments published in our last communication have been more than sufficient to prove its *non-existence*. We may, however, mention in this place, that the experiments of Dr. Prout (page 16, *et seq.* of his work) are directly opposed by Berzelius, an undisputed authority; for, in his Lehrbuch der Chemie, under the head of uric acid, we have the following passage:—"The colour (of the pink deposits) cannot be attributed to purpurate of ammonia, as the pink tinge is removed by alcohol, which is not the case with the alkaline purpurate: it is true, that when we mix urate of ammonia with a solution of a purpurate in acetic acid, the uric acid is precipitated of a pale rose colour, very similar to the urinary deposit; but alcohol, when boiled on this tinged acid, does not extract the slightest tinge of colour. When urine is mixed with a similar acid solution of a purpurate, the deposit obtained does not differ in colour from that yielded by a similar portion of healthy urine, to which no purpurate was added."

Dr. Prout, in his communication of August 2d, has observed, that uric acid may be converted into purpurate of ammonia, by "*many* other ways" besides the action of nitric acid. We were aware that a solution of chlorine is capable of converting uric acid into purpuric acid, although, by a prolonged digestion, it is converted into oxalic acid; but were aware of no *other* manner of producing this change, except through the agency of nitric acid.

In conclusion, we beg to tender our most grateful thanks for the kind advice expressed by Dr. Prout to us, in his last communication, but still cannot help thinking that our experiments, corroborated as they are by the observations of Berzelius, are sufficient to place beyond a doubt the fact, that the colour possessed by the pink deposits is not owing to the purpurate of ammonia.

We remain, sir,
Your obedient servants,
R. H. Brett,
Golding Bird.

Guy's Hospital, Aug. 13, 1834.

DISLOCATION OF THE LEFT FEMUR INTO THE ISCHIATIC NOTCH.

REDUCTION AND SUBSEQUENT DISLOCATION.

To the Editor of the Medical Gazette.

SIR,

A YOUTH, æt. 16, was admitted on the evening of April 21st, into the Liverpool Northern Hospital, having been knocked down by the handle of a steam-pump.

The head of the left femur was felt to be in the ischiatic notch; the knee was slightly bent, the heel raised, and the great toe advanced over that of the opposite foot. The limb did not admit of rotation.

The pullies were applied; and, after a moderate extension, the bone was replaced.

After being put to bed, the feet were tied together by a bandage. The patient had a good deal of pain in the night; but it gradually subsided on the ensuing day.

On the fourth day, having loosened the bandage himself, he was found walking about the yard by the house-surgeon, Mr. Davis. A few days afterwards, despite of having been cautioned, he was found standing on the window-sill of the ward, and trying to get out. To attain this position he must have needed the help of a chair or bench.

Though warned by Mr. Davis and myself of the probable consequence of such irregularities, still he was not to be kept quiet. On the 7th of May he was discharged, and walked home with the help of a stick. There was a slight halt in his gait.

About the middle of July he was admitted into the infirmary. The bone was found to be displaced, and in the ischiatic noteh. The attempt at reduction was deemed not advisable, and he was discharged.

Having heard of this untoward result, I called at his house on the 21st July. The thigh appeared wasted, compared to its fellow; the upper part, below the anterior iliac spine, was fuller than that of the opposite side; the trochanter higher; the head of the femur was to be felt in the ischiatic noteh, and seemed indented to the touch; the nates were fuller, but more flaccid; the hollow behind the trochanter not present. The limb shortened rather more than an inch; could be rotated, and the foot not everted.

He said, that in the course of a day or two after his return home (the 7th of May), he felt some pain about the joint, and which was severe enough to cause him restless nights. In the day this was less troublesome. The pain was felt in the groin, and extended as far as the knee, along the inner side of the thigh. About the 17th or 18th of May, having gone to the manufactory where he had been employed, he was thrown down with some violence, and for four hours was unable to move, but afterwards was able to walk.

Though an unpleasant task, I am induced to publish the case, as it may be supposed the bone was not reduced on the evening of the 21st; but as positive proofs that it was,—It was felt, nay, seen to give way during the extension. Secondly, after the bandage was removed, the patient was held in the vertical posision, and his limbs were ascertained to be of equal length. Thirdly, he was able to rotate, to move the foot forward, backward, and laterally.

As negative proofs, his walking about the ward, and the like length of the limbs while he remained in the hospital, may be adduced.

I have to regret that I discharged him so soon. I did so, because I believed that he would go without permission. I mentioned to him the necessity of being cautious and quiet. His friends neglected to report to me his state. To your junior readers the lesson may be useful.

I remain, sir,
Your obeidient servant,
WILLIAM GILL.

Liverpool, Aug. 9, 1834.

NITRATE OF SILVER FOR EXCORIATED NIPPLES.

To the Editor of the Medical Gazette.

SIR,

ON perusing the Medical Gazette of last week, I observed a letter from Dr. Hannay, of Glasgow, relative to the application of the nitrate of silver to excoriated nipples, in which he says, "Hav-

ing at length fallen on a method of treatment which never fails to give immediate relief, and ultimately to effect a cure, I cannot withhold it longer," &c. &c. I beg leave to remark, that I have been in the habit of using this remedy, in similar cases, for several years past; and for the last three or four I have always named it in my lectures. My object, however, in noticing the communication of Dr. Hannay, is more for the purpose of offering my testimony in favour of the remedy, than to urge any priority of claim in the discovery; indeed, I believe there are very many practitioners who are as well acquainted with its value as ourselves, and who, probably, as often employ it.

I have the honour to be, sir,

Your obedient servant,

GEORGE JEWEL.

Sackville-Street, August 12, 1834.

ON DR. HENRY'S REPORT ON THE PHYSIOLOGY OF THE NERVOUS SYSTEM.

To the Editor of the Medical Gazette.

SIR,

I HAVE always felt much interest in the phenomena of the nervous system, and read every thing relating to its laws which comes in my way.

It is remarkable, that most accounts of the present state of our knowledge respecting it, exhibit errors and defects which we should hardly expect to find in those who volunteer to inform others. I shall not detain you by endeavouring to account for this fact, further than by observing that I believe it, in a great measure, arises from the complicated nature of the subject; and writers professing to give a view of the whole, while their minds are directed chiefly to certain parts, which, for some reason or other, have particularly attracted their attention. It certainly does not in general arise from any wish to misrepresent, because the class of writers most open to this censure are those who cannot have any interested motive for concealing the truth; I mean those who are only stating the experiments and conclusions of others, and therefore can have no private interests to serve.

These observations are suggested by Dr. Henry's Report on the Physiology of the Nervous System, published in the two last numbers of the Medical Gazette. It is not my intention to enter into a critique of Dr. Henry's paper, either to point out the instances in which he gives a correct and able view of the points in question, or all the objectionable passages which may be found in it; but merely to point out a few striking defects and misconceptions, that his communication may not be received by those of your readers whose attention has not been particularly directed to the subject, as a correct account of the present state of our knowledge respecting it. I shall not observe any particular order in pointing out the inaccuracies I am about to mention, but take them as they occur to my memory, and state their proofs in as few words as I can.

He makes no attempt to distinguish the sensorial and nervous functions, which introduces much confusion into many parts of his paper, particularly in his observations respecting the medulla oblongata, which in one passage he says has never been supposed a seat of sensation, while in other passages he refers to experiments which proved the existence of sensation after both the cerebrum and cerebellum were removed. Where else can sensation, after the removal of these organs, exist, if not in the medulla oblongata?

The functions of the ganglions, Dr. Henry says, is at present only a matter of conjecture. In the 359th and following pages of the second volume of Dr. Roget's Bridgewater Treatise, he will find an explanation of the nature of their functions, which it would be difficult to controvert; and Dr. Henry gives no reason for believing that Dr. Roget's statement is incorrect, nor indeed does he seem at all aware that any experiments have been made which tend to unfold the nature of their functions.

The error above mentioned, namely, his confounding the sensorial and nervous functions, leads him into much confusion respecting respiration; in which, among other omissions, he forgets that M. le Gallois has proved that the spinal marrow is capable of its functions, independently of the medulla oblongata. But to enter into the sub-

ject of respiration would lead to too long a discussion in such a communication as the present; and I shall only observe, that the difficulties respecting this function, so correctly pointed out by M. le Gallois, again present themselves, when the influence of the sensorial power is overlooked.

He adopts Dr. Alison's extraordinary opinion respecting the indigestion occasioned by the division of the par vagum, and other derangements of secretion, from the division of nerves; namely, that they arise from the loss of sensibility. After the division of the par vagum, the appetite is for hours as keen as before it, proving the sensibility of the stomach to be unimpaired; and neither he nor Dr. Alison seem aware that the same failure of digestion may, through the ganglionic system, be produced by destroying considerable portions of the spinal marrow, while all the connexions between the brain and stomach, and consequently the sensibility of the latter, remain entire.

Besides, these writers forget that the effect of depriving secreting organs of their nervous influence is not merely that of deranging their functions, but their structure also. Will they ascribe this effect to impaired sensibility, or will they maintain that the derangement of structure is not the effect of the continued operation of the same cause, which in the first instance deranges the function?

Dr. Henry refers to the experiments of M. Legallois, respecting the relation which subsists between the spinal cord and the heart, and points out where he has been refuted by succeeding physiologists; but he does not point out where, in a point of no small consequence in the animal economy, the results of his experiments have been confirmed by them; for although M. le Gallois was wrong in supposing that the heart derives its power from the spinal cord, he was right in believing that it is wholly under the influence of this organ. The results of the experiments by which he demonstrated the important fact, that through the spinal cord the power of the heart may be so enfeebled as instantly to destroy the circulation, have been fully confirmed.

But the following is perhaps the most remarkable error of a writer whose attention appears to have been so much directed to the functions of the nervous system. "The function of the spinal cord," he observes, "is simply that of a *conductor* of motive impulses from the brain to the nerves supplying the muscles, and of sensitive impressions from the surface of the body to the sensorium commune." Whence then comes it that the removal of any considerable portion of the spinal cord deranges the functions, as well as the structure of the vital organs, precisely in the same way as injury done to the par vagum, and that the functions of both the stomach and lungs are always deranged by extensive disease of the spinal cord. Does not the spinal cord contribute to the formation of the ganglionic system, on which so many of the functions of life depend? Here I beg leave again to refer him to the excellent treatise of Dr. Roget above mentioned. It has even been proved by direct experiment, that the vital, which must be regarded as the most important functions of the spinal cord, are unimpaired by its complete disunion from the brain, cerebellum, and medulla oblongata.

I have said enough for the purpose I had in view, and shall only add, that it is impossible to give a distinct account of the nervous functions, without in the first place drawing the line of distinction between them and the functions of the sensorial system; and, in the second place, pointing out in what instances the former are influenced by the latter.

I am, sir,
Your obedient servant,
S. Y.

August 18, 1834.

MEDICAL GAZETTE.

Saturday, August 23, 1834.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."
CICERO.

PARLIAMENTARY REPORT ON DRUNKENNESS.

EVERY body has heard of the worthy people of Lagado, and their academy of projectors—whose schemes for extracting sunbeams from cucumbers, calcining ice into gunpowder, rendering fire malleable, and converting the most unsa-

voury substances into wholesome food, our friend Gulliver has so faithfully described; but few, we believe, have been aware that, in our own country, we possess some able projectors of the same sort: we were not ourselves conscious of it until we saw the late report of the Select Committee of the House of Commons, appointed to inquire into the state of drunkenness in the United Kingdom. Those who had the chief hand in getting up this document—a knot of Benthamite philosophers, as they have been represented to us—have accomplished a feat which puts them on a fair footing of rivalry with their Lagado brethren. So virtuous a horror do they entertain of the *abuse* to which distilled liquors are liable, that they have hit on a number of notable expedients, to do away with their *use* altogether; and comfortably do they look forward to the day when distillation in England shall be no more. Death in the dram-bottle is their motto; for they hold that "the wholesome and nutritious grain given by a bountiful providence, is converted by distillation into a poison;" whereas *they* would have all grain converted into bread; and they inform us, that "human beings are transformed, by intoxicating drinks, into *excrescences of corruption and weakness*, which must be cut off and cast away from the community, to prevent the gangrenous contamination of the whole frame!"

Are we in the regions of romance or reality? And is this truly a report ordered to be printed by the House of Commons, for the benefit of their collective wisdom, and on which legislative provisions are to be founded? A greater pack of rubbish and solemn nonsense has seldom fallen in our way; nor, perhaps, has there been so odd a specimen of parliamentary reporting published since the days of Barebones and the Puritans. It is filled not only with absurdities, but with the most gratuitous assertions—contradictory, frequently, of many of the tenets of the philosophers who drew it up—as if themselves under the influence of some Circean potion, or "intoxicating drink," when penning the precious farrago. We are all, for example, accustomed to hear from the radical school, when it suits their purpose, most edifying discourses on the great physical force of the people—on the wonderful achievements of the schoolmaster—and the decided march of intellect which is taking place throughout the country. Here, however, the story is the very reverse: we are told of the great *diminution of the physical power and the longevity of the British population*—all by drunkenness, causing the decline of health, and the progressive decay of the bodily and *mental* powers; nay, "intemperate parents," they tell us, "give a taint to their offspring even before birth, and *the poisonous stream of ardent spirits is conveyed through the milk of the mother to the infant at the breast;* so that the fountain of life through which nature supplies that pure and healthy nutriment of infancy, is *poisoned at its very source*, and a diseased and vitiated appetite is thus created, which grows with its growth, and strengthens with its increasing weakness and decay!"

Here, at all events, we have a curious piece of information—one which physiologists would do well to lay up in their note-books—namely, that some mothers have the surprising property of suckling their children with a sort of milk-punch! This comes of employing, in inquiries of this nature, great philosophers who do their work on abstract principles—like the tailors of Laputa, who took measure of their customers with a quadrant and compasses, and would have brought home clothes made to fit exactly, had they not mistaken a figure in the calculation. We know not how far the Drunkenness Committee may have gone wrong

in their calculations,—for we have not seen the data on which their report is founded; but we confess we cannot help distrusting most of the results they have arrived at, especially when we find them so pathetic and moral in their deductions. Michael Cassio, we recollect, was never so moral as when Iago plied him with bumpers. Hear one more of their "consequences." "The retardation of all improvement, inventive or industrial, civil or political, moral or religious; the hindering of education, the weakening of good examples, and the creation of constant and increasing difficulties in the propagation of sound morality and the sublime truths of the Gospel, both at home and abroad, according to the testimony of teachers, pastors, and others, examined by your committee; the sum expended in intoxicating drinks, in the city of Glasgow alone, being stated by one of the witnesses from that neighbourhood to be nearly equal to the whole amount expended on public institutions of charity and benevolence in the entire United Kingdom."

Nor is this all. It seems, according to the same grave authority, that our army and navy owe their *inefficiency* to this vice—intemperance being "the canker-worm that eats away their strength and discipline to the core; it being proved, beyond all question, that one-sixth of the effective strength of the navy, and a much greater proportion of the army, are as much destroyed as if the men were slain in battle, by that most powerful ally of death—intoxicating drinks!" We can well conceive how this must startle most people, who have thought that our army and navy had gained all their successes at a time when they were much more addicted to grog than of late has been their wont. The announcement is serious, both as it concerns the nation at large and its efficiency in the day of battle. But how do our philosophers propose to rid us of this misfortune? Project worthy of Lagado! By *throwing open the gin-shops*, like those of the butcher, baker, and fishmonger, "in order that the interior of such shops may be seen from without, and be constantly exposed to public inspection in every part."—Drunkards are no longer to hide their maudlin heads, but to show them for the edification of the young Spartans of Holborn-Hill and St. Giles's. A thought, however, strikes us. Suppose the owners of the gin-palaces should, on this account, only increase their attractions the more, by displaying to the public gaze their customers stretched on ottomans and elegant Grecian lounges—no great extension of their present magnificence: what becomes of this wise project then?—and how will the committee counteract so tempting an allurement? Possibly their counter-charm is the following, which is the summing up of their "immediate remedies:"—"A national system of education, which should embrace as an essential part of the instructions given by it *to every child in the kingdom, accurate information as to the poisonous and invariably deleterious nature of ardent spirits* as an article of diet in any form or shape, and *the inculcation of a sense of shame* at the crime of voluntarily destroying or thoughtlessly obscuring that faculty of reasoning, and that consciousness of responsibility, which chiefly distinguish man from the brute, and which his Almighty Maker, when he created him in his own image, implanted in the human race, to cultivate, to improve, and to refine, and not to corrupt, to brutalize, and to destroy!"

Was ever such a tissue of cant and impertinence in a document drawn up on such a subject? But it may be asked why we notice it at all. Simply, then, because it is professedly bolstered up on medical authority. The reporters

say they have the countenance and support of *medical men* for the statements which they lay before parliament. We must be excused if we have our doubts on this head. It seems to us scarcely possible that facts elicited from medical witnesses could give colour to the many ridiculous things in the report. But whether this be the case or not, the rumour of such a document being propped up by medical authority, cannot raise the profession much in the estimation of the better informed part of the public. All medical men are certainly ready to allow that the habits of dram-drinking and drunkenness are vices of the very worst description; but nobody of common sense would for this reason go to the absurd length of proscribing "the wine that maketh glad the heart of man," or the spirits which must be its substitute with those of humbler means — when either are moderately used—"to cheer, and not inebriate." It is glaringly ridiculous and incorrect to state in the unqualified manner they do, "that ardent spirits are *absolutely poisonous* to the human constitution,—that in no case whatever are they necessary or even useful to persons in health—that they are *always, in every case, and to the smallest extent, deleterious, pernicious, or destructive;* according to the proportions in which they may be taken into the system." With all deference to the philosophers on the Committee, we deny that they have any sound medical authority for such a statement. No medical man, in giving his evidence, could possibly forget the value of spirituous liquors, taken in moderate quantity, to counteract the effects of a cold and moist atmosphere, exposure to pestilential effluvia, in uncleanly occupations, in a damp military encampment, and occasionally in the circumstances of a temporary destitution of food. Even Hufeland, who is such a dietetician, and who so strongly enforces the means of attaining long life, cautiously admits, in speaking of wine, that "to render it friendly, and not prejudicial to life, it must be drunk daily, but always in moderation; the younger the person is, in less, and the older, in greater quantity. It is best (he says) when one considers and uses wine as the seasoning of life, and reserves it for days of mirth and recreation, to enliven the friendly circle."

Dr. Paris, too, who seems to condemn, as much as any man, the *abuse* of spirituous liquors, thinks it not improper to admit that "there exists no evidence to prove that a *temperate* use of good wine, when taken at seasonable hours, has ever proved injurious to healthy adults." Nor, might we add, is there evidence to prove that the habitual use of even considerable quantities (not absolutely excessive) has been prejudicial to health and longevity. Instances are constantly met with, of persons arriving at a goodly old age, though in the habit of indulging freely, every day of their lives, in "potations pottle deep." The same, indeed, may be said of the habitual use of opium. But on this we will not dwell; for we may probably be told that these are cases in which longevity has been attained through the strength of a good constitution, and *in spite of* the intemperance which would otherwise have cut them off. It is sufficient, however, to shew that evidence on so questionable a point should not be given hastily—especially where the habits alluded to do not reach to palpable excess,—which, after all, is but a relative thing, depending on the strength of each person's constitution.

On all these points, the Drunkenness Committee seem to have had no sober information: we cannot otherwise account for their putting forth such statements, worked up, as they are, in such a ludicrous manner by their "abstract principles." Had the sapient commit-

tee-men not ventured farther than to suggest the means of begetting a little more *temperance* in society, some good might have been derived from their labours: but in preaching up the necessity of perfect *abstinence* from the things that Providence presents to us, they betray an ignorance of human nature which unfits them to be the guides even of the most simple. It is surprising that the House of Commons should have given even its tardy consent to have the report printed; for though there are, undoubtedly, some few good suggestions in it—such as that the duties on tea, coffee, and chocolate, should be reduced—that the public recreation and the public health should be more consulted than it is—and that the facilities for acquiring knowledge should be more diffused—yet the general absurdity of the conclusions (and be it observed that we have noticed only a few of them) will ever render it, as a document, contemptible and useless. Maugre the philosophy of Tothill-Fields, until the Millenium arrive, we fancy mankind will not be taught to abstain from the product of the alembic and the fermenting vessel. Every people on the face of the earth have something of the kind afforded to them by bountiful nature; and as well might it be attempted to restrain them from the use of salt as from the spirit that cheers them. Let the water-drinking ascetics of the temperance societies preach and practice to the top of their bent; still we feel assured that the more reasonable members of the community will never be persuaded to follow the recommendation of the Committee, and utterly to cast away what, in moderation, "maketh glad the heart of man."

PARLIAMENTARY MEDICAL REPORT.

As we presumed, notwithstanding the announcements in the newspapers and journals last week, *no Report* on the state of the profession has been presented to parliament by Mr. Warburton. A form necessary to prevent the whole of the proceedings from being rendered unavailable has been gone through, and the Committee is to be reconstituted next session, when a legislative measure will doubtless be introduced.

MR. BRODIE'S ELEVATION TO THE BARONETAGE.

His Majesty has been pleased to raise to the baronetage *Mr.*, now *Sir*, *Benjamin Collins Brodie*. The place which the distinguished individual alluded to has long held in the estimation of the public, as well as of the members of his own profession, renders the propriety of his selection for this honour so conspicuous, that we feel assured its announcement will be received with undivided approbation.

THE FACULTY OF PARIS—M. VELPEAU.

The concours for the chair of surgery, vacant by the death of Boyer, has closed, and M. Velpeau has received the appointment. No nomination by the concours has ever, perhaps, been so generally satisfactory since the time Dupuytren was elected.

ANOTHER DEATH FROM MORISON'S PILLS.

Friday sennight, Mr. Best, Coroner for Worcestershire, held an inquest at Pershore on the body of Rebecca Cross, aged 15. It appeared in evidence that the mother of the deceased had given her Morison's pills, and that inflammation of the bowels took place, occasioned, in the opinion of Mr. Davies and Mr. Woodward, surgeons (who were called in to attend her) by taking these pills. A great part of the bowels was ulcerated through, and mortification had taken place to a great extent. It was not proved where the pills were bought. There was a difference in opinion among the jury on the case; but after being locked up for some hours, twelve out of the sixteen returned a verdict, that "Rebecca Cross died from mortification of the bowels, occasioned by taking two pills, called Morison's Pills." —*Morning Herald*, August 16.

CLINICAL OBSERVATIONS

ON SOME

DISEASES OF THE NOSE.

Given at St. George's Hospital,

By Mr. Hawkins.

1. *Simple Inflammation of the Schneiderian Membrane.*
2. *Chronic Thickening of the Schneiderian Membrane.*
3. *Obstruction and Closure of the Nostrils.*
4. *Diseased Secretions of the higher part of the Nose.*
5. *Ulcers of the Cartilaginous Septum.*
6. *Ulcers of the Schneiderian Membrane.*

James Beach, a boy about nine years old, was admitted on the 9th of July; and our notes give us this history:—He states that about four months ago a large pair of scales fell from the height of several feet upon his nose, and cut it on the outside severely (though there is little mark of this at present), and that this was followed by a good deal of bleeding both from the wound and from the nostrils. A fortnight after this he fell from a donkey, and was unlucky enough to bruise his nose again, against a tree. A week afterwards he began to feel a good deal of pain in the nose, from which there has been a discharge, and the nostrils have frequently bled a good deal. On his admission the pain had lessened, but he still complained of a good deal about the middle of the nose, where the cartilages and the ossa nasi are joined, which part seemed a little elevated. In the interior was an opening leading through the septum, which was large enough to admit the finger, around which there was some ulceration and redness of the mucous membrane. His health was apparently not very good, though with nothing particular to complain of.

There is another patient, a girl of 15, Ann Paisley, who was admitted as long ago as February 19th, with ulceration of the nose, much of which had been destroyed before her admission. This ulceration of the nose began three years before, but had been spreading more rapidly during the last three months. It seemed that in this case, also, the disease had commenced by an ulcer in the septum, which spread till the opening was large enough to admit the finger from one nostril to the other; and that about a year after the ulceration commenced in the interior, an ulcer formed in the ala also, both parts having since ulcerated till there is the great destruction which you now see, the exact appearance of which, however, I will not now dwell upon.

There is also another boy, in whom, just as in the last patient, an ulcer formed first in the septum, then affected one ala, and afterwards the other, on the inside, and finally spread to the skin of the outside, in the same manner as in Paisley, and with a somewhat similar appearance.

The occurrence of these cases, and of some other instances of diseases of the nose, has induced me to make a few remarks to you to-day, on some of the diseases of the Schneiderian membrane, and in another lecture, perhaps, on some affections of the outer part of the nose, so as to give you a slight sketch of their connexion with each other.

I. The most simple of these, as in most of the textures of the body, is common inflammation, with which we are all familiar, in the shape of catarrh or coryza; but in fact, similar inflammation may take place from other causes also, without the usual affection of other contiguous mucous membranes. You observe, then, as you are well aware, just as in inflammation of other mucous membranes, a great increase of its natural secretion, and discharge of watery mucus, acrid, and excoriating the parts around, and you see the membrane itself red, and inflamed, and swollen, the consequence of which is the loss of smell, obstruction to the passage of tears, impairment of speech, and so on; and if the inflammation affects the higher parts of the nose, a good deal of headache, and local tenderness, and pain, and considerable febrile disturbance. Then, if the case goes on well, the discharge becomes in a few days thick and viscid, or purulent and yellow; and this lessening by degrees, the membrane again becomes healthy. It would scarcely be necessary to allude to such a case, but for this caution to you, that if simple inflammation, from any cause, lasts a long time, and a person has a predisposition to frequent attacks of inflammation, the foundation may thus be laid of more serious mischief. To shew you to what an extent the discharge from inflammation may reach, I may mention that I have met with a case, in which not less than a quart of watery fluid was said to be discharged daily. Neither is it necessary to dwell upon the treatment, as a calomel and saline purgative will generally be all that is required; or, if there is fever, a warm bath, with a little Dover's powder in saline mixture, and locally steaming the nostrils, or snuffing up warm water, so as to foment the affected membrane. To check

the disposition to this local inflammation from slight causes, you should employ tonics, such as bark, or bitters, and alkaline medicine, and you should check the secretions, and invigorate the membrane, by the use of astringent solutions, such as common tea, or oak bark.

II. You will be more frequently consulted, however, with regard to a chronic inflammation of this membrane, or scrofulous thickening, for I believe it scarcely occurs, except in children or young persons, with scrofulous disposition; and very often it is accompanied with discharge from the ears, enlarged glands, or other symptoms indicating the state of system on which it depends. The thickening produced by this inflammation of the membrane is so great, that it often hangs down into the nostril, and resembles the fleshy polypus of the nose. A little girl of about six years of age was brought to my house very recently by a medical man for polypus, which proved on examination, however, to be only the state of membrane I am alluding to. You can generally distinguish it at once by this circumstance (besides the different form of the prominence), that the thickening is seen just as often on the septum as on the outside, and it frequently takes place on the whole circumference of the nostril, while polypi are always, or almost always, on the outside only, deriving their growth from the several spongy bones of the nose.

The little girl I spoke of used to snore violently at night, which is frequently the cause of the disease being first discovered; she breathed with difficulty, and from the mouth being constantly open, was tormented with constant thirst at night, leading her frequently to ask for water to drink; and from the same cause the tongue was harsh and dry, and the lips dry and chapped; and she spoke through the nose, as it is called—that is, she could not speak through the nose at all, the air being unable to pass freely through the nostrils. On examining the nose, the Schneiderian membrane was found red, and vascular, and thickened, especially on the septum, and there was a little mucopurulent discharge from the nostrils.—Sometimes the discharge is fetid, and the membrane slightly excoriated, or ulcerated; and if the disease is extensive, the little patient often complains of soreness and tenderness of the outside of the nose; and it appears flattened and broader than natural. Besides the other scrofulous symptoms which I alluded to, I have several times seen this thickened state of the Schneiderian membrane accompanied with an analogous enlargement of the lip; that is to say, half the upper lip, perhaps, is double its proper thickness, and hard and somewhat painful. If so, both the affections are cured by the same means.

The nature of your medical treatment is clearly indicated in these cases; you are to exhibit a combination of alteratives with tonic medicine. Let a young child take of hydrargyrus c. creta, four or five grains, with six or eight of rhubarb every other night, or a little rhubarb with half a drachm of sulphate of potash, or one drachm of tartrate of potash twice a week, or occasionally a dose of calomel with a few grains of rhubarb; and along with these medicines give some light tonic: one of the best for children is about twelve grains of calumba powder, with eight of carbonate of potash; or very often steel will agree very well, one drachm, or one and a half, of vinum ferri, three times in the day; or twelve grains of calumba, with ten of carbonate of iron, twice a day; or you may give some tincture of bark, with small doses of oxymuriate of mercury. Dissolve one grain, or half a grain, of oxymuriate in two ounces of tincture, and give the child a tea-spoonful twice a day. This is a medicine which was recommended by Sir Astley Cooper in scrofulous affections, and which I often give with advantage. To an adult you may give, with the same view, three or four grains of blue pill every night, and some bitter infusion, with carbonate of potass. These medicines must be given, as in all such scrofulous affections, for a considerable time, with occasional intermissions of a week or two; for though you may always expect to cure the disease, the disposition will sometimes last for many months, or even a longer time.

Locally, you will direct some stimulating application, such as the nitrated quicksilver ointment, diluted at first with a third, or half its weight, of spermaceti ointment, and subsequently undiluted. This should be slightly softened, and applied by means of a camel's hair pencil to the part every night and morning; or if the nostril is much obstructed, or the thickening extends far up, a little lint may be wrapped round a bougie, and the ointment applied by this means. You may in the same way apply the red or white precipitate ointment, if the other does not agree; or a solution of lunar caustic, five or eight grains to an ounce of water, may be used with a pencil. If there is a good deal of discharge from the nostrils, you may check this before having recourse to the ointments, by some lime water, or a solution of alum (a scruple, or half a drachm, to an ounce), which may be snuffed up the nostrils; or, lastly, where there is a great degree of enlargement of the membrane, you may snip off a small

piece with a pair of scissors, and afterwards use the stimulating applications which I have mentioned.

III. When inflammation has been extensive, the nostrils may be almost, or entirely, obstructed by the thickening of the membrane, or by adhesion. A girl of about 12 years of age was brought to the hospital the day before yesterday, with the membrane so much thickened by chronic inflammation of several years standing, that the air could scarcely pass through at all. She had a good deal of pain in the nose and forehead; the nose was swelled and tender, and the inflammation of the interior of the nose had recently occasioned an erysipelatous inflammation of the outside of the nose and of the face. In such cases as this, besides the local and constitutional means I have described to you, you will do well to keep a hollow tube, such as a piece of catheter, with some lint or leather round it, in the nostrils for a short time, to procure a more rapid removal of the thickened membrane, and enable the upper parts to be reached by your stimulating applications; the outside of this tube, or rather the lint upon it, being smeared slightly with one of them.

But further, if there has been much purulent discharge from the inflamed membrane, with a little excoriation or ulceration of the surface, the nostrils may be totally obstructed by adhesion between the opposite surfaces, which have been kept in contact by the subjacent swelling. I have several times seen this take place in one nostril, or partially in both; but I saw, with Mr. Fernandez, not long ago, a little child, about two years old, which had total obstruction of both sides, each nostril being closed by what now looked like a transverse thin membrane, about half an inch within the orifice. This obstruction was situated just at the entrance of the lachrymal ducts, which is the usual situation of such adhesion; so that the openings of the nostrils were quite dry, the tears in a great measure entering the nose behind the membrane, though they partly overflowed upon the face. This obliteration might possibly have been congenital, for there was no history of the affection; but as it had only recently been discovered by the child's snoring, so as to disturb the others who were with it, and from a scrofulous affection of the eyelids and conjunctiva, which had produced total blindness, I have no doubt that it had been the consequence of a similar scrofulous disease of the nasal membrane. In this or any similar case, an incision through the membrane is very simple; nor, with a little knowledge of the anatomy of the part, can there be any difficulty in dividing the adhesion of a much broader surface, a little more caution only being necessary in the use of the bistoury than where there is only a thinner partition. Subsequently to the incision, you will keep the nostril pervious for a little while, during the cicatrization, by a metallic tube or portion of catheter.

You would scarcely think, perhaps, that the obstruction of one nostril would be of much consequence; but I was only yesterday informed of a case in which one nostril was nearly closed by the septum being turned to one side, (as is often the case without any external deformity), and by the membrane on it being thickened: the consequence of this was so much inconvenience and distress, that a small hole was punched in the septum, which greatly relieved the patient, by allowing the tears and mucus, and the air, to pass freely from one nostril to the other.

IV. I have mentioned to you the secretion of considerable quantities of water, and mucus, and pus, from the inflamed Schneiderian membrane; but, besides these, you will meet with very singular secretions, when the upper part of the nose is affected, including especially the several spongy bones. A woman is now attending as an out-patient of mine, who complained of having had, for six months, a good deal of pain in the nose and head; loss of smell, (though with the sense of an offensive smell in the nostrils); obstruction in breathing and speaking, snoring and thirst; and she was evidently much irritated by this state. There seemed to be some vesicular polypi high up in the nose; but, on examination, this proved to be a tough, tenacious mucus, covering a softened and relaxed state of the membrane, which hung loosely, and allowed the probe to perforate it; and in one part there appeared to be a trifling exposure of the vomer. This has been much relieved in about a fortnight, by merely the application of lime-water, snuffed up the nose, and nitrated quicksilver ointment, with one or two doses of purgative medicine.

I was recently shown some small bodies, which were more like half-formed cartilage, both in external appearance and on a section, than any thing else, and which were exactly of the shape of the superior spongy bone. These used to come away from time to time, either by the nostril or from the throat, in a delicate person, subject to hæmoptysis, the nostril being obstructed for a few days previously to their discharge; and one or two thus separating at a time during many years.

Perhaps you may have seen some bodies of a similar shape, which have been formed

in the nose of an out-patient of the hospital, who had suffered for a considerable time from pain and obstruction in both nostrils; and on one side was a projecting body in the nostril, with some external swelling; the appearance of which altogether was exactly like that of a malignant polypus; at last some body projected lower than usual in the nostril, and a surgeon pulled out several portions of substance like chalk in consistence, and exceedingly fœtid, and which were also in shape exactly like the spongy bones; they were probably composed of phosphate, or perhaps carbonate, of lime, with fœtid mucus, secreted from the upper spongy bones.

I conclude, in such cases as these, that the upper spongy bones were in part diseased, and that the peculiar bodies were secretions from the membrane covering them, mixed with portions of the membrane itself, and dropping off from time to time to be again succeeded by others; and, from the small quantity of bone affected in the first of the cases I have mentioned, compared with the extent to which the membrane of the upper part of the nostril was diseased, I imagine the disease of the bone, (which was evidently not common caries), may be secondary to that of the membrane, which may be an inflammation differing from that met with in the lining membrane of the general cavity, in consequence of the peculiar texture of the æthmoid bone.

The last case was much benefitted by a combination of sarsaparilla, with arsenic and liquor potassæ. You may try, then, either this or bark, or some other form of tonic, with occasional purgatives; and locally you may employ lime-water, black-wash, a solution of oxymuriate of half a grain to the ounce, or a grain of sulphate of zinc, or a scruple of alum, or some similar lotion, which in all diseases of the higher parts of the nostrils, should be occasionally injected with some force by means of a syringe, besides being frequently snuffed up the nostrils.

V. We now come to those cases in which there is almost from the first some breach of surface by ulceration; and, first, let us consider ulcers in the cartilaginous septum, of which the boy, whose case I read to you, affords you an example. In him the affection, you observe, has been the result of inflammation arising from a blow; but an ulcer through the septum frequently takes place from other causes, as in Paisley and the other case I mentioned to you; and the patient is frequently not aware of its existence for some time.

A woman came under my care for ulceration of the throat, in whom I found, at the same time, a hole through the septum three-quarters of an inch in diameter, the edges being still ulcerated, the woman herself only thinking that she had frequently caught cold in the preceding six months: and I have often seen similar perforations, cicatrized and well, in persons who did not know that such a thing had previously been produced. In fact, as long as every part of the circumference of the cartilaginous septum is unimpaired, the form of the nose is not at all altered, and no possible inconvenience can arise from the perforation. If, however, the part of the cartilage adjoining the cutaneous septum is destroyed, the end of the nose will drop down a little,—as you saw in Paisley, on her admission; or if the upper part is removed, then an indentation, with turning up of the end of the nose, will be the result.

In some cases, however, if you watch the disease in its early stage, you will find the patient complain of some pain in the nose; the end of the septum, especially near the junction of the cartilage with the vomer, being red and inflamed: then there forms on one side or other a kind of pustule or white spot, which ulcerates, and leaves a little hole in the membrane and cartilage: then a similar little swelling may perhaps be seen in the other nostril, and in a day or two more a hole will be perceived through the septum; or more frequently the ulcer will spread from one side to the other; and a few days after you first observe it on one side, you find that the little hole leads from one nostril into the other. This continues to spread, especially if it is irritated, as will be usually the case by the patient's finger; and the hole enlarges to the size you see it in Beach and the other boy, with a red margin, and a little purulent and sanguineous discharge. There appears, then, in such cases, to be an inflammation of a portion of the cartilaginous septum of the nose, with a little abscess on its surface below the lining membrane, like the abscesses beneath the periosteum of a diseased bone; or perhaps there may sometimes be a small abscess in the cartilaginous septum itself, ulcerating towards both sides at the same time. I believe, however, that this latter form is rare, compared with the ulcer in the substance of the membrane, or a small ulcer of the surface, both of these forms spreading secondarily to the cartilage below.

Now very often the affection seems to be almost entirely local, so that the application of nitrated quicksilver ointment, diluted or not, according to the degree of pain it produces, or one of the mercurial precipitates in ointment will be sufficient,

with an occasional purgative, to effect a cure. At other times, however, you find some derangement of the general health, which requires to be attended to. In Beach there seems to be nothing wrong, besides some appearance of a scrofulous constitution in the countenance; but in Paisley there has been considerable alteration of health. What I have done, therefore, for Beach, has been to give him, first of all, two doses of calomel and rhubarb, and then I ordered some Calumba powder and carbonate of potash, twice a-day. Locally, he has only used black-wash; and under this plan all pain has gone; the bleeding has ceased, and the ulcer appears to be healing fast. I will speak of the treatment of the other case hereafter, on account of its complication: the general principle, however, is to be the same in all such cases,—tonics and alteratives of some kind. Sometimes, in a grown-up person, you find some degree of cachexia; and I have sometimes seen at the same time, with the hole in the septum, an ulcer in the pharynx, or a hole formed through the bony palate at the floor of the nostril, just like that through the cartilage of the septum. Give, then, to an adult, sarsaparilla and liquor ammoniæ, or sometimes small doses of blue pill, with gentian and potassa, or some other tonic, with good air and nourishing diet, and the disease will soon yield; in which respect it seems to be of a more healthy character than ulceration of the Schneiderian membrane itself, beginning on the surface.

VI.—We now come, then to the subject of ozæna, or ulceration of the Schneiderian membrane of the nose. I should observe, indeed, that you will find some surgeons employ the term ozæna to signify any ulceration of the nose, while others use it more strictly in its etymological sense—confining it to those ulcers in which the discharge is most offensive, and in which generally the bones of the nose are carious. In reality, however, there is no essential distinction between such cases; the difference is one of degree only—from a small ulcer of a portion of the Schneiderian membrane of one nostril, to an extensive ulceration of the whole surface of both nostrils; and lastly to those in which the ulceration of the membrane has spread to the bone below it, occasioning caries and ulceration of a considerable part of the bones of the nose.

These ulcers may begin in any part of the nostrils, and they secrete a purulent and generally offensive discharge, with occasional mixture of blood, which comes away with the natural mucus secreted in greater quantities than usual. The discharge sometimes becomes concreted into thick brownish crusts, which separate occasionally, or are picked off by the patient, and a superficial ulcer is exposed, with a smooth yellowish surface and a somewhat phagedenic margin, and the mucous membrane around is seen to be red and inflamed. There is some degree of pain in the part, and there is more or less pain and tenderness, and fullness, across the bridge of the nose; and the pain is frequently severe and extensive in the forehead and sides of the head, from inflammation of the membrane in the frontal and other cells; and there is a diminution or deprivation of taste and smell. If the ulceration is considerable, the health becomes very much affected; the patient being debilitated, emaciated, and in a state of constant irritation; the countenance pale, and expressive of much suffering; the pulse 120 or 130, small and weak, and irritable; and complaints are made of heat and flushing, with occasional chilliness, and pain in the limbs. One symptom that is almost universally complained of, is sleeplessness; so that I have known some persons assert that they have hardly slept above half an hour at a time for weeks together. These severer symptoms are chiefly experienced when the disease affects the upper part of the nose; nor is it extraordinary, if we consider the anatomy of the parts, that the patient should sometimes experience much disturbance of the sensorial powers, evinced even by epilepsy, mania, or a fatal effusion at the basis of the brain; though, if fatal, the patient seems to sink more commonly from weakness and irritation.

The causes of ozæna are evidently some kind of cachexia of the system; so that it is occasionally met with in scrofulous children—as in Paisley and the other boy, in whom the external disease was preceded, for a considerable time, by ulceration of the interior membrane of the nose, both on the septum and alæ nasi. A more common cause of extensive ozæna, however, is the poison of syphilis, or the abuse of mercury; so that you seldom now see the extensive destruction of the nose that used formerly to be observed, when syphilis was not so well treated as at present. It may occur, however, from any other animal poison—such as that of glanders, or from inoculation with putrid and foul meat. The consequence of this state of the system is, that from whatever cause it may arise, ozæna is often accompanied with loss of portions of the soft and bony palate, and extensive and foul sloughy ulcers of the pharynx and parts connected with it; with pains in the limbs; with eruptions of ecthymatous pustules, or ulcerating tubercles or rupia; such an eruption being sometimes confined to the ex-

ternal parts of the nose and adjacent parts of the face, from the immediate local irritation of the internal ulcers, but at other times extending over the remainder of the body. Such a combination of symptoms, in fact, is met with, as would some time since have been considered as decidedly syphilitic, but which may in reality be met with as a consequence of any poisonous or other cachectic cause whatever.

With this view of the case, then, it is little necessary to inquire minutely into the original cause of the complaint; since it is very seldom, indeed, right to adopt a mercurial course of medicine as an anti-syphilitic treatment, especially when mercury has already been given; and whatever may have been the origin of the ozæna, nearly the same plan of treatment will be successful. Sometimes, indeed, a cautious employment of mercury may be required, when it does not yield to other remedies, but its effects must be very carefully watched; for instance, one of the most successful cases of the kind was in a little child of about six months old, who was brought to me, and whom you may have seen last year, and who had not in the least grown since its birth; a more miserable object, indeed, could scarcely be seen. Ulceration and discharge from the nose had commenced when the child was six weeks old, and had totally destroyed the septum and much of the upper part of the nose, so that it was quite flat upon the face, and hardly had any orifice for breathing and for the escape of the copious purulent secretion that was present. About a month previous to my seeing it, a few stains had appeared about the thighs; not having, however, at all the usual colour of syphilitic eruptions, nor could I discover that either the father or mother had had any disease of this kind. The father had died of erysipelas, under my care, about a fortnight before. As the most probable means of saving the child's life, however, by stopping the disease suddenly and increasing the chance of absorption of chyle, I gave it half a grain of calomel and some compound cinnamon powder three times, and in a short time twice a-day, with some nitrated quicksilver ointment to be applied to the nostrils; the consequence of which plan was, that (without, of course, any evident mercurial effect) the ulceration nearly stopped, and the discharge was almost gone, and the child had grown more in three weeks than in the whole of the six months it had previously lived; the deformity, however, being, of course, irreparable. Unquestionably, however, the general principle on which the cases of ulceration of the Schneiderian membrane are to be treated, is the tonic system; and the best of the tonics that you can give is sarsaparilla. I give this medicine with perfect confidence that by far the majority of cases will be cured by it; and I am satisfied that the chief reason why many persons have little faith in its efficacy, is that it is not given in sufficient quantity, nor continued for a sufficient length of time. The common decoctions alone, in small quantities, cannot be relied on; the patient must take not less than twelve ounces of the compound decoction, with the addition of from one to two drachms of extract of sarsaparilla; and this must be continued from three to four months in old cases, and for several weeks after the disease is apparently cured. Very often your patients will get fat and well, and all local symptoms will be subdued in four or five weeks, and they will not submit to the necessary confinement and continuance of medicine after they think themselves cured. Such patients will be sure to have a relapse, but the same treatment again resorted to for double the time, will again cure the disease. Sometimes, indeed, a few weeks will suffice. A woman had suffered from ozæna a year and a half, and for half a year had had an ulcerated opening through the bony palate into the nose; experiencing much pain in the head and eyes, and throat, with considerable emaciation and disturbance of the general health. She was permanently cured in six weeks; but so short a course of medicine would be insufficient in most instances of so long standing. You may often, with great advantage, combine the sarsaparilla with twenty or thirty drops of liquor ammoniæ, or some salvolatile, when there is much nervousness and crethismus: sometimes with a drachm and a half of liquor potassæ, or some carbonate of potash. Sometimes alterative doses of mercury are of great service, with the sarsaparilla—one-fourth or one third of a grain of oxy-muriate daily, or three grains of blue pill every night; but not so as to affect the gums, and commencing some time after the use of the sarsaparilla; especially if the amendment first observed from the tonic begins to be less rapid, or there is a slight disposition to fresh ulceration.

In some persons other tonics will agree well: bark and nitric acid; nitric acid alone; a drachm of the strong nitric acid daily, in a pint of water, with some syrup. Whatever you give, however, must be accompanied with good diet and pure air, if possible.

Where there is the great restlessness and sleeplessness I alluded to, you may give your patient five grains of soap pill with opium, and three or four grains of extract

of colocynth, or eight or ten grains of extract of hyoscyamus, or conium, every night; and he will require occasional doses of purgative medicine, such as rhubarb or colocynth.

For your local applications there are none better than lime-water, or black wash, which may be snuffed up or injected several times a day, with very great comfort to the patient—the local pain being checked and the discharge diminished under their use; or you may direct a lotion consisting of about one-fourth of a grain of oxymuriate to an ounce of lime-water, or of common water, if more stimulus is desired; or half a grain of sulphate of zinc, or diluted red wash, or sulphate of copper. These lotions are especially required if the disease is high up in the nose, where other forms of applications cannot be employed; but if the ulceration is situated in the lower parts of the nostrils, you may use a peneil, or a bougie, with lint, with bismuth or oxyde of zinc ointments, when there is a good deal of irritation; and subsequently some one of the mercurial ointments I have before recommended, or stronger solutions of lunar caustic (eight or ten grains to the ounce, or any other astringent.

Where the bones are affected in consequence of the destruction of the Schneiderian membrane, producing caries below it, the case is much more tedious and obstinate, from the slowness with which the caries of these soft spongy bones yields to treatment, and the indisposition (except in more healthy individuals) to any exfoliation of the diseased parts. In these cases a relapse will necessarily take place, in many instances, and a succession, perhaps, of several courses of medicine will be required before the disease is finally subdued. It is seldom, however, that the complaint is actually fatal, except in a few of those miserably neglected cases that we meet with in half-starved individuals of the lower classes of society; it is seldom, indeed, that the complaint is seen in any aggravated state except in persons of this class.

It appears, then, gentlemen, from the few remarks which our time has allowed me to make to you, that some complaints which commence in the lining membrane of the nose, may spread to the outer parts, as in Paisley and some cases now in the hospital. In another lecture I may, perhaps, make some observations to you upon these affections, in addition to those I have now offered to you of the internal ulcers of the nose.

CASE OF

INCONGRUOUS TWINNING.

By Thos. J. Charlton, MD.
Of Bryan County, Georgia.

I was called to visit a woman belonging to Mr. Clay, of this county, who was said to be nearly nine months advanced in her third pregnancy, and to be suffering under a sudden affection of the womb, which her attendants supposed to be a prolapsus. On examination, I found the lower extremities of a fœtus in the vagina; it was extracted with no difficulty, and with the slightest exertion. Upon making a section of the umbilical cord, no blood issued, nor had there been any discharged from the uterus before I saw her, neither had she had the slightest pain. The symptoms which alarmed her, and occasioned my being called, was a sensation of faintness, which I attribute to apprehension of danger excited by the nurse. The fœtus was not putrid, but had evidently been dead some weeks; it was apparently one of the seventh month, and was in appearance, and to the smell, as if it had been preserved in pyroligneous acid. The viscera were much diminished in size, and externally it consisted merely of the integuments closely applied to the bones. As there was neither pain or flooding, I did not deem it proper to introduce the finger into the womb, fearing that the premature expulsion of another child, the motion of which I plainly felt on the application of my hand externally, might be caused by such interference. I directed her to be kept quiet, and that I should be immediately summoned, should any of the precursors of labour occur. For eight days she was without any symptoms; on the night of the ninth, labour came on, and the delivery of a child, healthy and of the full time, took place before I could see her. On examination of the placenta, I found the cord of the former birth attached to the side, and that of the latter to the centre.

I am aware that many cases have been mentioned by writers, of twins, born at remote periods from each other, but what I thought remarkable, and which has led to the report of the above case, was, the absence of pain, and, to all appearance, of contraction, in the expulsion of the dead fœtus. The uterus has been said to be "a life within a life," and the occurrence here mentioned would give some confirmation to its supposed powers of independent resolve, as well as action; for the casting off of the dead fœtus without the

usual means of pain and contraction, would imply, in some degree, a consciousness that such means were not fitted for the present case, as tending prematurely to expel its remaining contents: or, to speak more accurately, I may say that the above circumstances are additional evidences that all the organs, and most especially the uterine system, have the faculty of adapting themselves to contingencies and abnormal occurrences*.

CHRIST'S HOSPITAL—MEDICAL DEPARTMENT UNDER DR. ROBERTS.

To the Editor of the Medical Gazette.

Sir,

It is not consistent with my feelings to enter into correspondence with the Editor of a work of the avowed character of the Lancet; I have, therefore, to request, that you will insert in your Gazette the following letter, in reply to a letter contained in that publication of the 9th instant, relative to Christ's Hospital, signed by "A Governor," and dated July 1834.

I begin with that glaring and illiberal construction put upon the resignation of Dr. Roberts, the physician to that establishment, which the writer states to have been in consequence of the appointment of a special committee, to inquire into the efficiency of its medical department. Now it is a well-known fact, that Dr. Roberts resigned his appointment as physician to St. Bartholomew's Hospital, at the same time in which he relinquished his situation here, with the design of retiring from practice altogether, and of residing in the country; which is a sufficient answer to that unhandsome insinuation, especially as he had given, some weeks previous, notice to our Treasurer of this, his intention.

The letter proceeds to state, that his place was almost a sinecure, having had only eighteen patients in five years, of whom seven died, for which he received the sum of 300l. As there is great reason to believe, from its date, that this letter is the production of one of the special committee above alluded to, the writer ought to have known that Dr. Roberts's duty, in addition to his attendance upon the boys in the Infirmary, was to visit, when called upon, all the officers and servants belonging to the Hospital, in number somewhere about 150 persons. The writer, moreover, is egregiously mistaken as to the number of cases which have been under his care in the Infirmary. I am prepared to prove, from written documents, that he has attended, during the last five years, twenty-six boys, of whom fourteen died*, either there or after having been removed to the residences of their friends.

The physician of Christ's Hospital is not expected to visit the sick boys in our infirmary but in his consulting character, whenever the resident apothecary should have reason to consider a case to be either difficult or dangerous, (which will satisfactorily account for the large proportion of deaths, as above stated); and, under these circumstances, I am ready to bear my humble testimony to the ready assistance Dr. Roberts has always afforded me, and to the same attention, when at any time his services have been called for by the officers or servants of the house.

I should have supposed that the little occasion there has been for requiring the attendance of the physician, instead of being a subject of complaint by a governor, would have been the source of the highest gratification to him; as it is a convincing proof that the diet, care, and management of the boys are, as at present conducted, very well adapted to their health and comfort, and will admit but little improvement; as the average number of deaths, in the last ten years, has been only about one in 150 annually†. I am satisfied that an impartial public will be of opinion that the "venerable apothecary, in his 80th year," has not been deficient in his care and attention to the sick boys, although he may not be equal in professional qualifications to the "talented resident general practitioner," whoever he shall be, by whom the special committee may intend to supplant him.

Dr. Johnson has observed long ago, that there is no profession that does so much to serve the public gratuitously as the medical profession; and in charitable institutions, supported by uncertain contributions, physicians are never wanting who will render their gratuitous services; but in such a well-endowed establishment as Christ's Hospital the *honorary* services

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* American Journ. of Med. Sciences.

* The deaths above mentioned occurred between June 1829, and June 1834, and were caused by

Hydrocephalus	4
Scarlatina anginosa maligna	3
Phthisis	3
Obstipatio	1
Febris typhodes	3
	14

† See Medical Repository, vol. iii. p. 43.

of a consulting physician or surgeon ought not for a moment to be contemplated. The proposition is truly preposterous, nor have the managers or governors any right to expect that it will ever be acceded to.

Dr. Hue, the highly-respectable and well-qualified successor to Dr. Roberts, is stated, in the Governor's letter, to be a pluralist. If he be so, it is in the very laudable cause of charity; for I believe that the greater part, if not all, of his professional services are gratuitous, except at the two hospitals of St. Bartholomew's and Christ's. The principal advantages which belong to the former of these, arise from the pupils who attend the medical practice of it.

Having noticed such circumstances in the Governors' letter as come within my immediate department, I subscribe myself

Your obedient servant,

HENRY FIELD.

Christ's Hospital, Aug. 14, 1834.

IMPROVED SURGEON-DENTISTS' FORCEPS.

To the Editor of the Medical Gazette.

SIR,

IN the London Medical Gazette for April 19, 1834, and in Nos. 9 and 19, of vol. ii. of the Lancet, attempts are made by two individuals to claim the invention of *improved surgeon-dentists'* forceps for the extracting of stumps of teeth, of which Mr. Cyrus Fay was the original inventor, and for which he was rewarded with the large silver medal of the Society for the Encouragement of Arts, Manufactures, and Commerce, in the Adelphi, so long since as the year 1826. During the period of twenty years, that I have been a chairman of the Committee of Mechanics, in that patriotic and valuable institution, I have taken a considerable interest in forwarding his views of communicating his invention for the use of the public, through the instrumentality of the Society. I cannot, therefore, feel indifferent at these unworthy attempts to deprive him of the honour of being the original inventor of those valuable instruments, and trust you will please to afford insertion to these observations, in order that the public may be truly informed thereon. I may further observe, that the two individuals above-mentioned seem to affix a high value to the invention, as they are now actually contesting the priority of it. Mr. Fay has deposited two of his original instruments in my hands, for the satisfaction of the public.—I am, sir,

Your obedient servant,

THOMAS GILL,

Consulting and Advising Engineer, Patent Agent, &c. Savoy Depôt of Practical Science, 125, Strand, London.

Aug. 14, 1834.

QUACK PILLS.

THE celebrated Machiavel lost his life by quacking himself with a favourite nostrum—some vegetable pills resembling Morison's, but less drastic, as they contained no gamboge. Violent pains in the bowels carried off the author of "The Prince," in the 58th year of his age.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *Aug.* 19, 1834.

Abscess	1	Heart, diseased	3
Age and Debility	49	Hooping-Cough	10
Apoplexy	8	Inflammation	24
Asthma	5	Bowels & Stomach	14
Cancer	2	Brain	8
Childbirth	3	Lungs and Pleura	6
Cholera	67	Insanity	3
Consumption	73	Jaundice	2
Constipation of the Bowels	2	Liver, diseased	1
Convulsions	52	Measles	4
Croup	1	Mortification	6
Dentition or Teething	8	Paralysis	2
Diarrhœa	3	Small-Pox	9
Dropsy	17	Sore Throat and Quinsey	1
Dropsy on the Brain	19	Spasms	1
Dropsy on the Chest	1	Thrush	2
Erysipelas	1	Tumor	2
Fever	15	Unknown Causes	7
Fever, Scarlet	8	Stillborn	14
Fever, Typhus	2		

Increase of Burials, as compared with the preceding week . . . 45

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ *W. of Greenwich.*

Aug. 1834.	THERMOMETER.		BAROMETER.	
Thursday . 14	from 51	to 80	30·04	to 30·09
Friday . . 15	49	76	30·15	30·13
Saturday . 16	55	79	30·09	30·05
Sunday . . 17	57	78	30·01	29·97
Monday . . 18	58	75	29·92	29·87
Tuesday . . 19	58	75	29·80	29·72
Wednesday 20	54	73	29·67	29·63

Wind N. and N.W.; the latter prevailing.

A few drops of rain in the evening of the 20th; otherwise generally clear.

CHARLES HENRY ADAMS.

NOTICE.

Dr. E. S. Blundell's letter is an advertisement. Some of the newspapers, probably, as it is more in their way, would give publicity to reputed cures by "the magnetic fluid."

W. WILSON, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, AUGUST 30, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XLVII.

COMPLEX LABOURS—HÆMORRHAGE.

RETENTION OF THE PLACENTA.

At our last meeting I spoke of hæmorrhage occurring between the birth of the child and the removal of the placenta. I recommended you not to wait longer than an hour and a half for the expulsion of the mass, even if there were no sanguineous discharge; but at the expiration of that time to introduce the hand into the uterus, and remove it if necessary. But I also inculcated the caution, that if flooding came on to an alarming extent, no time was to be lost in emptying the uterus, that an opportunity might be given it of contracting itself perfectly.

It may, however, happen that you are fearful of removing the placenta yourself; that you are diffident in your own capabilities; that you are unwilling to act in the case entirely on your own responsibility, and desirous of obtaining the assistance of a friend. It is possible that you may somewhat restrain the discharge in the interval by keeping the patient perfectly quiet in the posture in which she has been delivered, cooling the atmosphere of the apartment, and applying cold to the hypogastric region, the pubes, and loins. But I strongly advise those of you who have paid the necessary attention to this department of medicine, after having candidly stated to the friends the perilous nature of the case, rather to trust to your own knowledge than to waste time by sending to any distance for assistance; because the danger is urgent and imminent; and because the lapse of a few minutes, never to be recalled, may depress the patient beyond the possibility of restoration.

About a year and a half since, I was sent for in the middle of the night to a lady, a few miles from London, whom I was to take charge of in her expected labour, but who had been seized with flooding suddenly before the term of gestation had expired. On arriving at the house I found her pale and faint from the loss of blood, and suffering under the pains of uterine contraction. I detected the placenta presenting entirely over the os uteri, which was dilating slowly. She was of a debilitated constitution, having been the subject of frequent abortions, and, to add to my embarrassment, was a much-valued personal friend. I stated to her husband the dangerous nature of the case, and the necessity for speedy delivery. He was kind enough to assure me of his confidence in the propriety of any means I might adopt, and at the same time offered, if it would be any satisfaction to me, to send to London for the assistance of any gentleman whom I might name. Had time permitted, I would most thankfully have requested my father's presence; but it was very evident, that if the discharge continued with any thing like the violence of the previous eruptions, the case would be hopeless long before his arrival; and I therefore proceeded immediately to my anxious duty. The lady was delivered with but little additional loss, but continued for some time under a state of faintness, and even suffered a slight convulsion. She rallied, however, and gradually recovered her former health. I have not the least doubt that if the operation had been delayed until assistance could have been

procured from town, we should have had to lament a fatal termination.

I urged you also, in cases where morbid adhesion existed, to use your best endeavours for the removal of the whole of the mass,—being persuaded that a woman can scarcely be left in a more perilous situation after delivery,—unless indeed extensive lacerations or contusions of the maternal structures have occurred during labour,—than with portions of a broken placenta left adherent to the uterine surface. The danger, indeed, is two-fold,—immediate and remote;—immediate, from hæmorrhage, produced by the inability of the uterus perfectly to contract its cavity, and to close its vessels, while any portions of the placenta remain within it; and remote, principally from the effects of putrefaction.

DISRUPTED PLACENTA.

When a portion of the placenta is left in utero, we generally observe violent after-pains returning at longer or shorter intervals, preventing sleep, and harassing the patient with excessive irritability. By degrees the pain becomes more continual, and at last almost incessant, and is much increased on pressure applied over the hypogastric region, and on putting the infant to the breast. When the first violent eruption has ceased, the uterine discharge is generally moderate, with the expulsion of occasional coagula. In the course of two or three days it assumes a character far from natural; it becomes sanious, of a dark and dirty brown colour, putrescent, and necessarily highly offensive to the smell; and, together with the exuding fluid, small shreds of putrid placenta will sometimes pass. Shortly the system in general sympathizes with the unhealthy state of the uterine organ; febrile symptoms, violent in degree, supervene, ushered in by one or more rigors; the pulse soon becomes rapid, and is generally small; there is heat and dryness of skin, especially on the abdomen; immoderate thirst; great anxiety both of countenance and mind; restlessness, almost amounting to jactitation; frequent sighings; occasionally vomiting, or a distressing sensation of choking, especially on attempting to take fluids, and perhaps also on pressure being applied to the uterine region; uninterrupted wakefulness; a diminution in the secretion of milk; the tongue white, loaded, and slimy, or red, dry, and shining; and there is more or less headache, with wandering of the mind. Sometimes the pain in the head is of a pulsatory character, and constant; at others, of a sharp, darting kind, and intermitting. Another very common, indeed almost universal symptom, is erratic pains in different parts of the body, most usually darting from hip to hip, or situated in the region of the diaphragm, much impeding respiration, and varying in intensity as in situation. The bowels at first are usually more torpid than ordinary, but after a time they become much relaxed, and there is difficulty in checking their action. As the case proceeds, the dangerous symptoms progressively increase; the strength hourly diminishes; the belly swells, and becomes tense; low delirium supervenes; the tongue acquires the typhoid character; vomiting of dark coffee-ground-like matter occurs; the extremities become cold; the fœcal evacuations and urine are voided involuntarily; subsultus tendinum comes on; and the patient sinks within a week or ten days after delivery.

It is not always, however, that the disease runs this fatal course. Sometimes the putrid mass is thrown off from the uterus, and relief almost instantaneously follows: at others, the symptoms never assume such violence of form; and on the third or fourth day from delivery, a purulent discharge, devoid of any unpleasant smell, flows from the vagina, in which filaments of the placenta are discernible, and which often lasts for two or three weeks. I suspect this discharge to be a secretion from the inner surface of the uterus, consequent on inflammation, and to be a means adopted by nature to get rid of the offending body, and always hail its appearance as one of the best signs we can observe. I seldom saw it afforded in such quantity as materially to depress the vital powers, and still less frequently have I known death ensue, where it had been freely formed. At other times, again, but very rarely, the remaining portions of placenta become the nucleus for hydatidinous formations; and more rarely still, I believe, they are actually absorbed.

Treatment.—When the hæmorrhage has ceased, which must be met by such means as have more than once been insisted on, our treatment must entirely depend on the violence of the symptoms present. The bowels must be moderately opened in the first instance, and their action restrained afterwards, if inordinate; the irritability of stomach may perhaps be allayed by effervescent draughts; and sedative medicines, either opium itself, or those of a milder kind, will generally be found useful. There exists a little difference of opinion among practical men as to the propriety of removing the after-pains by opiate remedies. My father and Blundell think it better not to interfere with the uterine contractions, because through their agency it is probable the irritating mass may be expelled. On the contrary, others, as Ingleby, regard opium as called for. For myself, considering the distress these

pains produce, I think it better to alleviate them, more especially as they frequently fail in producing the good anticipated, and, by their severity, occasion excessive irritability. Bleeding from the arm will seldom be borne with impunity; but more relief will be obtained by the application of leeches to the uterine region. I have occasionally found benefit from counter-irritants applied to the side of the chest or that part where the sympathetic pain was most acute; but not so frequently as I could have desired. As these pains, indeed, are usually erratic, and dependent (as I presume) on the state of uterine membrane, it is reasonable to suppose that more advantage will be derived from local applications to the uterus or its neighbourhood, than to the immediate seat of painful sensation. Under this idea I have generally directed my attention to the uterus, and my means to overcoming the disease existing in its structure: next to leeches, then, I have thought relief has been obtained from external fomentations, and particularly injections of warm water into the vagina or cavity of the uterus itself. If the os uteri be not morbidly tender, the nozzle of the syringe may be passed just within the orifice; but if this attempt should give much pain, I should content myself with merely washing out the vagina every two or three hours. The application soothes the parts, by acting as an internal fomentation, cleanses them of any putrid fluid which may be lodging within them, and may perhaps even tend to separate the adherent portions of placenta, and bring them away. A weak solution of the chloride of lime or soda may be substituted for the plain water, sometimes with advantage. When the symptoms of excitement have merged into those of depressed powers, wine, æther, ammonia, bark, and aromatics, may be used; but their efficacy in restoring the system to a healthy state is at the best inconsiderable.

ACCIDENTS LIKELY TO HAPPEN ON ATTEMPTS TO REMOVE THE PLACENTA FROM THE UTERUS, BY PULLING AT THE FUNIS.

The cautions I have so often repeated regarding the necessity of waiting until the placenta is wholly lodged in the vagina, before any attempt is made to withdraw it by the funis, are not without their object: for if the mass be adherent, we shall either separate it further from its attachment; or we shall break it, leaving a part in utero; or we shall rupture the funis, or cause it and the membranes to slip away from the bed of the placenta; or, lastly, we shall invert the uterus itself. Of these accidents, breaking the funis is the least, inverting the uterus the most dangerous, in its consequences. If we break the funis, we certainly lose it as a guide to the placenta; but this is not of much importance. We can pass the hand into the uterus, and remove the placenta, when the funis is broken off, almost as well as when it is entire, and its value as a guide is on the whole but trifling. If, by our improper interference, we separate a larger portion of the placenta, we shall bring on an increase of hæmorrhage, which will probably require the manual removal of the mass. If we extract only a part of the placenta, leaving a large portion behind, we shall also require to introduce the hand for the purpose of extracting the remainder. But it is possible that we may even invert the uterus: for if the placenta be adherent to the fundus of the organ,—if the adhesion be strong,—if the funis does not give way to the force applied to it,—and if the uterus be flaccid, and have not contracted round the mass,—the fundus will descend, pass through the os uteri into the vagina, and the viscus will be turned inside outwards, as a pocket might be.

Whenever this serious accident has happened (which, as it must always be the consequence of improper treatment, I trust will never occur in your hands), it may be known by the placenta still remaining adherent, though perhaps external to the vulva; by the sudden appearance of the sensitive tumor which occupies the vagina; and by the womb not being discernible above the pubes by the hand applied over the abdominal parietes. It must immediately be restored to its former state; for the lapse of every minute will be of consequence, since the longer we delay the more difficulty shall we experience in its reduction. For this purpose, the hand being half-shut, the knuckles are to be pressed firmly against the most depending point of the tumor; when the part will yield, the fundus will pass up with a sort of jerk, the organ will be restored to its natural situation, and the hand will occupy the cavity. The placenta may now be removed, as before recommended; and on the withdrawal of the hand the greatest possible care must be taken that the inversion,—of which there is some probability,—does not occur again. Unless this restoration be effected within a few minutes of the time when the accident took place, I should presume either that the uterus could not be reduced at all, or that the hæmorrhage must be excessive.

Barlow, following Puzos and Dionis, recommends that the placenta should be separated from its attachment before any attempt is made to replace the uterus. I should myself much prefer acting as

just advised, because, should the detachment be effected while the uterus remains in its inverted state, either the woman would bleed to death from the patulous orifices of the exposed vessels; or, if such a degree of contraction took place as to stop the hæmorrhage, that very shrinking of the uterine parietes would preclude the possibility of restoring it to its natural state.

Two cases of inverted uterus have come under my observation. In one I was requested to be present at the inspection of the body of a woman who had died from flooding soon after delivery; we found the uterus completely inverted, and lying in the vagina. The attendant had separated the placenta after the accident, and had contented himself with hiding the organ from sight within the external parts. The other was some weeks after delivery. The uterus had contracted to its small unimpregnated size,—almost as well, indeed, as though it was *in situ*; but the patient was draining to death with a copious fœtid discharge. The case presented an appearance, indeed, almost similar to this specimen [presenting a preparation.] Of the treatment of this chronic stage of inversion we shall speak when the organic diseases of the uterus become the subjects of discussion.

Errors liable to be committed.—The errors, then, that you are liable to commit in a case of adherent placenta are many. You may pull too violently at the cord, and cause rupture of the placenta, rupture of the cord, or inversion of the uterus. You may be too fond of removing the placenta by the introduction of the hand soon after the birth of the child. I have heard some practitioners boast how soon they can *fetch* the placenta after the birth of the child. Fetch the placenta, indeed! This fetching of the placenta has destroyed more women than all the accidents in labour combined. You may also,—but of this there is less danger,—delay removing the placenta until too late, when the patient having fainted frequently, lies gasping and tossing about, and is cold in the limbs, and a cold sweat breaks out on the neck. You may use too much force in introdueing the hand, and bruise the vagina or uterus, and too little care in separating the placenta, leaving a greater or less portion in the uterus. You may suppose, because you can feel part of the placenta, that therefore it must be entirely in the vagina; and you may, forgetting the precepts inculcated, endeavour to remove it by pulling at the mass; you may therefore break it, withdraw half, and allow the remainder to lodge in the uterus, and by its putrefaction to become the occasion of that train of distressing and highly-dangerous symptoms which I have this morning enumerated; or, lastly, you may not pay sufficient attention to the necessity of procuring a thoroughly contracted uterus; you may take away the placenta, and leave that organ in a flaccid state, the cause of a continuance of hæmorrhage.

The most horrifying case of mismanagement, perhaps, that ever occurred either in medicine or surgery, arose from an adhesion of the placenta, and is put on record by Dr. Boys, formerly physician-accoucheur to the Westminster General Dispensary. He was present at the dissection of the body, together with Mr. Brookes, Drs. Hooper, Fothergill, and several other gentlemen. They found wanting—the uterus, right ovarium and tube, part of the vagina, and part of the left fallopian tube; the greatest part of the rectum, cæcum, appendix vermiformis, the ascending portion of the colon, the right side of the transverse arch, all the ilium and inferior part of the jejunum, — altogether many feet of the small intestines,—with part of the mesentery, and the greater part of the omentum majus, which had been torn away from the right side of the large curvature of the stomach. The remaining portion of the transverse arch of the colon, and much of the jejunum, were torn from their attachments. The labour occurred on September 18th, 1807, and was complicated with an adherent placenta. The attendant broke the placenta by pulling at the funis. This produced hæmorrhage, and he left the patient. In about fifty hours, no attempt having been made to relieve her, the nurse found something hanging out of the external parts; and on his being apprized of it, he said it must be taken away, and placed her on her left side for that purpose. He made use of considerable exertion, and caused great pain. He then ordered a pair of scissors to be brought, saying there was a false conception, which must be removed: while using them the patient fainted, and died immediately. The parts taken away were preserved, and proved to be those I have just mentioned. We could scarcely suppose that such ignorance and barbarity could exist, as exemplified in the conduct of this case. But, independently of the precise account drawn up by Dr. Boys, we have my father's testimony in corroboration; for he saw the parts in Mr. Brookes's dissecting room. The man was tried at the Old Bailey for murder, and was acquitted.

HÆMORRHAGE SUBSEQUENTLY TO THE REMOVAL OF THE PLACENTA.

Even after the placenta has been expelled naturally,—or more frequently after it has been extracted by the hand, in con-

sequence of atony of the uterine fibres,—(notwithstanding that the labour is said to be terminated so far as the different stages are concerned) — the woman is liable to a continuance of the hæmorrhage, or to a fresh accession, owing to the want of due contraction in the uterine parietes.

Causes. — We shall generally find that this description of case occurs to women of relaxed habit, and weak muscular fibre—to those who have borne a great many children, or after a lingering or instrumental labour; or in cases where the child's body has been hurriedly extracted after the head is born. Exactly the same causes, indeed, will produce this state, as would occasion that inertia of which the retention of the placenta itself is the consequence.

Frequently, under these circumstances, the blood escapes externally; at other times it is retained in the uterus. A coagulum forms at the os uteri, and the effect of this plug is obvious; the blood is poured out from the open vessels into the uterine cavity, is prevented flowing forth, but continues accumulating within, sometimes to an amazing extent. The danger is even greater than when the hæmorrhage is external, both because the case may be overlooked, and because the more the uterus is distended,—the more blood the cavity contains,—the larger do the vessels become, dilating in their calibre in proportion as the womb increases. The organ has been known to acquire a size, after the birth of the child and extraction of the placenta, almost as large as it was previously to the commencement of labour, so that its fundus rises above the umbilicus, and its cavity contains many pints of blood. Notwithstanding this internal accumulation, scarcely a stain, perhaps, appears externally.

Sometimes the uterus contracts tolerably well immediately after delivery, and then again relaxes, contractions alternating with relaxation, until a very considerable quantity of blood having been lost, the patient sinks. It is on this account that I have more than once recommended you to examine the uterine tumor by your hand applied over the abdomen, three or four times within the first hour after delivery; and not to leave your patient until you are satisfied of the perfect and continued contraction of the organ.

Still another sort of case occurs, which we should scarcely expect,—a dangerous degree of hæmorrhage notwithstanding the uterus is acting powerfully, as evinced by violent after-pains. We lay it down as a principle,—true enough in general,—that the more strongly the womb contracts after delivery, the less danger is there of bleeding; but this proposition only applies to cases where the cavity is empty. If it contain a portion of placenta, or any other substance, its fibres may act preternaturally strongly, to expel the offending body; and yet, as the cavity is not perfectly closed, hæmorrhage may go on from the uncontracted vessels. Now occasionally the coagula which collect within the womb acquire such a degree of tenacity, that they adhere to the internal membrane almost as firmly as the placenta itself under morbid agglutination; and there is nearly the same improbability of their natural expulsion. Under such circumstances, the manual removal of the fibrinous mass, provided it can be accomplished without injury, offers the best chance of safety.

It has happened to me within the last year to meet with four or five cases of this kind, in which, although the uterus was small and tolerably firm, a draining of blood to an alarming extent was going on, while the patient was harassed with almost insufferable after-pains. On the introduction of the hand more or less entirely within the uterus, and the removal of the clots, not only has the discharge ceased, but the painful contractions have also in a great measure disappeared.

Of all these three states, that in which the uterus enlarges rapidly, fills with blood, and shews no disposition to contract, is by far the most dangerous.

Symptoms.—We know that hæmorrhage is going on, by the common symptoms which indicate the loss of blood. The colour vanishes from the cheeks and lips; the pulse flags; fainting occurs; the breathing becomes laborious, and drawn with sighs; the extremities lose their warmth; jactitation ensues, and perhaps vomiting. Vomiting, indeed, is not an universal symptom of loss of blood, and seldom comes on until the system is much depressed. Under great exhaustion, I consider it a good sign, rather than a bad one; because it shews that the nervous system is not deadened, but that impressions are still kept up between parts remote from each other, by means of sympathy: and I think, also, that the very effort of vomiting tends sometimes to induce contraction in the uterus, and may thus be the means of preservation. We also know that flooding is going on, if it be external, by an examination of the linen: sometimes we find a quantity of coagula expelled upon the napkins; at others, that part of the bed in which the woman lies is soaked with blood, and no misapprehension can arise as to the cause of the diminished vital energy. But the hæmorrhage may be internal and concealed;—still our means of diagnosis is easy and certain: the simple application of the hand over the uterine tumor will be sufficient to assure us of its state; and by

the sensation it conveys, we judge whether blood is pent up within its cavity. If we find the organ large, soft, and flaccid, and if it yield to the hand, and become harder when pressure is made upon it, and if then blood passes out of the vagina with a gurgling noise, we can be at no loss to declare the case one of concealed hæmorrhage. But, on the contrary, if the patient continue fainting, while there is no external flow—if we find the uterus as small as a fœtal head, and hard, and observe no relaxation in its structure—we must seek some other cause for the depressing symptoms besides loss of blood: the syncope is independent of hæmorrhage from the womb.

Treatment.—Our indication under hæmorrhages after the expulsion of the placenta, is to evacuate the uterus so as to ensure the closure of its cavity; and, if necessary, to rouse the flagging powers by the judicious use of stimuli. Both outward applications, internal remedies, and manual operations, will assist us in the accomplishment of our purpose. Pressure, and the application of cold, will often of themselves prove sufficient to restrain the flow, and they may be used in combination.

Called, then, to a case of this description, the first means I employ is the grasping pressure of the hand to the uterine tumor itself. It is not enough merely to lay the open palm upon the abdomen, and press steadily and flatly; but a squeezing or *kneading* action should be used, by which the organ is prevented filling and becoming distended with blood, and its fibres also are stimulated, to contract. It is not unlikely that the patient may complain of the pain you are putting her to; she may be desirous for you to remove your hand. If the pain she experiences, however, be that of contraction, her entreaties must be disregarded; because upon contraction alone her ultimate safety will depend. At other times she will not allow you to leave her a single minute; she feels so much comfort from the pressure of the hand and from the support which the abdominal parietes receive, and she experiences such a sensation of sinking when that pressure is removed, that she feels convinced she will faint if it be omitted. While hæmorrhage is going on with any activity, I place no reliance on a bandage, however tight it may be drawn, or with whatever local compresses its action may be aided. I cannot think any folds of linen applied over the uterine region, can secure contraction in a manner at all to be compared to the grasping pressure of the hand.

We have proof, indeed, that even the pressure of the hand will not always produce the desired effect: but other means are in our power, efficacious and of easy application; and of these, cold may next be resorted to. A napkin, soaked in vinegar and water, may be suddenly laid upon the hypogastric region, and the uterus will often answer the stimulus immediately. A succession of cold cloths may be used in this way, so as to keep the temperature of this part of the person below the standard, and pressure may be used occasionally at the same time. Should the bleeding, however, still continue, and the faintness increase rather than diminish, the means I next adopt (and sometimes this is much more useful than any other mode of applying cold) is dashing a quantity of cold water upon the lower part of the abdomen. This may, perhaps, appear a rough, and neither a very refined nor very delicate mode of treatment; but the case is of a highly dangerous character, and all other considerations must give way to ensuring the patient's safety. It is an universal observation, that a slight degree of cold, applied suddenly and with a shock, will produce a greater effect than a more intense one continued for some time. Thus Gooch gives us an instance in which the uterus was stimulated to contract by a quantity of water, the temperature of the chamber, thrown suddenly from an ewer on the abdomen, although it had not answered to the application of ice, which had been used previously for a considerable period.

We may, however, still be foiled, and must resort to other means. The introduction of the hand into the uterine cavity may next be put in practice, and it seldom fails in producing the contraction we desire. You may inquire, then, why we should not introduce the hand immediately the hæmorrhage becomes alarming? Because it is better first to adopt less harsh means. The introduction of the hand is always to be avoided, if, by any other method, we can produce the same measure of good, without the chance of injury: but yet there are many states that fully warrant even this proceeding. The coat, then, must be taken off, the left hand and arm greased, and passed gently into the uterus, and the parietes may be stimulated by the fingers moved within it; at the same time that the right hand grasps it externally; or, as Gooch recommends, the bleeding vessels may be compressed with the knuckles within, while the uterine tumor is pressed upon without; and by this combination of external and internal pressure, it is seldom that we shall not succeed in putting a stop to the discharge. If there be any fibrinous coagula adhering to the internal membrane, these must be removed as cautiously as we should separate the placenta.

Some cases, however, will not even yield

to this mode of treatment, and other expedients are recommended—such as injecting a quantity of cold water, vinegar, or other astringents, into the uterus itself. This is a plan I never had recourse to; but Gooch used to declare, he was not happy in attending a case of labour unless he had his syringe in his pocket. Ice has also sometimes been introduced into the uterus with advantage, either naked, or wrapped in linen or flannel. A small quantity of ice may be procured from a neighbouring confectioner's, held in the hand till the corners are rounded off, and then passed into the cavity; the fear of freezing the living structures being always uppermost in the mind. Again: it has been recommended that we should use the tampon—that we should stuff the vagina, or even the uterus, with cloths steeped in any astringents at hand. To this application, under such circumstances, you may recollect I have already decidedly objected; because the blood is not preserved in the woman's vessels by filling either the vagina or the uterus; it is escaping through their orifices, and collecting in the uterine cavity; and as the womb, by the presence of the plug, is prevented contracting, the very object which we wish to gain is defeated by our own anxious care.

Of remedies acting through the agency of the stomach, stimuli (domestic or medicated), opium, the acids, and the ergot, are the chief in use. The cautions respecting the use of spirits, ammonia, and other substances which powerfully excite the arterial system, I need not here repeat. They are only admissible—so long as there is a chance of active hæmorrhage continuing—when the powers are sunk so low that there is immediate and imminent danger present. I have stated that opium, in large doses, is very much extolled in cases of flooding, especially by Professors Burns, Hamilton, and Dr. Stewart, but that I consider it a medicine perfectly inadmissible unless the uterus have entirely contracted, when the danger of fresh bleeding is gone by. Opium certainly acts as a cordial, lulling the irritability of the patient, and producing a sleep, or at any rate a composing stupor; but it also takes off both muscular and uterine action: it disables the uterus, therefore, from contracting, even were it so disposed: and if the proposition be true, that on the contraction of the uterus alone we are to rely for the patient's ultimate safety, how contradictory does it not appear to resort to a medicine whose very action tends to prevent the effect desired! It has been objected, that although opium, in small quantities, takes away uterine action, yet, in large doses, it produces the very opposite result, and excites contraction. This proposition is at variance with common sense, with all analogy of the actions of other drugs, and with my experience. I have often seen, at the commencement of labour, uterine action suspended by what would be considered a large dose of opium; and if the same quantity will take away action at the beginning of labour, is it reasonable to suppose it will excite it at the termination? But it may be said the experience of practitioners of eminence proves the value of opium in the case under consideration; and that there is no reasoning against experience. I by no means deny that many patients have done well after the exhibition of large doses of opium, but that circumstance by no means proves that the drug was the agent of their preservation. I should be inclined to attribute the recovery to other causes, independently of the exhibition of the medicine. I strongly recommend opium in large doses and the solid form, in those cases of irritability produced by a loss of blood which had previously taken place; but that only when the uterus is contracted and the danger from flooding is past.

We should act unwisely to trust much to any of the mineral acids in these dangerous cases, but they may be exhibited in combination with the use of other means, and generally act as grateful refrigerants. The ergot seems to be indicated, as the grand object is to produce uterine contraction: it may be combined with an acid. I am persuaded I have witnessed cases of after-hæmorrhage, in which it has been exhibited with decided advantage.

Transfusion.—Our last resource is the transfusion of blood into the system of the dying patient,—a means most powerful in arresting the vital spirit, even as it flutters with tremulous delay upon the lip. To Dr. Blundell is due the merit of having restored the practice, of advocating its adoption with all the force of his powerful mind, and proving its efficacy both by reasoning and experiment. But transfusion can be of little use, unless contraction have taken place in the uterine parietes. It is evident that, while the vessels remain patulous, the more we excite the arterial system, the more likely is the flooding to continue, as is demonstrated in the use of the ordinary stimulants. If, then, by infusing blood while the uterine structure remains flaccid, we cause the heart to beat more forcibly, the fluid will again exude through the open vessels; and we might inject *ad infinitum*, the arteries emptying themselves as the veins become distended. But the case is very different when we have closed the vessels through which the blood escapes; it is then retained in the body, forms a part of the circulating current, revives the patient by its action on the brain, and restores her from temporary death to life. I would restrict the prac-

tice of transfusion, then, to those cases in which there is no chance of the blood again escaping; and I think it would be most useful when the placenta has been removed—when the uterus has contracted—when the hæmorrhage that had caused the depression has ceased—but when the patient remains fainting and in danger, unable to be roused by stimuli taken into the stomach, and without a well-grounded hope of restoration being effected by her own proper powers.

In such cases I have little doubt that transfusion may be most valuable; but if it becomes a common practice, I am persuaded it will often be employed unnecessarily. This opinion I form from having seen many women recover without the operation of transfusion, who seemed scarcely to have a chance of life. If in these cases the means under consideration had been used, the credit of the recovery would have been given to the operation, and strong arguments might have been adduced in favour of the propriety of the measure.

It is astonishing how tenacious of life some systems appear under uterine hæmorrhage, and how easily others will let it glide away. The necessity, then, of such means in any particular case, will be a question of the greatest nicety. If employed while active hæmorrhage is going on, it will be useless; if delayed until the breathing has quite ceased, it cannot be expected to be followed by restoration; if commonly practised, it will many times be resorted to unnecessarily.

Mode of performing transfusion.—Blundell, with much ingenuity, has contrived an instrument, named by him *the impellor*, by which the blood may be at once transfused from one system to the other, without being obliged to stagnate in a vessel; but as this is rather a cumbrous machine, and difficult to adjust, and especially as the same enlightened physiologist has proved beyond dispute that the vital fluid loses little or none of its valuable properties by being collected in a cup, absorbed by a syringe, and afterwards injected,—provided no time be unnecessarily wasted,—I shall merely describe the mode he adopts of performing the operation by a common syringe. The instrument should be of brass, tinned inside, capable of containing between two and three ounces, perfectly air-tight, and clean from oil. Two or three persons,—males in preference to females, from their less liability to faint,—being in readiness to supply the blood, one of the patient's veins at the bend of the elbow must be laid bare to the extent of an inch, and divested of its cellular web; an aperture must then be made into it of about a line in length. The blood must be drawn from one of the by-standers in a full stream, about three ounces must be received into a conical cup or tumbler, and absorbed as soon as it is collected: the nozzle of the syringe must be raised perpendicularly, and the piston slowly propelled upwards, to expel any air that might have passed into the instrument: its point must afterwards be inserted in the aperture formed in the vein, and the blood slowly propelled towards the heart. Blundell recommends that no ligature should be put upon the patient's arm below the wound; but Waller thinks it better to secure the vein, by passing a blunt-pointed probe entirely under it, having dissected it away from its surrounding attachments; and this particularly, lest blood should escape from the incision, and embarass the operator. The chief delicacy of the operation consists in regulating the quantity used, and in adjusting the velocity with which it is injected to the diminished power of the arterial system. If we throw it in with too much force, we may choke the heart, and death will be the consequence: if, on the other hand, we are too tardy in our operations, the blood may partially coagulate, and be rendered unfit for the purposes of life. We may inject a second, third, and fourth syringe full, deliberately watching between each the effect produced. Twelve or fourteen ounces will most probably, at the highest average, be sufficient, if any advantage is to result from our endeavours: for, since we know that, although a patient will sometimes bear a large loss of blood with comparative impunity, the addition of a very few ounces more may irrecoverably depress her, so I think we may reasonably infer that a small quantity added to the system, after a great diminution has been sustained, will be sufficient to raise it to the necessary point; unless, indeed, the nervous energy be too much sunk to be roused by any means that human ingenuity could devise.

I have directed, that if *transfusion* should appear necessary, human blood should be used for the purpose; and this recommendation is founded on experiments first, in modern times, performed by Dr. Leacock, of Barbadoes, and made known through the medium of his inaugural thesis, printed in Edinburgh in 1817; and afterwards frequently repeated and varied by Dr. Blundell. These experiments prove that the blood of one genus of animals is unfit to carry on the functions of life, when injected into the system of an individual of another genus. If human blood, for instance, be injected into the veins of a dog, after the animal has been bled to syncope, resuscitation for a time occurs; but it is not last-

ing—the powers soon begin to droop, and after a period, varying from a few minutes to some hours of languid existence, death takes place. Thus, then, we must never think of employing, for the purpose in question, any other blood than that derived from the human subject.

Management of a patient after dangerous hæmorrhage.—When a woman has suffered hæmorrhage to any dangerous extent, although the uterus may have become firmly contracted, and, to her own feelings, she is comparatively comfortable; yet she must by no means be considered safe for many hours; because the organ may again relax, and with a return of the relaxation there may be a return of the bleeding. It behoves us, then, to guard against such a possibility; and the best mode of prevention is to keep her perfectly quiet, to allay irritability, and solicit sleep by a moderately large dose of opium, and to sustain her by a frequent supply of fluid nourishment in small quantities. Strict injunctions, then, must be given that she should not be moved for many hours. In ordinary cases, after labour we require that an hour and a half, or two hours, should elapse, before the patient is placed in bed in the position she is to retain, and her linen changed; but after hæmorrhage, we shall frequently find it necessary to keep her in the same position for eight, ten, or twelve hours, or even longer. A bandage may perhaps be applied, but beyond that no alteration should be made in her person. Many cases are on record, where a patient, having suffered hæmorrhage, has been placed in a sitting posture, for the purpose of having her person made more comfortable, and has fallen down dead upon the bed; others, where moving from one side of the bed to the other has produced syncope, and even death; and some, where the same fatal consequence has followed the slight exertion of raising the head above the level of the shoulders: so that we cannot be too cautious in allowing the least disturbance. The room may be darkened and well ventilated; opium may be given, with a little stimuli, if necessary; and such nutrient fluids as are most easy of digestion should be exhibited at stated intervals.

EXTRACTS
FROM A
JOURNAL OF NATURAL HISTORY;

Kept during a Voyage from England to New South Wales, and on the return to England, by way of Batavia, Pedir Coast, Singapore, &c.

By George Bennett, Esq.

We sailed from Plymouth for New South Wales, on the 4th of May, 1832; and on the 9th of May, in latitude 47° 3′ north, and longitude 9° 41′ west, a poor linnet, which seemed to have been blown off the shore, came on board, and was easily captured by the hand, appearing quite exhausted: it was placed in a cage, where it remained tranquil, but would neither eat or drink: on the following morning it was found dead.

On the same day a fine large male bird, of the *Hæmatopus ostralegus*, or oyster catcher, alighted on the deck of the ship, was captured, and after being examined, was again set at liberty, and in the evening we saw nothing more of it. There are several species of this genus, differing in size and other characteristic differences.

The migratory character of some classes of birds and insects has often engaged the attention of naturalists. Many not of a migratory character have visited ships in great numbers when some distance from the land, having been blown off during strong gales, and unable to regain the shore.

Off the coast of Cochin China, in October 1830, we had doves, (which, when they alight on a ship, the superstition of the Venetian sailors regard as the token of a prosperous voyage), swallows, and other small birds, as well as three hawks of different sizes, which had perhaps been blown off the land with them, or had followed in hopes of making the little emigrants their prey. One of the hawks was killed, when perched on the royal-mast head, with a swallow in his claws, which he was in the act of devouring.

Off the coast of France numbers of larks annually perish from being blown off the land by storms, and they have often sought refuge on board ships which may have been near the coast at the time, in incredible numbers.

Beautiful butterflies and other insects have also alighted on board ships in great numbers from similar migrating and other causes. It was not long since that several ships, off the coast of Devonshire, were visited by multitudes of that rare and splendid moth in England, the *Acherontia atropos*, or *Death's head hawk moth*, which were captured in great numbers by the crew. The larva of this splendid moth is usually found in potatoe fields. Could these insects have congregated together, and have been emigrating, when captured on board the ships?

It is supposed that the flight of the swallow, when emigrating, is not less than fifty miles an hour; so that, when aided by the wind, they soon reach warmer latitudes. Spallanzani calculated that the swallow can fly at the rate of 92 miles an hour; and that of hawks and several other tribes to be 150 miles.

It was for a long time denied that swallows were birds of passage; and the opinion was, that during the winter they remained in a torpid state under water, or in some secret recesses: at last, M. Adanson, in his History of Senegal, p. 67, makes the following observation in proof of swallows being birds of passage:—"The 6th of October, at half-past six in the evening, being about fifty leagues from the coast, (between the islands of Goree and Senegal), four swallows came to take up their night's lodgings on the ship, and alighted on the shrouds. I easily caught all four, and knew them to be true European swallows. This lucky accident confirmed me in the opinion I had formed, that these birds pass the seas to get into the countries of the Torrid Zone at the approach of winter in Europe: indeed, I have since remarked that they do not appear at Senegal but at that season, with quails, wagtails, kites, and some other birds of passage, which come thither every year when driven by cold from the temperate countries of Europe. A fact no less worthy of remark is, that the swallows do not build in Senegal as in Europe; they rest every night, either in pairs or solitary, in the sand upon the sea shore, which they prefer to an inland situation*."

Off the Island of Luçonia, soon after leaving Manilla, on a former voyage, our ship was visited by a great number of swallows, as well as other birds, which accompanied us as far as the Cochin-Chinese coast, when but few remained. Many perished during the passage down the China sea, which was at this season of the year (September) against the monsoon. At night they took refuge on board at different parts of the ship; some took possession of the boats and even the cabins. Several were seen nestled together on some clothes in the chief officer's cabin, and were suffered to remain unmolested until the morning, when the cabin door was opened, and they took their departure, flying about the ship, again taking refuge at night on board. Flies, of which we had great numbers on leaving port, had almost all disappeared, which we attribute to the presence of our insectivorous swallows.

"Some insects are very local, while a few, on the contrary, are common to remote countries, between which the Torrid Zone and the ocean intervene. Thus our painted lady butterfly *(Vanessa cardui)* re-appears in New Holland and Japan with scarcely a varying streak*. The same species is said to be one of the few insects which are universally dispersed over the earth, being found in Europe, Asia, Africa, and America; and its wide range is the more interesting, because it seems explained by its migratory instinct, seconded, no doubt, by a capacity, enjoyed by few species, of enduring a great diversity of temperature.

A vast swarm of this species, forming a column of from ten to fifteen feet broad, was, a few years since, observed in the Canton de Vaud; they traversed the country with great rapidity from north to south, all flying onwards in regular order, close together, and not turning from their course on the approach of other objects. Professor Bonelli, of Turin, observed, in March of the same year, a similar swarm of the same species, also directing their flight from north to south, in Piedmont, in such immense numbers, that at night the flowers were literally covered with them. They had been traced from Coni, Raconi, Susa, &c. A similar flight at the end of the last century, is recorded by M. Louch, in the Memoirs of the Academy of Turin. The fact is the more worthy of notice, because the caterpillars of this butterfly are not gregarious, but solitary from the moment that they are hatched; and this instinct remains dormant, while generation after generation passes away, till it suddenly displays itself in full energy when their numbers happen to be in excess†." I recollect one year at Colombo, in the Island of Ceylon (but in what month I do not recollect, and have no memoranda to guide me), a quantity of butterflies, principally of one species, passed down the road in a northerly direction.

* Cited in the Corr. of Linnæus, by Sir J. Smith. Vol. i. pp. 48, 49.

* Kirby and Spence. Vol. iv. p. 487.

† Lyell's Principles of Geology. Vol. ii. pp. 113, 114.

The number was almost incredible; and whilst the main body kept a direct course, others deviated from it, and alighted upon the flowers which grew profusely on the road side: this continued for most part of the day for several successive days.

A ship, when outward bound to Sydney, New South Wales, from England, was visited, on the 8th of October, 1832, in latitude 22 deg. 5 min. north, and longitude 23 deg. 50 min. west, with numbers of a large species of Grillus, having the upper part of the body dark-green and scarlet, abdomen yellow, wings dark-green, and they were about four inches long; the same species was immediately recognized by the passengers and officers at the Cape of Good Hope, where the ship touched during the same voyage.

The wind was from N.E. by E. a fresh trade breeze. These insects continued about the ship for several successive days (one day several being captured, the next perhaps only a few straggling specimens), until the 14th of October, in latitude 11 deg. 6 min. north, and longitude 24 deg. 55 min. west, which was the last day on which any were seen.

On the 8th of October it was ascertained, by the chart, that the ship was distant 360 miles from the nearest part of the African coast, and a distance of nearly 300 miles from the Cape Verd islands. For three subsequent days they had a fresh trade wind from N. E. by E. steady.

On the 11th of October the ship was distant 100 miles from the nearest of the Cape Verd islands, and 480 miles from the nearest part of the coast of Africa; and on the 14th of October, the distance of the ship from the nearest part of the African coast was 480 miles, and to the southward of the Cape Verd islands 220 miles.

There was nothing further interesting in natural history until off Madeira, in a brief calm; when several of the molluscous animals were taken with the dipping-net from the chains.

On the 16th of May, about thirty miles to the southward of Madeira, a shark (Carcharias) was seen; and by aid of the dipping-net, some medusæ and a species of vellela (V. scaphidea of Leach) were captured. The vellela was colourless, probably a dead specimen, but it had two substances attached to each side of the crest by a short thin pedicle: the weight of these sacs kept the animal, when in the water, with the crest downwards, and the disc consequently uppermost, on the surface of the water. On placing these substances under a powerful microscope, they were seen to consist of distinct cells, or discs, each containing a moving body, in a state of constant gyration.

The sacs (or whatever else we may call them) appear to the naked eye to be globular, studded with spots, diaphanous, colourless, and globose. It is either a nidus attached to the velella, (which was dead,) or it may be a distinct animal thus existing in groups or colonies. Perhaps, instead of globose, the form may be compared to a flat substance rolled inwards. There could be clearly distinguished in the microscope, an appearance like moving branchiæ.

Several small specimens of the physalis pelagica of Lamarck were also taken at the same time.

In one of the large medusa captured, was a species of *oniscus*.

There seems to be an acrid property, more or less, existing in the medusæ tribe: the one captured this evening could be handled with impunity, not exciting any heating or pungent sensation on the skin; but when accidentally touching the interior of the nose, with unwashed hands, it seems that an acrid fluid existed, although not sufficiently pungent to irritate the skin of the hand, irritated the delicate Schneiderian membrane, for the touching it was followed by a heating sensation, and, for a short time, a discharge of mucus from the nose; the unpleasant irritation remaining, together with the coryza, for several hours afterwards.

We first had bonitos (scomber pelamis) on the 12th of May, about the ship, in latitude 39° 36′ north, and longitude 14° 5′ west.

It is inconceivable with what an enormous quantity of living animals the ocean teams, one devouring the other: were it otherwise, it is supposed that the sea would soon be a mass of corruption, and like the insects on land, would, if not kept in check by their numerous enemies, render both land and sea uninhabitable by the human race. The agitation of the waves keeps them more below the surface, as they are seen in the greatest quantities in those parts of the

sea where calms are the most frequent; or perhaps we may more correctly say, that the water not being agitated enables them to ascend to the surface. The appearance of these animals during a calm, was regarded by the old navigators as tending to produce *scurvy*. "The greatest part of the time we were becalmed," says Sir R. Hawkins, in his Voyage to Magellanica, "all the sea became so replenished with several sorts of gellies and forms of serpents, adders, and snakes, as seemed wonderful: some green, some black, some yellow, some white, some of divers colours; and many of them had life, and some there were a yard and a half long, and two yards long; which, had I not seen, I would hardly have believed."

As the ship passes through the blue waves of the ocean with a moderate breeze, there is seldom a living animal of the mollusca or crustacea tribe perceptible in the water; but the towing-net entraps and enables them to be brought on board, surprising the beholders at the various and curious forms of some, and the beauty of the colours in others.

In latitude 29 deg. 43 min. north, and longitude 17 deg. 26 min. west, on the 19th of May, I placed the towing-net overboard (Palma in sight, bearing S. S. W. about fifty miles distant), and captured, of the Pteropoda, an hyalœa, apparently the tridentata of Lamarck; which, on being placed in a glass of water, exercised its lively disposition by making rapid movements around the vessel, by aid of its two opposite retractile fins, which are inserted at the sides of the mouth.

Of the *Stomapoda* order, several of the diaphanous *alima hyalina* of Leach were captured.

Among the crustacea was a small specimen supposed to be of the order *Loemodipoda;* family, *Ovalia:* having two projectile fins on each side of the body, of a similar colour on the surface to the upper part of the animal, being of spots of a dark-red colour.

On the 20th May, in latitude 28 deg. 28 min. N.; longitude 18 deg. 11 min. W.; some molluscous animals would float by the ship at night, giving out, at intervals, a phosphoric light, which seemed to diffuse itself for some distance around them, would last for about ten seconds, and then suddenly disappear. The appearance was very beautiful.

On the 21st May, latitude 26 deg. 28 min. N.; longitude 19 deg. 4 min. W. several of the large species of physalis pelagica passed the ship, sailing proudly along; the raised crest glowing with crimson, and its bright purple tentaculæ spread in various dirictions, for the purpose of entrapping its prey; and an additional beauty is produced when the sun shines on the carnation tints which deck the crest of this singular animal. The greatest length their tentaculæ has been seen, has been five yards.

May 24th, latitude 19 deg. 44 min. N., longitude 21 deg. 51 min. W., several shoals of flying-fish rose from the water, occasioning an audible rushing noise, which seemed to be produced by the spreading of their pectoral fins as they rose from the ocean. They seemed at this time more in sportive gaiety than in terror from pursuit.

From *Quail Island* (situated in Porto Praya Bay, St. Jago, Cape Verd islands) I procured several specimens of patella and a murex (M. fucus of Linn., purpura neritoides, Lam.), with three spots on the under part of the shell, near the operculum, and the specimens measured $1\frac{1}{4}$ inch in length; and several of a species of aplysia, or sea-hares, as they have been popularly named. The latter animal is of the class Mollusca; order, Gasteropoda; and derives its name from it being supposed that the secretion which exudes from the body, when the animal is touched, occasions the hair to fall off, and excites vomiting and nausea. These animals, on being touched, discharge a quantity of purplish fluid, which renders the water turbid: it seems used for a similar purpose to the dark inky fluid of the sepia tribe. This genus of the gasteropodes have an "oblong body, convex, bordered on each side by a large mantle, which, in repose, covers the back; head, with a neck and four tentaculæ, of which the two are upper and auriform, and the others near the mouth; eyes sessile; scutellum semicircular, subcartilaginous, fixed on on one side, and covering the bronchial cavity; anus behind the branchiæ."—Stark's Elements of Nat. Hist. vol. ii. p. 62.

These animals were considered, by the ancients as well as moderns, to be of a poisonous nature. In Italy they were used in a poisonous beverage, and Domitian was accused of having given it to his brother. The symptoms of poi-

soning by this animal were described as a livid skin, swollen body, urine at first suppressed, and then comes out sometimes of a purple, and at others of a blue colour, and often bloody; and the patient at last perishes with violent vomitings and colic pains. The remedies proposed as antidotes were also innumerable, and, as is usual in these erroneous poisons, of contrary virtues. It was described by Apuleius, and one of the principal proofs brought forward, it has been said, against him, when he was accused of magic and poisoning, was, that he had engaged some fishermen to procure him a sea-hare, for which he paid them a high price.

The name of sea-hare seems to have arisen from the auriform appearance of the two upper tentaculæ, which (in some species more than in others) resembles the ears of a hare.

Cuvier states that there issues from this animal an acrid liquor, which has been regarded as venom; but, besides that liquor, which is whitish, and which is rarely emitted, the animal discharges around another, in greater abundance, which is of a very intense reddish-purple colour: this red liquor is not contained in a particular sac, but is situated in the same substance as the branchial covering, around its free border. The colouring liquor of this animal, it is further stated by Cuvier, when drying, becomes of a very deep purple tint, which has been compared to that of the flowers of the *Scabiosa atro-purpurea*, and is not changed by the atmosphere alone. Nitric acid gives it more of a violet tint; when more is added, it becomes of a dirty-yellow colour.

The species captured at Porto Praya was above of a greyish colour; the lower part of a pale dun colour. When found on the rocks, their copious mantles are seen floating out to some extent on each side. These specimens measured five and six inches in length, and two inches in breadth, at the broadest part.

[To be continued.]

TREATMENT OF CHOLERA—EMETICS OF COMMON SALT.

To the Editor of the Medical Gazette.

Sir,

During the last three months I have attended several cases of cholera, and the following plan of treatment has been so extremely successful, that I am induced to trouble you with this communication, thinking it may possibly benefit some of my professional brethren.

The symptoms of cholera, in its worst form, are unfortunately too well known to require description, and to this stage of the disease I wish it to be understood the following plan of treatment is especially applicable.

To an adult, I give three table-spoonfuls of common table salt dissolved in half a pint of tepid or cold water. This produces vomiting in less than one minute, and the vomiting ought to be powerfully violent: such effect is followed by an immediate diminution of the cramps, and a very marked increase in the fulness of the pulse; the other symptoms are much amended, the voice becoming stronger, and the muscular power greater. It may happen that the first dose answers every purpose; frequently, however, the beneficial effect of the first dose is not permanent, for within twenty minutes, or half an hour, the pulse begins to flag, and then rapidly decreases in power; another attack of cramp ensues, and the general prostration increases. A second dose of the salt is now to be administered, and even a third may be required. I have never had occasion to give it oftener than three times.

The good effect of the salt emetic really depends upon its producing *powerful* and very efficient vomiting: without this full operation, it ought to be *thrice* repeated, with an interval not exceeding five minutes between the doses.

The adjuvant treatment consists in placing the patient (who must not be oppressed with heavy covering), between warm blankets, diligent friction of the parts spasmodically affected with stimulating liniments, and occasionally applying a mustard cataplasm to the pit of the stomach.

As medicine, I administer a draught containing half a drachm of carbonate of soda dissolved in two or three ounces of water, to which I add four or five drachms of fresh lemon-juice, and give it while effervescing, every hour, for three or four doses; afterwards once in four hours. For forty-eight hours I give no other medicine. I have tried in two instances scruple doses of the muriate of soda, with a like quantity of

the carbonate; but the patients were nauseated by the flavour, and no extra benefit was the result.

Generally on the day succeeding the attack the patient passes a small fæcal evacuation, containing bile; otherwise I give, at the expiration of forty-eight hours, a drachm of castor oil, or a few grains of rhubarb.

The active part of the above treatment requires the constant superintendence and watching of the medical practitioner from two to four hours, during which time I allow the patient to drink as much water, or toast and water, cold or tepid, as may be wished for; but no other beverage. For the next twenty-four hours I advise only weak black tea, with thin arrow-root; and on the third day a little broth, or light pudding; my patient now being generally convalescent. Indeed, it is remarkable how rapidly recovery ensues, considering the late direful state of the individual.

The plan may require some modification, when young or aged persons are under treatment.

In conclusion, I beg leave to remark, that I claim no merit for originality of system; but as I communicate some important practical facts, relative to the successful treatment of no less than eleven consecutive cases of this disease in its worst form, I hope my professional brethren will consider that I am actuated by no other motive than a wish to lay before them to such extent the result of my experience.

I am, sir,
Your obedient servant,
GEORGE BEAMAN.

32, King-Street, Covent Garden,
August 19, 1834.

ARTERIALIZATION OF FŒTAL BLOOD.

To the Editor of the Medical Gazette.

SIR,

ON looking over the Medical Gazette of July 12th, I find an answer to my inquiry on the absorption of oxygen by the lungs, from Dr. Allnatt. That the fœtus may derive a portion of its nutriment from the mother, by absorption, is certainly very reasonable; indeed, unless we were satisfied that such was the case, we should have extreme difficulty in accounting for phenomena connected with the uterus which are daily making their appearance, commonly termed false conceptions, which not unfrequently attain a considerable size, and yet exhibit, on dissection, no heart, brain, or any of the important parts necessary to an independent existence. Yet I cannot give up my opinion that the fœtal blood undergoes a change in the placental vessels, in consequence of its near approach to the arterial blood of the mother. No argument brought forward by Dr. Allnatt can induce me to think otherwise. The great size of the uterine vessels, or sinuses, inclines us to believe that the blood circulates through them at a slower rate than through other parts of the system; and I am at a loss to conceive what peculiarity there is in the constitution of a vein to render it repugnant to the retention of arterial blood. It may be said, with as much propriety, that it is also repugnant to its transmission; of which fact we have direct evidence in the pulmonary veins, which convey the blood immediately after it has undergone the change in the lungs, and at the moment of its greatest purity, into the left portion of the heart; therefore the retention of blood in the veins will not depend on the constitution of the vein, but on the locality, calibre, and the degree of obstruction the fluid may meet with *in transitu*. Stagnation of blood is not at all necessary to enable the maternal circulation to attract the impurities of the fœtal blood, or the fœtal to imbibe, *if required*, the oxygen of the maternal. The change, although it would be more fully effected during a slow transmission of blood through the vessels, might take place at the moment of its passing from the arteries to the veins; and why should we hesitate to believe, although it may be difficult to obtain direct proof of the fact, that *carbon may be attracted by the oxygen of the blood in the placenta, even through the medium of a membrane*, knowing, as we do, that it does take place under similar circumstances in the lungs? The affinity of the one gas for the other is well known; and surely it is contrary to all analogy, to hold that oxygen can exert its influence over carbon, and unite with it to form carbonic acid (being chemically combined with other gases, as in the atmosphere), and that it should lose that

power when mechanically mixed with the arterial blood.

What the quantity of oxygen required to produce the change may be, is uncertain; probably it is small, and therefore is this theory the more tenable; but that a certain portion is necessary, I feel a clear conviction. Many of the oviparous animals—the crocodile and turtle, for example—deposit their eggs in the sand, cover them over lightly, for the purpose of concealment merely, and leave them exposed to the heat of the sun's rays, *and as much as possible to the influence of the air.* The instinct of several of the finny tribe directs them at certain seasons to leave the ocean, and ascend small brooks and rivulets to deposit their spawn on sand-banks but lightly covered with water. It cannot be altogether for the protection of the eggs, (for fresh-water fish destroy and devour them with perhaps greater avidity than those of the sea), but because the water in small streams, from its constant agitation, is more impregnated with atmospheric air, which exerts over the spawn a peculiar vivifying influence. Why has nature, ever provident, ordained that the shell of birds' eggs should be of a porous nature? Is it not that the air may exert its power over the chick within, without which the heat of the parent's breast would be of no avail?—for if the shell be covered with a coat of varnish, or any other substance which will fill up its pores, the process of incubation may go on week after week, but the yolk would thereby undergo no change. Seeing, then, that for the production of the young of oviparous animals, independently of the nutriment derived from the yolk on which they are formed, a communication with the atmosphere is indispensible, we may consistently imagine that its agency is also required by the fœtus in utero: the vessels of the latter cannot benefit so directly as those of the class of animals above mentioned; but there will be no difficulty in explaining how it does take place, while we believe that oxygen is absorbed by the arterial blood.

I have the honour to be, sir,
Your obedient servant,
GEO. PHILLIPS,
Surgeon.

Haverfordwest, Pembrokeshire,
August 17, 1834.

DISLOCATION OF THE CRYSTALLINE LENS.

To the Editor of the Medical Gazette.

SIR,

IF you think the following case sufficiently remarkable, you are at liberty to insert it in your Gazette.

I am, sir,
Your obedient servant,
JOHN OKES.

Cambridge, Aug. 18, 1834.

A few weeks since, a man, about 35 years old, was received into our County Hospital, giving the following account of himself. Several years ago he suffered severely from inflammation of the eye; from the effects of which he never entirely recovered. Two years from the present time the eye became slightly painful, and about a twelvemonth afterwards he observed a white appearance at the pupil, which gradually became more distinct. Upon one occasion of his suffering considerable pain, he examined it at the looking-glass; when the white substance appeared to him to be flat and round, and much nearer the surface of the eye than it used to be.

It was then, no doubt, lying in the situation in which it was noticed at the time of his admission into the hospital, in the anterior chamber, pressing upon the iris. It continued more or less painful to him for some time, until upon some accidental occasion of jerking his head backwards, he suddenly became free from pain. Looking into the glass, he found the substance much smaller, and, as he described it, "gone backwards into the eye," much in the same manner as it used to do two or three years before. Some time afterwards the pain returned, and the substance had become as large as before. Remembering that he had obtained relief by jerking the head backwards, he tried it again, and was pleased enough to find it successful. For several months he adopted this mode of relief whenever the diseased lens had been protruded into the anterior chamber.

At length, however, the usual means of relief failed him; the lens remained fixed; and he was driven by continued pain to seek for assistance at our hospital. With the hope that he might shew us the manner in which he used to relieve himself, I desired the belladonna

might be freely applied; but it had no effect; the long-continued irritation, arising from the pressure of the lens against the iris, had rendered its fibres immoveable.

As it appeared that nothing but the removal of the lens would afford him relief, I extracted it in the usual manner through an incision in the cornea. After the extraction an opaque capsule was found pressing against the pupil: this was not opened: it was not thought advisable to prolong the operation in the present irritable state of the iris. He was relieved entirely by the extraction of the lens: the incision healed up quickly, and he left the hospital in about a week afterwards. The eye continued quite insensible to light; and the opacity, which was so apparent when the capsule touched the iris, was now scarcely to be seen. This circumstance, in addition to that of the breach which must have taken place in the anterior capsule, leads to the supposition that the opaque membrane was posterior to the lens, probably involving the hyaloid membrane. An opening being made in it at the time of the operation would not perhaps have contributed much to the patient's advantage.

ON THE COMPARATIVE MERITS

OF

DR. BELLINGERI'S AND SIR C. BELL'S WRITINGS

AND OPINIONS ON THE FUNCTIONS OF THE FIFTH AND SEVENTH PAIRS OF CEREBRAL NERVES.

By G. Negri, M.D.

[Continued from p. 751.]

Bellingeri's Opinions on the Ganglionic Portions of the Fifth Pair of Cerebral Nerves.

What Sir Charles Bell's opinions were, in 1821, on the functions of the fifth pair of cerebral nerves, have been stated in my first article. He gave, in his fourth paper, read before the Royal Society on June 19th, 1823, the following definition:—

"The fifth is the universal nerve of sensation to the head and face, to the skin, to the surface of the eye, the cavities of the nose, the mouth and tongue, and the manducatory nerve." (See Bell's last work, p. 217.) How those opinions were altered, in 1829, with regard to the smaller branch of the same nerve, has also been said.

What Bellingeri's *real opinions* were, in 1818, on the use of the smaller portion of the fifth pair of nerves, I hope has been satisfactorily proved by his own words. It is for him a nerve of voluntary motion, possessing no sensation: in certain circumstances it acts also as a nerve of involuntary motion, or of instinctive action; but this property is bestowed on it by the intermixture with the inferior maxillary branch of the ganglionic portion, and then it is to be considered as a compound nerve: both this last and the smaller portions of the fifth take their origin from distinct parts in the encephalon.

Now I must come to what Mr. Alexander Shaw very properly considers the most important point of this question—viz. to the larger or ganglionic portion of the fifth nerve. With the view, however, of soon satisfying Mr. Shaw, let me bring forward at once just the two concluding sentences in Part III. of Bellingeri's Dissertatio, of the first chapter, of which the title is, "Portionis majoris quinti paris *usus, consensus, influxus*." (p. 119.)

"Statuam igitur," says he at last, "*quintum par nervorum esse sentientem, et motorium vitæ organicæ in capite.* Quæ de quinto pari generatim diximus de ipsius portione majori tantum intelligenda." (p. 175.)

Bellingeri's so called *Dissertatio Inauguralis*, contains, properly speaking, six distinct ones; the different subjects of which are the following:—I. *Ex Physica: De Physico-chemicis albuminis proprietatibus.*—II. *Ex Anatome: De nervis faciei.*—III. *Ex Physiologia: Quinti, et septimi nervorum paris functiones.*—IV. *Ex Medicina Theorica: De Neuralgiæ faciei.*—V. *Ex Praxi. Cura Neuralgia faciei.*—VI. *Ex Materia Medica. Specimen de remediis nervinis.* It is evident, therefore, that the leading subject of the author was principally the general pathology and treatment of the *Neuralgia Facialis*, and that he had very judiciously prefaced his remarks by the anatomy and physiology of those parts and organs which, from their morbid condition, were to be restored to their natural state.

Both the anatomical and physiological

parts leave nothing to be desired in point of clear and minute exposition, as well as of information taken from the most celebrated authors who have distinguished themselves on this subject. The opinions and authorities of Eustachius, Faloppius, Willis, Vieussens, Santorini, Winslow, Haller, Meckel, Hirsch, Wrisberg, Cotunnio, Scarpa, Soëmmerring, Bichat, Malacarne, and others, are brought forwards when necessary to settle some disputed point; and with such a degree of ingenuity, without pretension, which is indeed highly creditable to the author.

He does not, then, come forward as a discoverer of new things, but modestly exposes the ingenious deductions of his patient and extensive inquiries on the structure, distribution, connexions, and functions of those parts which make the subject of his work. Of the correctness of my assertion, every one may have a convincing proof, from the very beginning of the already-mentioned chapter.

§. I. " Disputatissimam aggressurus partem, omnino certa non proferam, satis, si quæ probabili magis conjectura, assequi licet, afferam. Vidimus porro in præcedenti dissertatione, quintum par multiplicibus in partibus inseri, quarum omnino varii sunt usus. Sunt, quæ sensui obediunt voluntario, sunt quæ involuntarie, vel automatice moventur partes, sunt tandem, quæ secretionibus dicantur; omnes denique partes vitam vivunt organicam. Sed quasnam exequitur actiones quintum par? Definituri, antea consulamus auctores.

§. II. Galenus jam tradidit, nervum quinti paris, quem sub nomine tertiæ conjugationis designat, sensui tactus, et motus voluntario dicatum esse, in super et ramos qui in toto ore distribuuntur gustui inservire."—*(De usu partium*, lib. ix. et xi.)

" Willis docuit, sensibus formulari quintum par, tactus scilicet, et gustus; motus etiam, sed involuntarios, vel ab instinctu perficere, asseruit; sympathice, quoque, et pathetice affici, atque moveri: actionibus etiam involuntariis (sive organicis functionibus) dicatum esse ramum lacrymalem ophthalmici, aperte innuit, visui quodammodo, et olfactui inservire; sanguinis circulationem in facie immutare*."—pp. 119, 120, *(Cerebri Anatomes*, cap. xvii.; *Nervor. Descript. et Usus*, cap. xxii.)

And so on, with Vieussens, Meckel, Soëmmerring, Bichat, Gall, and Boyer.

§ III. " Multiplicatas igitur admodum, diversique ordinis exequitur functiones, secundum memoratos auctores, quintum par. Partium vero, in quibus inseritur, vitæ organicæ præ primis conferre par quintum, nobis ratio suadere videtur.

§ IV. " Et primum quidem, argumento ex anatome ducto, peculiaris ipsius structura, quæ cum nervis vitæ organicæ quammaxime convenit, intertexta filamentorum dispositio, gangliorum ubique præsentia: [Præter plexum semilunarem, ganglioformem dictum, convenit ad efformandum ganglion ophthalmicum, ganglia quoque ciliaria a Reil descripta; ganglion in sinu frontali a Wrisbergio indicatum efficit; ganglion spheno-palatinum, et ganglion maxillare a Meckel inventa.] mire repetitæ ipsius anastomoses, ramorum interdum volumen adauctum, ut in ipso quinti trunco in ciliaribus, in nasali externo cum Boyer, et in palatino posteriori cum Scarpa in anatomisis vidimus, quod similitudinem cum intercostali constituit; constans cum arteriis associatio, quod ex Bichât spectabilem sistit nervorum vitæ organicæ characterem, prolatæ opinioni favent. *Fortasse et ex ipsius origine duci argumentum potest; fere enim ex integro ortum trahere videtur a corporibus olivaribus, quæ merito uti ganglia habentur.*" [" In ea sum opinione, ut credam, *nervea filamenta quæ a corporibus olivaribus ortum ducunt, unice vitæ organicæ famulari.* Revera oritur ex ipsis maxima ex parte imprimis *nervus pneumo-gastricus, glosso-pharyngeus, trifacialis: aliqua etiam filamenta accipiunt sextum, et septimum par*, undecimum etiam*, *vel magnus hypoglossus.* Verum nervi priores unice vitæ organicæ famulantur; *posteriores vero, plus minusve, exquibus tantum in partibus.*"] —Pp. 121, 122.

§ V. " Majoris momenti sunt argumenta, quæ physiologia suppeditat. Revera quam distribuitur iridis, glandulæ lacrymali, pituitariæ narium membranæ late expansæ, sinibus maxillaribus, sphenoidalibus, frontalibus, dentibus, internis aurium partibus, glandulis salivaribus cunctis, glandulisque mucosis, glandulis tonsillaribus, pharyngi, et periostio, non nisi vitæ organicæ munera adimplet. *Verum qui-*

* See Mr. Joseph Swan's article, in the Med. Gaz. July 19, 1834.

* He adopts Soëmmerring's classification of the cerebral nerves.

dem est, et musculis voluntatis imperio subjacentibus prospicere, et tegumentis; sed animadvertamus velim, *non ibi solas existere quinti paris propagines, sed alias statim accedere:* sane, dum supra, et infra orbitum egreditur, ad tempora, et in regione malarum, prope foramen menti, *nonne statim filamenta septimi paris superveniunt, et intma anastomosi conjuncta cum ramis quinti, unicum fere cum ipsis nervorum constituunt?* Si igitur ubi solæ sunt quinti paris ramificationes, uti in ramo lacrymali, nasali, dentalibus, et palatinis, ibi sola est organica vita, *nonne probabile redditur, quum in musculis, et integumentis frontis, labiorum, nasi, et oris, universæque faciei distribuitur,* ipsorum tantum vitæ organicæ famulari, *animalem vero vitam, motum scilicet voluntarium, et sensum animalem, ab adjunctis nervis pendêre?* Profecto quum sensum organis distribuitur quintum par, *quod organici est,* ab ipso dependet, *quod vero animalis,* a peculiaribus nervis ipsis organis prospicientibus.....Physiologia igitur, organicæ vitæ inservire quintum par suadet."—P. 124.

§ VI. "Tandem quintum par organicæ imprimis vitæ dicatum esse, comprobant ipsæmet observationes pathologicæ. Re quidem vera in neuralgia faciei rami infraorbitalis instituitur sectio neque inde musculorum subsequitur paralysis. Huc etiam facit fortasse observatio monstri a Lawrence descripti, in quo encephalum fere ex integro deficiebat..... Hoc antem monstrum veram animalem vitam non vixisse, sed potius organicam...."

§ VII. "Dum interim statuimus vitæ organicæ *præcipue famulari* quintum par, patet profecto, *et animalem vitam consequenter,* ex ipso dependêre: læso enim *organico partium existendi modo, et animales ipsarum necessario præpediuntur functiones.* Hoc pacto sequentem explicamus pathologicam observationem."—Pp. 125, 126.

Here he relates, in support of this statement, a case by Bellingeri, considered as one of paralysis of the ganglionic portion of the fifth, but by Mr. Alexander Shaw declared "*an undoubted* case of disease of the portio dura." After the very able manner in which Mr. John Walker has answered him, I do not consider it necessary to say any thing more on this particular subject*.

* See Mr. John Walker's article in the Med. Gaz. for August 16, 1834.

§ VII. "Verum neque tantum solæ partium organicæ præest quintum par, sed *et affectionibus patheticis, sympathicis, involuntariis experimendi inservit.* Animi pathemata præprimis per ramificationes quinti paris mire in faciem depinguntur."—Page 126.

After having given a beautiful description of the effect of the different passions on the change of physiognomy, he says—

"Esse porro productiones quinti paris, non autem septimi, quæ diversimode ab animi pathematibus *afficiuntur;* inde dignoscitur, quod in variis animi passionibus *non soli afficiuntur musculi, sed et vasa sanguinea, et glandulæ quam plurimæ, multæque partes, quæ dumtaxat a quinto, nullos vero a septimo nervos accipiunt.*" . . . (p. 127.) "Quintum igitur tantumodo est par, non vero septimum, quod involuntaria animi pathemata exprimit. [Quarti præcipue, quinti, et sexti paris nervi, pneumogastricus, et intercostalis, sunt, qui influxum pathematum animi sentiunt, et manifestant; isti vero *aliqua,* vel *maxima ex parte a cerebelli productionibus exoriuntur;* putarem inde, posse quadantenus conjecturari, *cerebellum, præ aliis encephali partibus, organum, et sedem esse animi pathematum.*"] pp. 127, 128.

"Sympathice præ primis moventur quinti paris propagines. Sympathias vero . . . . in *proprias* quinti paris, et *communes* cum aliis nervis hic loci distinguo."

The proper sympathies are very clearly explained by the numerous connexions of the fifth with other nerves. The common, or passive sympathies, are explained in the same manner, through the connexions which the fifth has, by means of the great sympathetic, with every part of the body, and with the brain, through its conjunction with the sixth. Then he says—

"Jure, meritoque Wrisbergius quintum par inter *sympathicos nervos* recensuit, et sympathicum medium dixit." Bellingeri's opinion is, that "nervus intercostalis sympathicus magnus est dicendus, sympathicus medius pneumogastricus, parvus sympathicus trifacialis, et *sympathicus minimus facialis dicendus.*" p. 143.

Here it is remarkable what he says on this subject.

§ XVII. "Putarem sympathias in animales, et organicas esse distinguendas; priores quidem a cerebro, non vero secundæ dependent. Animalis fere sem-

per est sympathia, quæ facit, ut per olfactum, visum, auditum, alimentorum, vel veneris excitentur desideria; hæ cum animi peragantur conscientia, et voluntatis imperio præpediri possunt. Organica sympathia est, quæ a cerebro inconsulto, et invito perficitur; renum adest calculus, movetur ventriculus, et vomitu premitur, inscius est animus, reluctaretur si datum esset. . . . . Organicas igitur sympathias a nervorum anastomosibus, plexibus, et gangliis ut plurimum pendêre arbitrarer." p. 145.

§ XIX. "Non denegandum interea, ad nonnullas partium sympathias per nervos explicandas, ad ipsorum communem originem sive in medulla oblongata, sive spinali necessario esse confugiendum. Punctum vero in encephalo, ubi perficiuntur sympathiæ organicæ, nobis statui posse videtur in corporibus olivaribus; revera ex ipsis exoriri videntur maxima ex parte præcipui nervi sympatici vitæ organicæ, intercostalis nimirum, pneumo-gastricus, et trifacialis; quod magis comprobaret, corpora olivaria vitæ organicæ dictata esse; atque ita ratio rederetur cum encephali concursu, absque ulla tamen animi conscientia.

"Sunt igitur sympathiæ, quæ a cerebro, sunt quæ a spinali, vel oblongata medulla dependent, sunt quæ etiam per simplices nervorum anastomoses, plexus, et ganglia fiunt, et explicantur. Animales vero sympathias cerebro intercedente fieri credam; organicas, et per simplices anastomoses, plexus, et ganglia absolvi posse contendo, quæ quidem non in nervis vitæ animalis, sed tantum in nervis vitæ organicæ perficiuntur. Hinc cum tot, tantusque quinto pari peragi sympathias demonstravimus, vidimus quoque in ipsius anatome, suos omnes ramos inter se, et cum vicinis nervis ubique anastomosibus jungi, ita ut intimus sit omnium fere filamentorum connexus, et circulum quodammodo præseferant; multiplices quoque, et insignes instituit plexus, gangliisque frequenter obsidetur quintum par." p. 147.

Here I must end my second article, leaving the remaining part, on the ganglionic portion of the fifth pair of nerves, for the following week.

I am, sir,
Your obedient servant,
G. Negri, M.D.

August 22, 1834.

SUFFICIENCY OF THE PROVINCIAL SCHOOLS.

To the Editor of the Medical Gazette.

Sir,

Though averse yourself to dispensing with the regulation of the Royal College of Surgeons, requiring the student to pass six months in London, yet I trust you will insert in your journal what I offer, why this restrictive clause should be withdrawn.

I regard the setting up any comparison as to the respective qualifications of the metropolitan and provincial surgeons, as wholly irrelevant to the question. I am not one of those who would presume to pit myself against the constellations of London; but still I have an idea that I possess sufficient information to instruct pupils, so as to render them capable of passing an examination in the department I profess to teach.

The metropolitan surgeons possess, of course, numerous advantages; they are, or ought to be, impelled to pursue the path of laborious investigation and research, by a combination of excitants. Following in the wake of the by-gone and renowned, they have to sustain the repute of their localities. He who traverses the same ward as once did Cheselden, must be, or ought to be, warmed by the recollection. There are, too, now in existence among them, those who have earned imperishable repute—men whose writings serve me as a beacon and a guide in the hour of doubt. There are others who tread in the same career, who will be one day celebrated; but there are many, very many, who I am sure will not. High-sounding titles, family influence, the playing off tumid bombast, and skirting on the confines of quackery, will never persuade us, common-sense provincialists, that we ought to range ourselves in the rear of men of such a stamp, though residents even in the metropolis.

Taking the superiority of the metropolitan teachers for granted, is it necessary the imperative clause should be retained? The certain conviction of the fact, and the recollection, that the Examiners of the College are the judges as to the candidates' acquirements, might safely induce the withdrawal. The talented and skilful will always be sought

after by pupils, who are all desirous to attend the best preceptors ere they shall settle as practitioners. Every provincial teacher will say to his class, gentlemen, if you can afford it, pass a winter not only in London, but as well in Paris, Edinburgh, Dublin. The mental powers are enlarged and strengthened by collating a variety of objects. You will be led to analyse and test, to ascertain the validity of what you might else have regarded as incontrovertible. You will thus be enabled to discard that which is worthless, and to stamp indelibly in memory that which else might have been forgotten.

Restriction associates the idea of monopoly; this is a term that comports not with an art like ours. We can understand why a monarch should grant a monopoly for a term of years to a band of enterprising merchants, or voyagers; it is a stimulus to efforts by no means certain of success. A royal charter, empowering a certain number of surgeons to superintend the interests of their art, could never have contemplated that they would have employed themselves in enacting by-laws that tend to concentrate talent, in place of diffusing it. The Sovereign could but be anxious that the inmate of the poorest cottage should be as skilfully treated as the owner of a palace.

I am sure the Council and Examiners of the College are individually men of liberal feeling, profound acquirements, and high urbanity; that in their private walks of life, they would spurn at the idea of being actuated by any thing bearing too strong a stamp of self-advantage; and that they would not wound the feelings of any of their brethren. As a corporate body, they present a very different aspect. Self-elected, irresponsible, residents in London, they have ever been intent on advancing the interests of their circle and connexions. It could not be otherwise. A House of Commons, elected by metropolitan constituents, would naturally be forgetful of the condition of the provinces.

I think I may assert, that if the Council of the Royal College were influenced by a desire to improve their art, they would have recommended that teaching should be commenced at all the principal country hospitals and dispensaries. They would have said—"hospitals are not wanted for teaching, but teaching is for hospitals. We pray you, therefore, gentlemen, to begin as soon as possible. In place of hurrying through your wards, detail to those who accompany you your reasons for the adoption of your treatment; do not put up your fractures in silence, but describe the state of the parts — predict what you expect will result — explain what you behold—read to your pupils from the book of nature. We can assure you that if any thing can induce a rash practitioner to pause—an inconsiderate one to reflect—it is the being obliged to describe to those around the reasons of their diagnostic, and detail the steps of the operation they are about to perform. The attempt to teach will serve to elicit talent that would otherwise have been dormant—will cause a research into, and perusal of, authors that would else have been disregarded. Surgery, too, is a practical art; but lectures are only of use to those who are tolerably versant therein, and who desire to compare their own deductions with those of the lecturer. Hasten, therefore, to adopt clinical instruction."

What has been the fact? The enactment of by-laws, announcing that three years' hospital attendance in the country was equal to one in London; and latterly, that six months in the metropolis is equivalent to twelve.

I am not one who desires that medical education should be rendered cheaper. I believe that the master-key to an effectual elevation of the class of surgeon, would be, that no pupil should be allowed to article, apprentice, or apply himself to the study of surgery, without previously undergoing an examination (by a local board) as to his scholastic attainments. I am inclined to believe that youths of a higher intellectual order would thus be obtained. The public at present appears to suppose that not a whit better education is required for an apprentice to a surgeon, than to a grocer.

Twelve months' attendance on a London hospital, or six months, and twelve months at a provincial hospital, is deemed sufficient to qualify for examination. I would have this period doubled. I am at a loss to conceive how an apprentice from many of the cantonments (if I may be allowed the term), where neither machinery nor mines are

to be found, can acquire, in twelve months, a knowledge of surgery to fit him for practice.

I would suggest that the examination as to the qualifications of the student should be a very different process to what it now is: if, besides being oral and in public, it required written answers—if the parts were shewn on the subject, and if the capacity for distinguishing the nature of ailments and accidents were ascertained at the bed-side and admission-room of an hospital—I have an idea the fitness of the aspirant for a diploma would be better known than by the existing imperfect mode.

Assuredly the primary object of the Court of Examiners of the College must be to ascertain the quality and extent of the professional knowledge of a candidate. Whence the necessity for inquiry at what sources it was obtained? If found to be qualified, the purport is fulfilled: no unqualified person will be able to practise.

The principal argument, I presume, for the council legislating as to the "where," is, that it may be presumed or asserted that London, Dublin, Edinburgh, Glasgow, and Aberdeen, possess better teachers and more richly-furnished hospitals than any other part of the empire.

This is the only one that ought to weigh with the legislature. If the fact, and it is deemed unadvisable to attempt the formation of new teachers and new schools, it would be well, perhaps, not only to retain the restrictive clause, but to oblige every pupil to receive his education at one of those places.

The impracticability, however, of such centralization—the impolicy of allowing the early years of apprentices and pupils to glide by without any species of mental culture, and of neglecting the riches that are to be found in the large provincial hospitals—the injustice, also, of expecting that surgeons will apply themselves to teach ardently and energetically, when held up by their brethren, who legislate, as a second class,—will, I think, be evident at a glance.

Aberdeen *versus* Manchester is a good sample of antithesis; but it may be said the professor of surgery there is profoundly read, eloquent, and didactic. I am glad of it; but he can have very few cases on which to display his talents. I would prefer that one of the Manchester surgeons should describe, in humble style, his numerous cases to his pupils; he will, at all events, have the advantage as to material.

The centralization plan narrows the field of competition,—freedom would extend it. Should any unqualified persons venture to become teachers, the remedy, as I have said, is a public, honest, and searching examination; the examiners, if possible, selected by government, liberally paid, and unconnected with either hospital or school.

A board, hung up at the college door the day after an examination, with the names of the passed and rejected pupils, or at all events the names of the teachers whose pupils have passed or not, would be an excellent guide for a student. I am inclined to believe that pupils or their friends may very safely be left to choose for themselves. A monthly sheet of the kind I have mentioned would, however, be an excellent chart for their guidance.

If teaching were general at the country hospitals, it would soon lead to the subdivision of labour; and this, generally speaking, would tend to the good of our art.

I believe I have seized on a few of the prominent points of the question; many minor ones yet remain; but, to avoid being tedious, I beg to subscribe myself,

Your obedient servant,
WILLIAM GILL.

Liverpool,
August 4, 1834.

MEDICAL GAZETTE.

Saturday, August 30, 1834.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."
CICERO.

THE LATE CONCOURS IN PARIS.

WE mentioned last week, that the Concours in Paris for the chair of *Clinique chirurgicale*, left vacant by the death of Boyer, was concluded; and that M.

Velpeau had obtained the nomination. The candidates were most of them men of high professional character: the names of Lisfranc, Sanson, Blandin, and Berard, are familiar to every medical man who attends to the literature of his profession; and the merits of M. Velpeau are generally appreciated throughout Europe. Not only has the latter gentleman distinguished himself by his various and elaborate works, which have procured for him an extensive celebrity, but by his indefatigable industry and ability as a teacher, he has long been a prime favourite in the French metropolis. Added to this, he has had the advantage for some time back of being looked upon as a man of brilliant talent unfairly dealt with, and possessing qualifications for a professorship in the school, which were not liberally recognized in influential quarters. Within but a few months he has publicly contended for no less than three successive chairs—for *Pathologie externe*, *Clinique d'Accouchment*, and the surgical *clinique* just mentioned. On each occasion he has acquitted himself with the highest applause; and his want of success at the conclusion of each of the two former trials created considerable murmurs among his admiring friends. His present triumph is accordingly hailed both as an act of justice and a tardy acknowledgment extorted from a reluctant jury.

The Concours which has just terminated was a fair specimen of that truly French mode of appointing to medical offices: it was more; for owing to the undoubted and unquestionable eminence of the *concurrents*, it could not but go off to the heart's content of our lively neighbours — *brilliantly*. We watched the proceedings, as reported in all the French journals, and must confess, not without interest, as it was conducted from the first with much decorum—that is to say, with comparatively little of that rude interference with the business of the jury, and the conduct of the candidates, which generally renders every exhibition of this sort, in "young France," so exceedingly offensive. On one of the days of the contest, indeed, there was no slight disturbance produced by some scandalous reports circulated to the prejudice of one of the candidates, M. Blandin, who, it was whispered, was supported by the patronage of certain persons in power; and billets to that effect were sent round among the public, or rather the pupils, assembled in the amphitheatre. One of them having reached M. Blandin just as he was proceeding to deliver a probationary lecture, he took the opportunity, on coming forward, to protest, in the most indignant terms, against the injurious insinuation; and so effectually did he vindicate his character, that a strong reaction in his favour immediately took place, and he was cheered from all parts with the loudest applauses.

What scenes these are (and they are quite common in the Parisian amphitheatres) to be permitted to take place on such an occasion! The pupils of the school, it is but too manifest, have it all their own way, and claim to be the supreme directors and judges on every recurrence of the concours; they raise at will the most uproarious opposition to any thing or any person that displeases them: they drown with their plaudits the proceedings at any moment, when anything has been said or done to give them pleasure. Were we ill-disposed towards the concours—as we certainly are not, so long as our Gallic friends keep it to themselves—we should wish all parties joy of this tyranny. But, as it is, we cannot but condole with the judicious, who see in the prevalence of the custom, no-

thing but an opening for the most serious ill consequences. Who but the most blind admirer of revolutionary novelties fails to see in it the risk of bringing forward bold bad men—persons of superficial attainments, but gifted with those arts which are suited to secure them popularity in the theatres? When this is done in a few instances—and there is by no means a remote possibility of its occurrence—adieu to the beloved concours, and let the school suffer for its folly! We know, however, perfectly well, how little hope can be entertained of a reform on this point. The love of power is too strongly implanted in human nature to be readily surrendered, especially by a multitude of hot-headed young men who have once enjoyed it; and the *display* which is connected with it, we are aware, has strong attractions,—being, as it is, so very peculiarly French.

The usual routine of trials gone through, as constituting the several acts of the concours, is generally known; and there was nothing so unusual as to require special remark on the present occasion. The claims of the candidates on the score of distinction in the profession *(titres antérieurs)*, as authors, or improvers of the medical art, were all duly set forth; the required lectures on given subjects, and with little or no preparation, were severally delivered; and the bed-side proofs of ability were satisfactorily tendered. But perhaps the proceedings with regard to the theses may deserve a short notice; for, with all due respect for the learned projectors of this sort of test, we cannot conscientiously give it much of our approbation, and for that reason cannot allow this opportunity to escape without in some measure expressing our opinion on it. The practice is, to draw by lot certain questions for the candidates, on which they shall have each a printed thesis to produce in the space of ten days, proving their authorship of the same by supporting their respective compositions in disputation with their competitors. Now, what sort of a work can possibly be produced in ten days—especially on a chance subject? Be the candidate who he may, it is impossible for him honestly to bring forth all his resources in a regularly written and printed treatise, such as is expected from him, in that period. A thesis, no doubt voluminous enough, and perhaps compact and well put together, may be forthcoming; but for its originality, we do not hesitate to say that it will be as much his who opposes, as his who defends it in *argumentation.*

And this was pretty well shown by the late result. Seven theses, on a variety of subjects, were ready by the appointed day; but all were allowed to be mere compilations, and scarcely worthy of the men who composed them. Some of the candidates had to treat on subjects of which they had *no experience.* M. Sanson, for instance, had to give a lengthy statement of the comparative merits of the method of union by the first intention—a method of the real merits of which he *confessedly* knows nothing; and it was generally understood, on the allotment of the questions, that not one of the candidates had specially applied himself anteriorly to that particular subject which fell to his share. The professed object of this proceeding is to ascertain the powers of each *concurrent* as a writer. But how much more certainly might this be effected, if desirable, by setting down the candidates at once, with pen, ink, and paper, and obliging them to supply an extemporaneous essay on a given subject, in a given time, rather than by allowing ten days for the preparation of a thesis, with which persons behind the screen have infinitely more to do than the nominal author!

We should not, however, omit to mention a few facts touching the extraordinary thesis of M. Velpeau, whose well-known fecundity as a writer, renders it not impossible to believe that he may himself have really got it up. It proved to be a *book*—we can hardly call it a mere thesis—on the subject of the *Trepan*, running to 270 printed pages, in quarto! This is surely a phenomenon in authorship: allowing for the time of printing and correcting the press, we cannot calculate on less having here been written than thirty quarto pages a day! We often had our doubts about Dr. Johnson's having written, as he said he did, forty-eight pages of the Life of Savage at a sitting; but we confess M. Velpeau's alleged feat of thirty pages a day continuously, for at least nine days, while it certainly keeps those doubts in countenance, hardly tends much to remove them. M. Velpeau, it is possible, drew the skeleton of the work, and opened the sources of compilation to his assistants; or at best, perhaps, he dictated the whole to certain short-hand writers: but on any supposition, the fact is a remarkable one, and has given rise to much conversation in Paris. It is scarcely necessary to add that his opponents detected several errors in the production; some of which the author endeavoured to apologize for, on the ready plea of haste and inadvertence.

MORE QUACKERY.

In the new order of things which we are led to expect is to take place in our profession, we earnestly hope that one change for the better will be, that whatever body or bodies are established shall have the power of expelling from their halls, and expunging from the list of their members, all those who have recourse to unprofessional modes of gaining notoriety. How many do we see around us over whom the dread of such exposure would exert a salutary check! and how absurd it is, that they who degenerate into common advertising quacks, should still be allowed to affix to their names titles of honour! We have seen a Fellow of the College of Physicians associating himself with St. John Long, and yet he remains a Fellow, though no longer summoned to the College meetings. We have seen, and daily see, the walls about town plastered over with the indecent hand-bills of certain rivals of Dr. Eady, who designate themselves "Members of the Royal College of Surgeons." Nor is this disgraceful system by any means limited to those notable instances: we have reason to fear, if we may judge from some specimens which have recently been forwarded to us, that the practice is much upon the increase. Among those documents we find one in the shape of a hand-bill, bearing the signature, "R. S. Richardson, L. S. A., Member of the Royal College of Surgeons, &c.;" in which all the varieties of impudence and ignorance seem happily blended. The advertiser professes to be able to cure malignant cholera, "having saved ninety-three persons out of a hundred confirmed cases of the disease!" How does the reader imagine this is to be done? Simply by taking "two or three doses of medicine, prescribed by a medical man skilful in managing the disease,"—meaning, of course, Mr. R. S. Richardson, L.S.A., and Member of the Royal College of Surgeons!

Again, the public is cautioned against consulting any of the older practitioners. "Many of the old surgeons are grossly ignorant, so that their practice is falling into the hands of younger and better qualified men,"—viz. we presume, into those of R. S. Richardson, L.S.A. and M.R.C.S.

But, then, our puffer is a great philanthropist; for he offers to become the guide of physicians and surgeons—who, especially the former, according to him, are very inferior persons; and, God knows, he could not give a greater proof of his estimate of their ignorance than is contained in the proposal which immediately follows—viz. that they should apply to him for instruction! "As it might be a public benefit, I shall be happy to instruct physicians, surgeons, and apothecaries."

As to the L.S.A., Mr. R. S. Richardson may or may not be entitled to append to his name these same letters, the meaning of which we profess not to understand; but we are quite sure that it is very disgraceful to any College of Surgeons that its members should be capable of such quackery; and we hope to see so glaring an abuse speedily reformed.

EXCLUSIVE PRIVILEGES.

LONDON *UNIVERSITY* CHARTER.

THE question of the London University Charter may be looked upon as settled. It is satisfactory to us, however, to have ascertained, and may not be uninteresting to our readers to be made aware, that the learned Professor of Medicine, in the evidence which he lately gave, fully concurred in the opinions, on this point, which we have so frequently laboured (and we are happy to say, not laboured in vain) to inculcate. We subjoin an extract, in illustration, from

Dr. Elliotson's Evidence before the Parliamentary Committee.

"1745. What opinion do you entertain of the expediency of allowing any particular school of medicine in the metropolis to have the power of conferring medical degrees?—I should think it very objectionable to give to any one particular school a monopoly.

"1746. Either to the King's College, or to the London University?—Certainly.

"1747. Do you think that this power should be vested in the College of Physicians?—Certainly not.

"1748. Do you confine that answer to the College of Physicians, as it now exists; or would you entrust to it that power if it were reformed?—Under no circumstances should I think it advisable.

"1749. Will you state why no such power should be entrusted, first to any particular school of medicine in London; nor secondly, to the College of Physicians?—I do not think any one medical school has a right to have advantages secured to it above another: but that is not an objection to any University, either King's College or the London University, granting certain degrees; because a person who takes a degree in medicine, ought, in my opinion, previously to take a degree in arts; and the want of this rule is a great objection, I conceive, to some of the Scotch degrees. Perhaps it must be in such a place as King's College or the London University, that a previous degree must be taken; and therefore I hope that one or both of those will have the power of granting degrees in arts: but when it comes to the medical degree, very likely a board composed of examiners from different schools might be advisable. I would not do any thing that could give an undue advantage to the London University or to King's College, any more than to any hospital.

"1750. You think it would be expedient that there should be a general board in London, having the power to confer medical degrees; but you do not think that board should be composed exclusively of persons attached to any particular school of medicine?—No: and I think that those who go before a board, ought not to have been necessarily educated at a particular institution; but whether a man comes from St. Thomas's Hospital, or from St. Bartholomew's, or from King's College, or any where else, for his examination, if he can pass it, he ought to have his medical degree, provided he have taken his degree in arts.

"1751. Why do you think the power of conferring medical degrees should not be vested in the College of Physicians?—I think the College of Physicians, while it exists, ought to be above all bodies that grant degrees; that it ought to be able to stop any one that has passed any other place, without deserving it. At all universities there are men who get through and obtain degrees, without deserving them; this will happen, and the use of the College of Physicians is to check the universities, to examine men a second time, and to make their qualifications certain. I believe that in many countries, the students first of all take their degrees in the respective universities, but, before they practise, are obliged to present themselves to a supreme body, for a second examina-

tion: and that, I conceive, is the purpose of the College of Physicians.

" 1752. Suppose a degree given at the College did not necessarily imply a license to practise?—Then the College would be acting in two capacities, giving degrees like the universities, and re-examining men who had already obtained degrees in universities. But I think the use of the College of Physicians is to give a license to practise, the man having previously obtained a degree somewhere else.

" 1753. May not a good medical education be had in London?—Certainly.

" 1754. Is it not an inconvenience, that a medical student should be compelled to go to a distant university, merely to comply with the form of obtaining a degree?—So much so, that I hope London will have the power of granting degrees, to save them that journey.

" 1755. If the College of Physicians had the power of granting, as well medical degrees as licenses to practise, might it not be subject to the imputation, in granting licenses, of favouring its own graduates?—There would be that objection, and I think it ought to examine none but graduates from other places.

" 1756. Are you decidedly of opinion, that the College of Physicians, before giving a license to practise, should subject those who come before it to examination?—I think so, because, when I was censor, I saw men of different universities, who had satisfied the professors of those universities, pass in very different ways, and some not at all.

" 1757. What means should the College take to satisfy itself, that those who come before it, not only have received a good medical education, but also that preliminary education, which you have stated you think desirable?—It must have a knowledge of all that is going on in the universities from which men come, and, in some cases, it may be contented with knowing, that in universities from which a man comes there are excellent regulations and strict discipline; and in other cases, where a man comes from a university which they have not reason to think so highly of, they might subject him to a scientific and literary examination; they might have the option of doing it always.

" 1758. Does not this difficulty now attend any university, in endeavouring to enforce by examination a superior course, either of preliminary, or of strictly medical study; that there are other universities that will confer degrees to those less qualified, and therefore at a cheaper rate to students?—Certainly. At this moment, I believe, that England is becoming inundated with degrees from Heidelberg; which, I am told, may be procured by a friend who is travelling that way; as they might formerly be obtained from certain universities in Scotland.

" 1759. Ought the licence of the College to be merely a title of honour, and certificate to the world that the party licensed has really received a competent medical education; or ought it to convey to its possessors an exclusive title to practise as physicians within the precinct of London; which title those who practise within that precinct should be required to possess, and which all that do not possess should be excluded from so practising?—I think there should be a licence from the College of Physicians, or from some supreme body. I think there ought to be a body or bodies to license all practitioners.

" 1760. That none should be allowed to practise within the precinct of London as physicians but those who possess a certificate from the body appointed to grant certificates?—I think so, because persons will employ the most ignorant. A man who is very ignorant may obtain a large practice.

" 1761. Has it been found practicable to exclude irregular practitioners from practising?—It is not done.

" 1762. Considering the number of qualifications which entitle persons to practise physic, either as surgeons, apothecaries, or even druggists, where is the utility of excluding from medical practice those physicians who have not received a licence from the College?—Certainly it is infinitely more injurious to exclude a physician who has not a licence, if ignorant persons are allowed to practise.

" 1763. If he is refused a licence from the College, may he not practise physic under another title?—Yes, he may practise as a physician. He has only to say that he is an accoucheur-physician, and he is not touched. Of course I think it very ridiculous for a physician, who has not the licence of the College, but who is a well-informed man, to be prohibited from practice; when a set of ignorant people are allowed to practise to the right and left, people who have never been educated at all; and I think it is a great pity that this is not altered.

" 1764. Therefore, if any punishment attached to a physician practising without a licence, do you think the punishment should attach, not to his practice in physic, but to his false assumption of the title of physician?—I think an ignorant man ought to be punished for attempting to practise what he does not understand.

" 1765. Supposing that, besides surgeons and apothecaries, druggists are able, without subjecting themselves to punish-

ment, to practise physic; what is the use of punishing a man for practising physic, who has the knowledge requisite for a physician, though not the licence of the College?—It shows that the whole system wants reform."

SURGICAL REPORTS
FROM THE
LONDON DISPENSARY.

By R. R. Robinson, Esq.
Surgeon to that Institution.

Case IV.—*Curvature of the Spine—Lumbar Abscess—Porrigo Sceutellata.*

Richard Tichore, æt. 5, admitted February 3d, was seized with fever, said to have been of the typhoid character, three months ago. A stout healthy child previously. About three weeks ago, a swelling was perceived over the lumbar region and inferior ribs laterally, on the right side, which increased. There is now a considerable abscess in that situation; the integuments are very thin and inflamed; the child is worn to a skeleton; all its bones prominent; great depression of the system; restless nights; some cough.

Inf. Gent. Co. ℥iss.; Liq. Potass. ♏v. ter die.

February 4th.—Abscess broke this morning, and upwards of a quart (it is said) of offensive yellow pus was discharged; some still oozes, and is propelled out with much force upon coughing.

7th.—Considerably more lively; appetite very good since the abscess has broken; it was lost before; skin considerably contracted, and much less inflamed; pus small in quantity, and healthy; spine slightly curved posteriorly; pain appears to be produced by pressure on the spinous processes.—Perstet.

25th. — Improved greatly; discharge much diminished. He walked about, but has had more pain in the tumor and spine the last few days, and more discharge, which appears to come upon pressure from the sacro-iliac region; discharge in appearance healthy, but very offensive.

March 4th.—Stronger; walks well; less pain in the back. Porrigo sceutellata of the scalp, with abscess of the glands behind the right ear, from which ℥ij. of pus were let out to day.—Rep. Med.

25th.—Abscess quite healed up; health materially improved; porrigo entirely disappeared; a tendency to atopecia.

Ung. Hydr. Nitr. Dilut.

30th.—Cured.

Remarks.—It does not seem to me clear that the abscess in this case depended upon disease of the vertebra; for, although it is true that spinal disease had existed, yet it did not appear to be going on at the time he was under my care. The pus being propelled with violence upon coughing, shows that it was deeply situated; and its offensive character might induce a belief that it depended upon dead bone; yet deep-seated abscesses form occasionally in this situation, without any connexion with the vertebra,—a case of which I shall presently relate,—and pus, when long confined, is frequently offensive: that therefore, does not necessarily imply that diseased bone must have been its cause. Had so large an abscess been the consequence of disease of the spine, I think the little patient would not have recovered so rapidly as he did. I am therefore more disposed to look upon it as an abscess — the sequela of the fever, from which he is said to have suffered; a sequela which, in scrofulous subjects, the surgeon has frequently an opportunity of witnessing.

Case V.—*Lumbar Abscess—Slight curvature of the Spine—Recovery.*

James Castle, æt. eleven and a half years, admitted March 22d, of strumous make; has a slight lateral curvature towards the lower part of the spine. About seventeen weeks ago he was noticed to drag his right leg after him in walking, which was soon followed by pain in the loins, and inability to move; to this succeeded a distinct abscess over the left psoas muscle, which broke about the 5th of January, and discharged a considerable quantity of pus, which it has since continued to do. There is now a triangular opening of rather large size in the centre of the left psoas region, which copiously discharges a thick yellow pus, which comes more freely when pressure is made below. There is also a redness over the whole left buttock, which extends down the back of the thigh to the ham: the skin is so tender here that the least touch makes him scream loudly. Has lost flesh greatly; appetite bad; bowels open; tongue clean and moist; nocturnal sweats; pulse small, quick; skin dry.

P. P. g. xv. Dec. Cinch. c., Acid. Sulph. Dil. m. x. ter die; Cat. Lini.

March 26th.—Easier, except when touched, and then he cries out lustily; stronger; less sweating; some appetite; copious discharge from the wound; an abscess, pointing low down in the thigh, was opened, and discharged ℥iv. of green offensive pus, tinged with blood.—Perstet.

28th.—Much easier; discharge copious throughout the day of the operation; very little from either orifice since; can bear the skin pressed.—Perstet.

May 5th.—Wounds healed up in a few days; constitutional irritation quickly subsided; pain entirely left. He has been able to sit up the last three weeks: the muscles are recovering their power, lost by long confinement; he has some motion in the right hip, and is able to move about on crutches.

June 1st.—Has gained strength greatly; can draw the right leg up easily; walks without crutches. Discharged cured.

Remarks.—In this case the dependence of the abscess upon disease of the spine is to be inferred from the boy complaining of pain in the back; from his being observed to trip in walking, and from his ultimately becoming incapable of moving. His perfect recovery is very satisfactory.

Case VI.—*Caries of the Lumbar Vertebra—Psoas Abscess—Pertussis—Introsusceptions—Death.*

Thomas Thurman, æt. 6½, admitted 28th of November; a strumous child, ailing from birth; backward on his legs: has frequently tripped in walking for the last two or three years; coughs occasionally; a swelling perceived in the lower part of the back twelve months ago. There is a considerable posterior curvature in the lumbar region, where pressure produces pain. Much emaciated and exhausted; scarcely able to move the lower extremities, which are even more than other parts emaciated. He was seized with hooping-cough and fever, from which, together with exhaustion, he died six weeks after admission.

Among the means employed were purgatives, steel, salines, and tartarized antimonial ointment.

Dissection.—A few tubercles in the upper part of the lungs; right pleura pulmonalis firmly adherent to the ribs; liver strongly attached to the anterior abdominal parietes by long, old membranous adhesions; numerous slight introsusceptions were met with, apparently produced in the act of dying, as the gut was healthy, and there were no preternatural adhesions; mesenteric glands healthy; the third lumbar vertebra much diseased, its body almost entirely absorbed, and converted into a kind of cyst, containing a few ossific spicula and much blood; the ligamentum commune anticum considerably thickened. Behind the peritoneum was a large cavity, containing thickened caseous matter, which at parts had a purulent appearance: this cavity was bounded by the ligamentum commune and iliac fascia. Much serum in the spinal canal, and ramollissement of the medulla spinalis.

Remarks.—The dependence of the abscess upon disease of the vertebra was very obvious. Inflammation appears to have commenced in the body of the bone, producing purulent infiltration, then caseous matter, and ultimately absorption of great part of the osseous texture, and extension of the disease to the spinal marrow and its membranes. As, in addition to the extensive disease of the spine, there were tubercular disease of the lungs, pleurisy, peritonitis, and hooping-cough, it was impossible for remedies to have been of much use.

Case VII.—*Psoas Abscess, with Scrofulous Inflammation of many Vertebræ—Fistulæ on each side of the Spine—Tubercles in the Lungs, &c.*

Ann Studdam, æt. 5½, admitted November 22d; a strumous child; abdomen large; a long time subject to thoracic disease. A tumor was perceived three weeks ago over the lumbar muscles, to the right of the spine; since which the child has been more unwilling to move, though for some time past it has limped in walking, and the lower extremities have much fallen away.

On tracing the spine, there is a considerable posterior curvature in the situation of the two or three upper dorsal vertebræ; pressure at this part does not produce pain. Tumor is hard, though softer than it was; the skin covering it is very red, and has a disposition to point at one spot: a lancet introduced here gave exit to ℥iij. of good pus, with some blood. The matter appeared to come chiefly from towards the posterior sacro-iliac articulation, where a cavity was subsequently left. The lips of the wound were closed; slight pressure was applied here, and a purgative powder ordered every other morning.

November 26th.—Tumor much diminished; no pain, but little discharge; skin not inflamed. Contrary to strict orders, the child has been allowed to walk across the room; has since been and still is feverish. Pulse quick; tongue white; bowels well opened; motions knotty, hard, brown.

P. P. g. x. Rep.; Mist. Salin. Ant. ter die.

28th.—Offensive discharge from abscess; orifice sloughy; fever much subsided; skin yesterday covered with a rash much resembling measles, which she has had; free from it to-day. Tongue much coated; bowels open.

Perstet Cat. Panis.

December 3d.—Seemed better yesterday; was more lively; had but little pain; towards evening was seized with an epileptic fit, and died early this morning.

Dissection.—Right lung firmly adherent to the spine. Both lungs inflamed at parts; a few tubercles scattered through them. Abdominal viscera healthy; mesenteric glands not much enlarged, and not unhealthy in structure; kidneys pale, with pink stripes at parts; some fluid in the chest; the abscess opened was lined with a sloughy membrane, and had a free communication with a fistulous canal, which ran all up the spine, and downwards towards the groin; the ilium not deprived of its periosteum. Ligamentum commune anticum completely destroyed in parts, and full of holes at others; the bodies of the dorsal vertebræ in a state of scrofulous inflammation, and purulent infiltration much softened and broken down; the third dorsal entirely destroyed, and a cavity in its room; intervertebral cartilage unaffected; a slight posterior curvature at the upper part of the dorsal column, and a double lateral curvature; a fistulous canal, with numerous orifices at different parts on each side of the spine; the two sides communicated above through the cavity, in the place of the third dorsal vertebra; the psoas muscles in a sloughy state; medulla spinalis soft; some fluid in the theca vertebralis.

Remarks.—Here we have the same disease pervading many vertebræ, with numerous fistulæ on each side of the spine. The child might probably have lived longer, had it not been for the supervention of irritation from the rash, and the imprudence of the parents in allowing it to walk about, which was productive of inflammation throughout the cyst: it is plain, however, from such extensive disease the child could never have recovered.

The immediate cause of death appears to have been an epileptic attack, owing to the extension of the inflammation to the spine and its membranes producing serous effusion and ramollissement.

Case VIII.—*Psoas Abscess bursting into the ascending Colon—Prolapsus Recti.*

Henry Stone, æt. 34, was admitted under Dr. A. Frampton, on the 13th November, in the last stage of debility, and died the next day. He was a waiter at a tavern; a free liver; received a blow on the back fifteen years ago; was stout and healthy till ten weeks since, when he was seized with pain in the lumbar region, and dyspnœa, for which he was twice largely bled, and leeched on the loins. For the first week the bowels were obstinately confined; purgatives and enemata were used, after which profuse diarrhœa came on, which was not controlled by the means employed. He became reduced to a skeleton, and had all the symptoms of hectic.

Dissection.—Thoracic viscera healthy; stomach and small intestines free from disease; spleen and pancreas healthy, as was the anterior surface of the liver; behind it was preternaturally adherent to the right kidney and ascending colon, the mucous membrane of which was of a deep red colour, and ulcerated in several places. In one spot at this part of the colon, all the coats were completely perforated by a large abscess, which extended from the two or three last dorsal vertebra to the trochanter major, which contained upwards of a pint of pus, some of which appeared to have passed into the intestine, as a fæco-purulent fluid escaped from the rectum on pressure. The posterior part of the right os innominatum, and two last ribs, were deprived of their cartilage and periosteum, and blackened; psoæ muscles much destroyed; bodies and processes of the spine uninjured; right kidney harder than the left, apparently in a state of hypertrophy; lymph upon its external surface. Attached to the upper part of the left kidney was a pancreatic looking substance, the altered remains of the renal capsule. Mucous membrane of the rectum very vascular, and some way prolapsed.

Remarks.—We have here an instance of a large abscess forming in the loins, and bursting into the colon without any apparent cause, and in spite of active antiphlogistic means. The vertebræ were *not* affected here; and the carious state of the ribs and posterior part of the spine was subsequent, I apprehend, to the formation of pus. The inflammation and ulceration of the mucous membrane, as well as the diarrhœa, were attributable to the irritation of the purulent matter that passed into the bowel. The prolapsus of the rectum was also partly owing to the same cause, and partly to the exhausted condition of the patient.

Case IX.—*Psoas Abscess opening into the Colon, and also outwardly—Artificial Anus—Ureter perforated (?)*

Frederick Whindam, æt. 8½, admitted February 18. Was a stout healthy boy before Christmas. On the day after Christmas day he went to a show, and was very much squeezed. Six weeks ago he had pain in the loins, and fulness on the right side of the back, where he was repeatedly leeched, but without benefit; for the pain increased, and extended to the abdomen. Very much reduced; sacrum

nearly through the skin, from the pressure of lying on his back. On the evening of the 16th, the integuments midway between the spine of the ilium and last rib on the right side gave way, and half a pint of fœtid pus immediately escaped. The same night fæcal matter came from the same place so copiously as to soak his shirt as far as the axilla. Much pus and fæcal matter came from the wound yesterday and to-day; fæces have passed also per anum. Debility very great; hectic.

Cat. panis; Pulv. Dov. gr. v. o. n. Dec. Cinch. c. Sodæ Subcarb. ter die.

Feb. 25th.—There was a copious discharge of pus and fæcal matter from the wound until the 23d instant, when the wound closed, and kept so throughout yesterday, when he discharged two copious motions per anum. Much tenderness in the abdomen and sacral region; upon pressure fæces and pus come from the wound.

March 11th.—No motion passed from the wound since 28th ultimo, which appears quite healed up. Bowels open daily; motions clay-coloured, and particularly offensive; less tenderness on pressure; copious nocturnal perspiration.

Ol. Ricini.

14th.—Orifice opened again yesterday, and has discharged pus, and some fæces since.

April 4th.—Orifice closed to-day; has freely discharged till then fæces and matter; more hectic.

Decoct. Cinch. c. Acid. Sulph. D. ℥x. ter die.

8th.—No motion by wound since the 4th; motions per anum regularly; wound closed till yesterday, since when it has discharged a little matter. Much pain about the sacrum; still hectic, but stronger.

May 2d.—Weaker; copious discharge of fæces from the wound, and a large quantity of fluid like urine, but devoid of urinous smell. His mother has noticed this copious flow of fluid three times, at the exact interval of a fortnight. He is in great pain just previous to this; the wound, which is generally closed, opens, discharges a little matter, then motion, and then a large gush of this fluid. Throughout the day that this passes no urine is voided by the urethra, though at other times he micturates copiously night and morning. During the time that this gathering takes place, which used to occupy twenty-four, and now only twelve hours, he is sick, in great pain, and feverish; but these symptoms all subside upon the expulsion of this fluid from the wound. Much pain around the right hip, which he can elevate, extend, but not rotate outwards.

9th.—Pus and fæces continue to be discharged from the wound; he is very tender over the left hip. A probe passed into the wound proceeded in a straight direction inwards three-fourths of its length, but would not pass at all laterally; it was tinged of a deep brown colour.

25th.—Easier; stronger. On the 21st instant, two days within the fortnight, he had a desire to pass urine, but it was ineffectual. After this, the same copious discharge of fluid above described took place from the wound. Motion now comes regularly from the wound daily, between five and six P.M.; he first passes a little by the bowel, and then a large quantity by the wound. A small orifice broke the day before yesterday, below the old place, which discharges a watery pus.

30th.—Small orifices are breaking around the old wound; portions of nuts, lettuces, and other substances, eaten contrary to orders, visible in the fæces discharged from the wound.

Tr. Iodinæ ℥ij. ter die.

June 20th.—Iodine made him sick, and was discontinued. There are now five strumous fungous openings, through all of which fæces and air pass; and yesterday the watery discharge, as before. At times the colon becomes painful and distended by air and fæces, which subsides upon the fæces passing by the wound, which they now do, at irregular intervals. Weaker and feverish; has taken with some benefit the effervescing mixture, with gentian.

July 22d.—Fæces and pus continuing to discharge from the wounds, slight pressure was tried; but it produced so much pain and restlessness, that it was necessary to discontinue it, and immediate relief followed.

Sept. 9th.—Certainly stronger; copious fæcal discharge from the wound; is sitting outside the door.

29th.—Much stronger, and more healthy in appearance. Less discharge of pus; but little fæces by the wound, a great deal by the anus.

Oct. 24th.—Much stronger; has been allowed to run about too much. Wound closed entirely about three weeks ago, and fæces passed entirely by the rectum. It continued closed a fortnight; it then opened, and discharged fæces for a week, but for the last three days only a little pus: there is a little fulness and tenderness about the wound, and occasional gushes of water.

Nov. 11th.—Called at the Dispensary to-day; wounds quite healed; feels well.

Dec. 12th.—Much stronger; general

health very good. Wounds now all healed, though they frequently break out, discharge a little pus and fæces, and now and then the same watery fluid.

Remarks.—This is a curious, not I believe very common, but certainly a very interesting case, as it shews how great sometimes are the natural powers, especially in children, in overcoming very serious mischief. I saw this boy a short time since playing with as much activity as other boys, in apparent health and spirits, though his mother informs me that occasionally some of the orifices open and discharge matter, but that fæces have not passed from them for some time.

It is rather extraordinary that the abscess should have opened into the bowel, and also outwardly, much about the same time. There is no doubt the cause of this abscess was the injury he received in the crowd, though some difference of opinion may exist as to its exact situation. I think myself that it began in the course of the psoas muscle, and then extended towards the lateral parts of the abdomen. There does not seem any reason to suspect disease of the spine here.

One of the most curious features in the case is the sudden discharge of watery fluid from the wound, so frequently and so regularly noticed, which I can only explain by supposing there was a valvular communication with the bladder, or more probably with the ureter of the affected side.

EXTRAORDINARY CASE OF LODGMENT OF A FOREIGN SUBSTANCE IN THE LOWER PORTION OF THE COLON.

Reported by George Moody, M.D.

I was called, on the 5th of April, 1834, to visit a man named Steven Hovey, in North Andover, in the vicinity of the factories. I inquired of the person who came for me the nature of the case. He could give me no further information than that the patient had a pain in the abdomen, and complained of numbness in one of his legs, and requested me to make haste. I hurried as expeditiously as possible to the patient's residence, supposing it to be a case of common colic. On my arrival at the house, I found the patient up, with his clothes on, exhibiting but slight marks of indisposition. I immediately asked him if he had an attack of colic. He hesitated a moment, and then said (looking round to see if any one was within hearing), "I have got a tin tumbler up in my body! and I wish you to keep the circumstance as private as possible, as people will laugh at me." I immediately inquired how he could possibly get it there, and how long it had been there. He simply stated that it had been in his bowels about thirty hours; that he had had a prolapsus of the rectum, (an affection to which he had been subject,) for which I had treated him in 1829, but he had not been troubled with it since, till the present time, when, owing to unusual dilatation of the sphincter ani, the rectum had descended to a considerable extent, and having tried the usual method of reduction with his fingers, &c. ineffectually, he placed a large tin tumbler on the floor and sat upon it, thus forcing up the prolapsus. When the intestine returned to its proper situation, owing to a sudden and violent action of the sphincter and the sudden and quick contraction of the parts, the tumbler passed up with the rectum. He had made an ineffectual effort to extract it with his fingers; he then had recourse to a pair of shoemaker's forceps. With these he had considerably broken and flattened the edge of the base, or rim, of the tumbler, and forced it beyond the rectum, into the colon. This was the situation in which I found it on my arrival.

After examination, considering it as nearly a hopeless case, I immediately placed him in a horizontal position, and, passing my hand and fore-arm through the rectum, seized the substance, and made a powerful effort to extract it; but, owing to the violent contraction of the walls of the rectum and the sphincter, I had not strength to accomplish my object. I then desisted, and had recourse to the blunt hook. With this I brought it down so as easily to distinguish it externally; but, owing to its flattened state, it hitched in the plicæ of the intestine. Finding I had not strength to extract it, if it was practicable, and wanting advice, the Hon. Jos. Kittredge was called. He made another attempt to extract it with the blunt hook, but did not succeed: he likewise said he should like more counsel, and the Haverhill physicians were sent to. Accordingly, about half-past two or three o'clock, Dr. Whiting arrived from Haverhill. I then related the facts of the case as they had come within my knowledge; stated that I thought the sphincter ani might be divided, and an incision made up the sciatic notch, in the part nearest the sacrum, the walls of the rectum divided by a bistoury, and the levator ani cut through, a speculum ani introduced, and the parts dilated so as possibly to extract the offending substance. I thus stated my opinion, and submitted myself,

as the youngest, to be guided by my elder associates. Dr. Whiting was of opinion it was best to extract it with several kinds of forceps, and accordingly the patient was placed again in a horizontal position. Another young man and myself dilated the parts, and the tumbler was brought down and seized with forceps by Drs. Kittredge and Whiting, and they made a powerful but vain effort to extract it. It was then agreed that Dr. Whiting, he having the smallest hand, should introduce it beyond the rectum, and make another effort to bring down the tumbler; he succeeded in passing his hand into the rectum, but was unable to bring the tumbler away, owing to its lodging in the folds of the intestines. The patient now expressed a desire to be opened through the walls of the abdomen: this, he was told, would produce certain death. It was then proposed to open through the levator ani, but Dr. Kittredge thought he might expire from hæmorrhage from the hæmorrhoidal arteries; and, as it was now growing dark, the operation was abandoned.

The patient lived about three days after this. His tongue sloughed and turned black, and gangrene took place in the large intestines. The tumbler was extracted after death; it measured three and a half inches in length, three and a half in width in the direction of the flattened part, two inches across the bottom, and would hold nearly three gills.

Hovey had been cut for the stone in the Massachusetts Hospital in 1828, by Dr. Warren. It seems that he introduced the nucleus of the stone himself through the urethra, which consisted of a small bean, when a boy; it rolled about in the bladder, and this collected the stone which was extracted by Dr. Warren *.

Boxford, Massachusetts,
April 18, 1834.

BARK OF THE XANTHORHŒA.

The following note, on the resinous substance contained in the bark of the xanthorhœa, was furnished to M. De Candolle by M. Vignet, of Geneva.

"The trunk of the tree sent to Professor De Candolle is remarkable, not only for the peculiar disposition of its fibres, but also for a resinous substance which impregnates the bark, and fills the numerous fissures which exist therein.

This substance is of a beautiful reddish-brown, is semi-transparent at the thin parts, has a brilliant fracture, is inodorous when cold, and has a slightly aromatic astringent taste. When heated it melts, and burns with much smoke and a slight odour of benzoin.

Water, the fixed oils, and the essential oil of turpentine, have no action upon it.

Mixed with lime, it acquires the property of becoming partially soluble in water, and a brilliant yellow precipitate is obtained by saturating the lime with hydro-chloric acid.

Concentrated nitric acid has a very energetic action upon this resin, and converts it partly into carbon, partly into a deep brown substance, soluble in water and analogous to artificial tannin; it, besides, disengages a little benzoic acid."

M. Vignet concludes that this substance is dragon's blood, in its purest state; which, he asserts, is less soluble in oil of turpentine, as its purity is greater.—*De Candolle's Organographie Végétale*, tom. i. pp. 123, 124.

LITERARY INTELLIGENCE.

Researches on Diseases of the Brain, &c. By John Abercrombie, M.D., F.R.S.E. New edition.

Cases of Tic Douloureux, and other Forms of Neuralgia. By John Scott, Esq.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *Aug.* 26, 1834.

Abscess	2	Hæmorrhage	1
Age and Debility	53	Heart, diseased	2
Apoplexy	10	Hernia	1
Asthma	21	Hooping-Cough	15
Cancer	4	Hydrophobia	2
Childbirth	8	Inflammation	42
Cholera	103	Bowels & Stomach	20
Consumption	71	Brain	3
Constipation of the Bowels	1	Lungs and Pleura	3
Convulsions	53	Insanity	2
Croup	2	Liver, diseased	19
Dentition or Teething	8	Measles	9
Diabetes	1	Mortification	6
Diarrhœa	1	Paralysis	2
Dropsy	12	Small-Pox	1
Dropsy on the Brain	21	Sore Throat and Quinsey	1
Erysipelas	2	Spasms	4
Fever	10	Thrush	2
Fever, Scarlet	6	Unknown Causes	4
Fever, Typhus	4		
Gout	4	Stillborn	13

Increase of Burials, as compared with the preceding week . . . 103

NOTICE.

Mr. Shepherd is very wroth at Mr. Cyrus Fay's claim to the invention of a certain *stump*-forceps; but till Mr. S. is more composed, we must decline publishing what he says. Does Mr. Glasford Shepherd think that we would be induced by the peremptoriness of his request to give "immediate insertion" to his unmannerly letter?

* Boston Med. and Surg. Journal.

W. WILSON, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, SEPTEMBER 6, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XLVIII.

COMPLEX LABOURS—HÆMORRHAGE.

EFFECTS OF THE LOSS OF BLOOD.

There are many distressing symptoms consequent on the loss of blood, independently of fainting. Some of these appear soon, and are comparatively evanescent; others do not occur for some time, being more remote, but more permanent. Of the latter kind are cachexia, wastings, purgings, dyspepsia, dropsies,—especially an œdematous state of the legs and feet. Such affections are to be attributed to the balance of the circulation being destroyed, by the sudden abstraction of so much blood from the circulating system.

The loss of blood will sometimes excite dormant morbid actions, which may terminate in organic disease, where there is a predisposition to its formation. I have seen two cases in which phthisis itself seemed to be called forth, in its direst form, as a consequence of violent hæmorrhage, no symptoms of diseased lungs having previously existed.

The loss of tone in the system generally, is best relieved by sending the patient into the country, to the sea side, or some chalybeate spring, enjoining regular hours, regular exercise, regular nourishing diet, with gentle stimuli and tonic medicines,—provided there be no local affections, or any contra-indicating symptoms. Œdema of the legs will most likely disappear spontaneously; if an accumulation of water take place in the abdominal cavity, diuretics may cause its removal, or an operation may be required. The purgings produced by an irritable state of the mucous membrane, so frequent as a consequence of a copious loss of blood, may be relieved by chalk, opium, rice, and nourishing diet. The aphthous state of the mouth accompanying the purgings will also often yield to the same treatment. Both these symptoms, however, are most likely to be alleviated by sending the woman into a purer air. It is commonly observed by practitioners in crowded cities, that the diarrhœa has subsided immediately on removal, although it had not given way in the least degree to the exhibition of the most approved medicines.

Re-action after flooding.—The symptoms which appear immediately after flooding, when the first effects of fainting are gone off, are those of re-action and nervous irritability in an extreme degree. When the system is deprived of a large quantity of blood, the circulation is carried on in a much more rapid manner, that the increased velocity may compensate for the diminution in quantity; and in proportion as the quantity is diminished, will the velocity be increased.

This re-action is attended with fever; quick, small, sharp, jerking, and wiry pulse; increased heat and dryness of skin; shrivelled features; dryness of the mouth, and a parched and pinched state of the lips; a diminution in all the secretions; desire for fluid, and dislike to solid food; intolerance of light and sound; inability to sleep, and most distressing pain in the head. Palpitations are often present; so also are panting, dyspnœa, and a degree of hurry and alarm on waking, or being suddenly disturbed.

The pain in the head is almost universal after hæmorrhage, and is very characteristic. It is described as being similar to the thumping of a small hammer with-

in the skull, or the ticking of a clock; sometimes, but more rarely, it resembles the roaring of the sea, or singing of a kettle. Every movement of the head becomes distressing; and if raised from the pillow, a sense of fainting supervenes. I have little doubt that this sensation of thumping arises from the column of blood being lessened in diameter, and the arteries not being sufficiently distended by their contents. As these vessels are highly elastic, their calibre contracts in proportion to the decreased quantity of blood that they contain. When they are fully filled, and their coats are duly distended, they propel the blood onward with but slight effort; but when partially emptied, so that the natural and healthy agreement between their capacities and the measure of their contents is disturbed, they are compelled to beat violently, in order to carry on the circulation; and this forcible contraction propagates an increased jerk to the fluid.

Most probably this state pervades the whole body; but it is only perceived in the brain, in consequence of its structure and peculiar sensibility. This beating pain is so very general, that we may often at once commend ourselves to the confidence of our patient, by the pertinence of our questions regarding it. There is nothing in the whole range of the practice of medicine which attracts the attention of a patient so much, or so forcibly convinces him that his attendant understands the nature of the disease under which he labours, as an accurate description of the painful sensations he is suffering. For our own sake, then, as well as his, it is desirable in our general conduct at the side of a sick bed, that we should habituate ourselves not to hurl a number of random questions at the patient, but only to put such as appear pertinent to the case, and are likely at once to strike his notice. The confidence of the sick, indeed, is a heavy weapon in the hands of his physician; it is sometimes more serviceable than the whole combined armament of the Materia Medica. These observations apply particularly to the case under consideration. If you see a woman blanched, with her skin of a waxen paleness; if you find a jerking, bounding, hæmorrhagic pulse, and learn that a few days before she has suffered a large loss of blood, you may be almost assured that this peculiar pain in the head is present. The simple inquiry, then, addressed to her, "Whether she is not suffering under a violent beating, like that of a small hammer, within the skull?" will often of itself be sufficient to inspire her with confidence.

Treatment.—If we bear in mind that the cause of the disagreeable symptoms is referable to the decreased quantity of blood circulating, and its augmented velocity, dependent on the diminution in its quantity, we shall never be much at a loss in directing our treatment. Our object is to diminish the present irritability—to alleviate the febrile symptoms—to remove the distressing headache—to preserve as much as possible the remaining power—and gradually to add to the mass of circulating fluid.

With the latter view, nutritious diet must be frequently exhibited: sago, arrow-root, milk, jelly, and strong broths, in as large a quantity as the stomach will digest. To diminish the irritability of the system, and moderate the excessive action, saline medicines may be given, either in effervescence or not; cold sponging may be employed to the hands, arms, and face; and opium may be administered in tolerably large doses. Both the mental and corporeal irritability produced by a loss of blood, is better alleviated by opium than any other remedy. It is a maxim, however, in medicine, not to give opium when the head is much affected; and therefore I have not often exhibited it alone when this peculiar headache has been present; yet I have found relief from Dover's powder; and I think this combination the best form in which it can be used. I have not found any thing quickly remove the headache; but I believe, as fresh blood is formed by the assimilation of nourishment, all the symptoms will, in general, gradually subside. I cannot help thinking, indeed, that if no medical treatment were employed—if nothing were exhibited but mild nourishment in small quantities, at regular intervals, the symptoms would of themselves disappear; and I am persuaded, that in no few instances medical interference has been of decided injury.

I must caution you against using any means which are likely to diminish the quantity of blood circulating in the system; and therefore all powerful evacuant remedies must be avoided, particularly bleeding. Generally nothing can be more injurious than the bold use of the lancet under this state of re-action, consequent on the loss of blood; and no treatment can be more unphilosophical; and yet I have known venesection repeatedly resorted to. Taking blood, indeed, will for a time almost always relieve the distress; but the alleviation is merely temporary, it only remains as long as the patient continues faint; re-action soon occurs in a still greater degree than before, and an aggravation of the symptoms is the consequence. I cannot believe the pain arises from undue determination of blood to the brain; but am persuaded it is produced rather by

the vessels being too empty. For this reason, also, I should avoid leeching and blistering the head, or its neighbourhood. If, indeed, instead of a pale exsanguined countenance, which is almost invariably present, there should be a turgid and suffused face, indicating an extraordinary fulness of the veins of the skin, I should presume the same state to have taken place within the brain, and should then apply leeches pretty freely, or use other means to relieve the surcharged vessels. The same reasons would induce me to reject the plan of violent purging. My father, indeed, says, that nothing relieves this pain so soon as copious evacuations from the bowels. I think I have seen two or three instances where this practice appeared to aggravate the symptoms. It is certainly proper to take care that the lower part of the intestinal canal should thoroughly empty itself daily, but not to keep up by medicine such a constant irritation on the mucous membrane as will produce a very much increased secretion, and a copious drain from the system generally. A slight purgative may be given every day, more with the view of stimulating the sluggish action of the intestines, than of procuring a number of watery stools.

Cold applications to the head, and especially ice, will often relieve the pain in a great degree during the time they are being used; but it returns when they cease to be applied. I have not the slightest objection to the application of cold; it must be useful if it bring freedom from pain; and it can produce no injurious effects subsequently, as powerful evacuants may do. We are much indebted to Dr. Marshall Hall for his treatise on the Effects of Loss of Blood, and for his means of diagnosis between the affections of the different viscera, particularly the heart and brain, produced by excessive depletion, and somewhat similar symptoms the result of inflammatory action.

You will be most successful, then, by avoiding powerful evacuants, yet regulating the bowels daily, by the exhibition of saline medicines and Dover's powder, or small doses of ammonia with hyoscyamus, by the application of cold to the head, by sponging the face, arms, and hands, with vinegar or cold water,—provided there be increased heat of skin,—by admitting fresh air, enjoining perfect quiet, soothing irritability, soliciting sleep, and by a frequent and liberal supply of mild, fluid, and nutritive food.

CONVULSIONS.

An attack of puerperal convulsions is one of the most frightful accidents that can happen to a patient under labour.

A convulsive paroxysm during labour may occur under two extreme states of system diametrically opposed to each other; the one in which the cerebral vessels are inordinately distended with blood; and the other, when they have been drained almost empty, as in the case of excessive hæmorrhage: and it is a curious fact, that the two perfectly opposite states, viz. too great a fulness of the vessels, and too great emptiness, will produce exactly the same phenomena.

It is almost universally observed, that when an animal is bled to death, the last act of life is a most violent convulsion of the voluntary muscles. Andral says, "in an attack of convulsions, the brain is equally affected by an over-abundant, or too sparing a flow of blood to the head;" and my friend Dr. Abercrombie, if I remember rightly, in his valuable work on the Diseases of the Brain, has made the same remark. Some pathologists, indeed, have denied the probability of "any state of things materially augmenting or diminishing the actual amount of fluids within the cranium," in consequence of the brain being perfectly inclosed within a bony case, and removed from the influence of atmospheric pressure. I am inclined to think, that when hæmorrhage to any extent takes place, the cerebral vessels participate, in no small degree, in the general inanition of the circulatory system.

I have already mentioned, that the occurrence of a convulsive seizure, in a patient who has suffered violent hæmorrhage, is to be regarded as a highly dangerous, and frequently a mortal symptom. Such cases, however, are not to be looked upon as true puerperal convulsions: this term should be restricted to the disease it becomes my duty next to describe.

Period at which they occur.—The true puerperal convulsions may occur at any period of the latter half of pregnancy, or in any stage of labour; they not unusually first make their attack even some hours after the child is born, and the placenta expelled; when the process is popularly considered as completed. We generally meet with them, however, during the last week of utero-gestation, or the first stage of labour, previously to the entire dilatation of the os uteri. At other times, but more rarely, they occur when the head is pressing on the outlet of the pelvis, and distending the perineum—when the uterus has been acting excessively strongly, and the labour is somewhat lingering. The great bodily exertion consequent on labour may, in such a case, have much influence in their production. Occasionally, indeed, convulsions appear early in pregnancy; and Perfect gives us two cases in which they attacked the patient before quickening.

Universal liability to them.—These convulsions may assail women of all ages, and of all kinds of constitution; women with

their first child, as well as those who have borne many; but they most frequently accompany first labours;—and the kind of patient most obnoxious to their attack is the stout, florid, robust woman, of strong muscular fibre, with a thick set form, and short thick neck—just such a person as you would consider predisposed to apoplexy. But the most delicate and slim female is by no means exempt from the danger of a convulsive seizure. I have sometimes thought that a naturally excessive sensitiveness of the nervous system predisposed to the disease; and I have traced an attack, more than once, to originate from mental emotion, particularly grief, despondency, or other depressing passions.

Nosology.—Cullen arranges the disease—convulsio, in the class *neuroses*, order *spasmi*; and defines it "*musculorum contractio clonica, abnormis, citra soporem.*" He makes no species of puerperal convulsions. His *citra soporem* cannot apply to the variety under consideration, because we shall presently see that there is a state of sopor generally present between the fits as complete as that observed in apoplexy. Sauvages has placed them in his genus *Eclampsia*, and defined them *Artuum vel musculorum plurimorum spasmus clonicus acutus, cum sensuum obscuratione.* Sauvages' definition embodies its chief features.

Analogy to apoplexy and epilepsy.—Puerperal convulsions are more allied to apoplexy than any other disease of the body, inasmuch as they usually depend upon exactly the same causes. There is also an analogy in respect to the stertor and stupor, which form prominent features in both diseases. They would seem also to bear some resemblance to epilepsy from the violent spasmodic paroxysms which constitute the most striking symptom of puerperal convulsions. The disease is unlike apoplexy, however, because in common apoplexy we seldom have the convulsive fits, and seldom or never is permanent paralysis produced as a consequence of puerperal convulsions.

Again, it is seldom that in epilepsy the convulsive paroxysms follow each other so rapidly as we observe under an attack of puerperal convulsions; but they have a tendency to return at different periods throughout the whole or a considerable portion of life. This latter observation, however, does not apply to the disease we are now considering. Upon the whole, then, it bears much more resemblance to apoplexy than to epilepsy, because it is produced by exactly similar causes, (those causes, indeed, acting on the system under peculiar circumstances), and is relieved by exactly the same means.

I look upon a case of puerperal convulsions to be, in fact, one of apoplexy, only that we have superadded to the common apoplectic phenomena violent spasmodic contractions; and this symptom is consequent upon the irritable and excitable state of the nervous system always in a greater or less degree accompanying pregnancy and parturition. We can readily imagine that in a highly excitable state of nervous system, any irritation which the brain might suffer would be more likely to produce convulsive action of the muscles than the coma attendant on apoplexy; and this opinion is borne out by our observations on infants.

In speaking of the peculiarities of the fœtus, I have already shown you that under infancy the nervous system bears a very large proportion to the general bulk of the body; and we may presume that its influence on the body generally is in proportion to its development. Now, we see no such disease as the apoplexy of adult age in an infant; but when the brain is irritated by pressure or other causes, we have convulsive paroxysms excited. I consider, then, the case of puerperal convulsions to be exactly analogous to that of infantile convulsions, and that they are both of them analogous to apoplexy; the causes, however, acting upon the system under a highly excitable state. This view of the case, whether correct or not, is practically valuable, and will lead to the most judicious treatment.

It was the want of tracing this analogy between the two cases that led to the destructive practice until late years so universally adopted.

Atmospheric influence.—I have remarked that puerperal convulsions are much more frequent in hot weather than in cold, and especially at times when the atmosphere is charged with electricity; and under such a condition of the air I have been called to two or three cases in quick succession. Thus they are oftener met with towards the end of summer and in the autumn, or in the spring, when a few unusually warm days have suddenly burst upon us.

My father first called my attention to the fact of convulsions being more frequent when (to use a common expression) there was "thunder in the air," than at any other time; and he has pointedly mentioned such a conviction in the second part of his work. Andral has recently stated his opinion to the same effect, when speaking of convulsions in general. "The electrical state of the air on the approach of a storm has often served to bring on a convulsive fit." And Denman remarks,—"It has been justly observed, that women are far more liable to puerperal convulsions in certain years and seasons than in others;" and he then proceeds to enumerate, among other causes, "the particular influence of the air." Nor did this circum-

stance escape the acute Smellie; for he states, that in the course of the year 1747 he attended several patients who were attacked near their full time with convulsions; that other practitioners also saw similar cases during the same time, "so that they seem to have proceeded from the constitution of the weather."

Whether this liability, indeed, merely arises from women not being able to bear the fatigue of labour as well in hot weather as in cold, or from the blood being then more rarified—or whether it be that such kind of weather exerts some specific influence over the system, particularly of puerperal women, which predisposes to these convulsive attacks,—I cannot pretend to determine; but I suspect that the peculiar effect is principally to be attributed to the atmosphere being highly charged with electric fluid.

Proximate cause.—The most usual proximate cause of puerperal convulsions is without doubt pressure on the brain; this pressure being sometimes produced by a rupture of the vessels, causing a sudden effusion of blood; sometimes by serous exudation into the ventricles, or between the membranes; sometimes, and by far the most frequently, by simple congestion of the cerebral vessels themselves. But the disease has sometimes proved fatal without any organic lesion being evident on dissection, and without even the vessels being observed to be preternaturally full. In this respect, also, there appears a strong analogy between apoplexy and puerperal convulsions; for Zuliani, of Brescia, in 1780; Kortum, of Dortmund, in 1785; and, more recently, Abernethy, among us, have recognized a species of apoplexy, to which the term *nervous* has been given. In two cases, also, related by Abercrombie, of his *simple apoplexy*, no anormal appearance was observed after death.

Remote causes.— Into the remote causes it is not my wish to enter at any length, because the subject is at best but unsatisfactory, and little understood. They have been ascribed to articles of food remaining undigested on the stomach, or irritation existing in some other part of the alimentary tube;—to general irritability of constitution;—to a delicate and luxurious mode of living;—to the depressing passions;—to an overloaded state of the system;—to overdistention of the uterus; and to the death of the child. But the affection, in my opinion, originates most frequently in some deranged state of the uterus itself, probably in its nervous system, and consists in some irritation propagated from that organ to the brain. I have met with three or four cases which have strongly impressed me with this idea, and one, the most striking, I will relate. I was called, some years ago, by one of the midwives of the Royal Maternity Charity, to the assistance of a woman under puerperal convulsions. When I arrived I found she had been bled largely by a medical friend living in the neighbourhood, who had been sent for on the instant of the attack. The bleeding had relieved her partially, but it was thought right to repeat it. A third quantity of blood was taken some time after, with such a beneficial effect that the convulsions entirely ceased, and, after a few hours, perfect consciousness gradually returned. About fifty hours after the attack, active labour came on; and in less than five hours more the child was born, dead. The placenta did not descend, and in two hours after the expulsion of the child I was summoned. I found her perfectly conscious, in good spirits, and she made no complaint. There had been no hæmorrhage, the uterus was not strongly contracted, and the placenta entirely within it. Under no greater anxiety than usual, when the placenta is retained, I proceeded in the ordinary way to remove it. The moment I had passed my hand completely into the uterine cavity the patient turned upon her abdomen, and, without uttering any expression of pain, went into a convulsion, though not of a violent kind; intense coma supervened, which yielded to no treatment I could devise, and terminated fatally in in about two hours from the removal of the placenta. The vagina, and especially the inner surface of the uterus, communicated to the hand a more pungent sense of heat than I ever recollect to have experienced on any other occasion.

About forty-eight hours after her death I made an accurate inspection of the body. The dura mater adhered more firmly than usual to the inner surface of the cranium, and was healthy in appearance; the vessels of the brain contained less blood than ordinary; the plexus choroides were quite blanched; there was no fluid in the lateral ventricles, none between the membranes, but about two drachms at the base of the brain; no extravasation of blood in any part of the cerebral mass. The viscera were all healthy; the uterus was contracted; nor did it present any uncommon appearance.

Here was as clear a case as can possibly be made out of irritation propagated immediately from the uterus to the brain; and I have no question in my own mind that if the placenta had not unfortunately been adherent, but thrown off naturally, the woman would have recovered perfectly.

A case, which I will read, almost analogous, is related by Ingleby; the woman, however, not having suffered any convulsion before delivery. "A highly-esteemed

friend of mine once found it necessary to pass his hand into the uterus for the purpose of removing an adherent placenta, the ergot of rye having been previously administered. The introduction was carefully performed. The straining and opposition to his efforts, on the part of the woman, was exceedingly great; and at the moment when the operator's hand had reached the organ, my own hand making counter-pressure on the abdomen, the patient became violently convulsed, and died in less than a minute."

The cause of this convulsion could not have been excessive loss of blood, because Ingleby would doubtless have mentioned that fact, if it had been so; besides, if the woman had been faint from hæmorrhage, she could not have so strongly resisted the efforts made to introduce the hand. I look upon this case as one also proving that the remote cause of this kind of convulsion often exists in the uterus, and that the irritation is propagated through the agency of the nervous system to the brain.

Symptoms.—The symptoms of puerperal convulsions are so prominent and strong, that if once the disease has been seen there is no likelihood of its being mistaken. If they occur, as is usually the case, during the first stage of labour, the patient is probably sitting or walking, may be even occasionally joining in conversation; and, without giving any previous warning, suddenly falls down in a strong fit. All the voluntary muscles of the body are thrown into a state of violent spasms, alternating with relaxation, so as to produce rapid and powerful contortions and struggles; the arms and legs are thrown about in all directions; and the strength of two or three assistants is required to restrain the patient. The face becomes turgid and livid, swollen by the increased quantity of blood with which the vessels are loaded. The throat also seems to swell, the carotids beat inordinately, and the jugular veins appear prominent. The countenance assumes a most hideous expression, partly from the suffused state of the features, and partly from their distortion and convulsive action. The eyes seem starting from their sockets; and, in consequence of the spasmodic action of their muscles, are drawn obliquely upwards, one to the inner and the other to the outer canthus; so that none of the pupil, and but a small portion of the cornea, can be seen. The eye-lids are half open, and violently agitated; the pupils themselves (if the eyes can be so opened as to obtain a sight of them) are generally dilated; sometimes, however, more than usually contracted. The lips partake of the general convulsion. The mouth is drawn to one or other side, and twitched spasmodically. The temporal and masseter muscles act with amazing strength, firmly clenching the teeth together. The tongue is almost invariably protruded beyond the gums; and the muscles of the jaw acting powerfully at the same time, catch it between the teeth, and lacerate it dreadfully. A quantity of frothy saliva escapes from the mouth, generally tinged with blood, which issues from the wounded tongue, adding very much to the hideousness of the aspect. The breathing is hurried, irregular, and laboured, and performed with a sharp hissing noise, from the air being impeded in its passage, partly by the clenched teeth, and partly by the saliva which hangs about the lips. The pulse, during the paroxysms, varies, being full, slow, and oppressed, at the commencement and before the attack, and increasing in velocity as the intensity of the fit becomes greater. As these frightful and alarming symptoms occur so suddenly, it is not surprising that they should strike the attendants with terror and dismay. So general and powerful is the alarm, that every one, in the distress of the moment, is running in search of they know not what; and the medical attendant, deprived of the assistance of the bystanders, is often compelled to collect for himself whatever he may require.

After an uncertain time, the violence of the fit abates; and probably in a few minutes the convulsion has quite disappeared. The patient then will, perhaps, slowly recover her consciousness. She appears as if she were awaking from sleep,—is perfectly unaware that any thing dangerous or extraordinary has happened,—and has no recollection whatever of the interval. She will most likely complain now of a violent pain in the head. This truce will quiet the attendants, and restore something like tranquillity in the lying-in room. Short-lived, however, are the favourable expectations: another attack will presently dissipate their hopes, and again all are thrown into confusion.

At other times, and more frequently, although the more violent symptoms of the attack have subsided, the patient remains comatose, without sensation or motion, lying in the senseless state of apoplectic stupor; the breathing heavy, dull, and stertorous. At others, again, a certain degree of consciousness returns, a knowledge of persons and objects, but an inability to articulate or make the wishes known; and often with a partial return of consciousness, there is a constant rolling about the bed, and deep and distressing moaning. During the continuance of the fits, uterine action is not suspended, although no signs of pain are manifested by the woman if she remain comatose. Gene-

rally, with each return of uterine action, a fresh paroxysm occurs; so that we may sometimes count the frequency and duration of the pains by the number and length of the fits. Occasionally, under convulsions, dilatation and expulsion have gone on so rapidly, that the child has been propelled into the world before the attendants were aware that labour had begun; and some instances have come under my own eye of a child being expelled during a strong fit. Baudeloeque states, that he has seen some cases in which he found the child between the woman's thighs, though "an instant before he could discover no disposition for delivery." Thus convulsions neither suspend nor interfere with efficient uterine action.

It generally happens, but this is by no means an universal remark, that the infant is born dead under convulsions, especially if the attack has occurred early in the labour, and continued for any length of time. It is difficult to account for this circumstance: pressure on the child's body or the funis umbilicalis, alone, cannot explain it. I suspect it is owing to the necessary changes in the fœtal blood not being effected during its circulation through the placenta. Denman justly remarks, that the death of the child is rather to be considered as a consequence than as a cause of the convulsions; and Spence gives a case in which the mother having died of convulsions before there was any disposition to labour, the Cæsarean section was performed immediately after: the child was extracted alive, was itself soon seized with convulsive paroxysms, and died in less than an hour.

Premonitory symptoms.—Convulsions often arise suddenly, as just described, without any premonitory symptoms: sometimes, however, and I think even more frequently, there are signs which appear a few days previously to the convulsions showing themselves; and at other times there are some which immediately anticipate the fit itself. Thus a woman will perhaps appear perfectly well, bearing the commencing pains of labour with great fortitude, and in good spirits, when she will suddenly declare that there is a bright light in the room, and a convulsive paroxysm immediately succeeds. One of the most dangerous cases of convulsions that came under my father's notice was ushered in with the remark that the room was studded with diamonds. After such a declaration, then, we might expect an attack of this frightful disease; but little time would be granted us for acting in prevention, since the more violent symptoms would almost immediately follow.

But there are others which appear a few days or hours before the fit, leading us to suspect that convulsions are likely to occur; but yet not so strongly marked as to warrant us in saying that the patient must necessarily experience an attack. These are such as we are in the habit of referring to an overloaded state of the brain; violent headache; a feeling as if a blow was inflicted on the head; giddiness; a sensation of intoxication, and inability to walk straight; drowsiness; singing in the ears, and deafness; total or partial loss of sight; scintillæ, or *muscæ volitantes*, floating before the eyes in rapid succession; impeded utterance; numbness or cramps in the arms, and occasionally violent cramps in the stomach. Such symptoms, especially in full habits, should never be neglected: we may infer that they arise from a fulness of the vessels of the brain, and in most instances may deplete the patient both by bleeding and purging.

Prognosis.—Our prognosis must be most guarded in all cases of puerperal convulsions; because there is always great danger, and the danger is in proportion to the length and strength of the fit; the shortness of the interval; but more especially to the degree of consciousness between the paroxysms. If the patient lies in a state of complete stupor, accompanied with stertorous breathing between the paroxysms, and insensible to any stimulus that could be applied, even if the fits were of short duration, I should consider her in greater danger than if the convulsions were stronger, with a state of perfect consciousness in the interval of the attacks. Usually, the stronger the fits the deeper is the accompanying coma; but that is not always the case; and I would rather form my prognosis by the intervening state than by the actual violence of the fits themselves. We may comfort ourselves with the assurance, however, that although convulsions are so dangerous, and although our prognosis must be in the highest degree guarded, under our present improved treatment, the danger is not at all in proportion to the frightfulness of the patient's appearance and the excessive alarm occasioned. The terror created in the minds of the friends is often so great that they at once give up the case as hopeless, and conceive the patient must be dying. In this respect, convulsions and hæmorrhage are strongly contrasted with each other; in the latter case, it not unfrequently happens that the fatal event is stealing on insensibly; the anxious friends, who are watching by the bed-side, not being aware of the impending danger until the case is almost beyond the reach of art. If we could be certain, indeed, that no permanent injury had been inflicted on the contents of the cranium, I think we might with much confidence hope for a favourable issue.

AQUEOUS DISCHARGE AFTER PARTURITION.

To the Editor of the Medical Gazette.

Sir,

If you think the following cases, and the remarks accompanying them, deserve a place in your valuable and widely-circulating journal, they are entirely at your service.

I am, sir,
Your obedient servant,
Samuel Ashwell, M.D.,
Lecturer on Midwifery and Physician Accoucheur to Guy's Hospital.

Some years ago, I met with aqueous discharge from the uterus very soon after parturition, and I carefully noted the circumstances. Several other cases have since that time occurred in my practice, attended by similar symptoms. One of my pupils at Guy's Hospital, Mr. Cotton, presented to me the notes of the third; and my friend Mr. Burn, of Earl-Street, Blackfriars, favoured me with the particulars of the fourth case.

I am desirous to communicate these cases to the profession for two reasons; first, because they are important, and attended with danger; and, secondly, because I can find not even an allusion, much less a history, of a similar disease of function in any obstetric author I have consulted.

Discharges of water from the uterus are not unfrequent during gestation, and they are occasionally occurring in the unimpregnated condition of the organ. I am, however, disposed to think that the passing away of pints of watery fluid, soon after labour, by gush, in the first instance, and afterwards by draining, is a rare occurrence. Had it been otherwise, it would certainly have been noticed in some of the valuable works we possess, and it would have become a topic of discussion in medical society.

In the subjoined cases it will be seen that the labours were natural, although somewhat protracted and severe. The patients were delicate women, but free from serious illness: indeed, there was nothing in the labour, or in the condition of the system immediately preceding it, with which the discharge could have been satisfactorily associated, as its cause. The principal dangers connected with this morbid secretion appear to be of the inflammatory kind. The weak and rapid pulse, the tenderness and enlargement of the uterus, and the almost entire suppression of the milk and the lochia, plainly point to puerperal mischief, too likely, when the danger is not averted, to terminate unfavourably. It is, perhaps, singular that the mucous lining of the uterus should secrete a serous or aqueous discharge, especially when such discharge of water is not dependent on the membranes inclosing the fœtus; but we know that the mucous lining of the nostril occasionally pours forth large quantities of aqueous fluid; and there seems no reason why, under a like derangement of function, the lining membrane of the uterus may not do the same.

That this affection decidedly interferes with the consecutive series of events following parturition, is evident, from the suppression of the lochia and milk, as well as from the scanty secretion of urine: it may, therefore, be placed in the class pyrexia.

I cannot state with certainty what might have been the effect of vigorous antiphlogistic treatment; because, in the cases related, a modified plan was pursued. As, however, the affection may be regarded as catarrh of the uterus, attended by inflammation, cases will probably occur in which depletion to a greater extent will be required. There can be no doubt that the lining membrane of the uterus furnishes the discharge. Under natural and healthy puerperal circumstances, the same membrane, then possessing considerable extent of surface, pours forth the lochia. This secretion, we know, is at first sanguineous, subsequently it becomes paler, but is still mucous; nor is it till many days have elapsed that it assumes a leucorrhœal or serous character. The derangement of function, which is productive of this "aqueous discharge," instead of the lochia, is the disease now described.

Case I.—Mrs. G—— was confined on Saturday, November 6, 1830, of her fifth child. The labour was severe, but perfectly natural.

Sunday, November 7th.—Complains of a good deal of pain in the hypogastric region; the uterus is large and tender; pulse 120, but neither full nor strong. Urine scantily secreted; scarcely

any lochial discharge; and the after-pains not at all severe.

Ordered Castor Oil, bran fomentations to the belly, and six grains of Dover's powder, in common saline mixture, every six hours.

Monday, 8th, 5 P.M.—I was hastily sent for. On my arrival I found the nurse, as well as the patient, much alarmed; there having occurred suddenly a very copious gush of transparent, colourless, inodorous discharge from the uterus: at least three pints had escaped. The bed was wetted entirely through, and a pool had formed about the lower part of the person. The fluid had scarcely any taste, and closely resembled water. She was exhausted; pulse quick, 130; and she was altogether hurried and alarmed. There was neither tension nor fulness of the mammæ, indicating the secretion of milk; the uterus was not so large as on the previous day, but very tender to the touch. There has been no lochial secretion, and only a scanty discharge of high-coloured urine. She was made comfortable by the removal of the wetted linen. A draught composed of *thirty* minims of aromatic æther and the aromatic spirit of ammonia was immediately given, and she was allowed some bland nourishment. The other remedies were ordered to be continued as before.

Tuesday afternoon, the 9th.—I have seen her several times since last evening. The "aqueous discharge" still continues, *twenty* napkins having been used: it is not all streaked with blood, nor is there any appearance of lochial or mucous discharge. After sleep, the water comes away in slight gushes, but by draining at other times. The urine is still scanty and high-coloured; pulse 120, small and compressible; the breasts are quite flaccid—the child, therefore, is fed. Abdominal tenderness less; uterus better contracted, still reaching half way between the umbilicus and pubes.

Continue the remedies.

Wednesday.—Bowels have been naturally relieved. Urine still sparing in quantity and high in colour; pain of the abdomen less, and uterus not so tender to the touch. Pulse 100; debility excessive; little or no secretion of milk; "aqueous discharge" still very abundant; no lochia.

After this period, no distressing symptoms occurred, if the languor and extreme debility be excepted. The discharge of water continued for twelve days; for the last six it was evidently on the decline. The milk was never naturally nor healthily secreted; the child, in consequence, being brought up by hand.

CASE II.—Mrs. T——, æt. 28, a woman of spare and delicate habit, and the mother of five children, was confined December 8, 1832, of a fine healthy boy, after a natural and quick labour. I visited her some hours after, and I found her suffering severely from after-pains. Pulse 120, sharp, yet compressible. Bladder rather distended, and uterus large.

Ordered half a grain of Opium, and 3 grs. of Calomel; directing a table-spoonful of Castor Oil early on the following morning.

December 9th.—I was requested to see her in the afternoon, in consequence of a discharge of water, which had exhausted and alarmed her. I found that she had lost between two and three pints of limpid inodorous fluid, and it was still draining away. The uterus had descended behind the pubes; it was not large, but very tender. Pulse 130, small and feeble. There had been no lochial discharge, and she had passed once a large quantity of urine.

Ordered bran fomentations, Dover's powder, grs. vj. every six hours; and enjoined perfect quietude.

This patient went on just as in Case I., only that there was some secretion of milk, and the child was partly nursed. She was much debilitated, and the discharge did not cease for ten days.

CASE III.—Mrs. Penkin, æt. 40, the wife of a labourer, has had several children; her labours good, with the exception of a temporary state of melancholia. She is of sallow complexion, and has ailed greatly during the present gestation. For the last three days she has been suffering; and owing to the increase of her pains, the presence of Mr. Cotton was requested. Her labour was natural, although severe; and there having been copious losses of blood in her preceding deliveries, Mr. Cotton bandaged the abdomen, and exhibited 3ss. of the ergot, previously to the expulsion of the placenta. Things went on com-

fortably until the fourth day, when Mr. Cotton was sent for. He found his patient had passed a bad night, and complained of severe pain in the hypogastric region, which was accompanied with the greatest pain on pressure; her pulse was small, 115; urine scanty and high-coloured; and there had been a discharge of transparent watery fluid, sufficient to soak from twelve to twenty napkins. Bowels confined.

Leeches and fomentations were ordered to the abdomen; calomel and opium to be given, with an ounce of castor oil, the following morning, if the bowels remained unacted upon.

5th day.—Very low, and in great distress of mind, saying she should never recover. Her abdomen was tympanitic, and tender to the touch; countenance pale, and bowels still unmoved; her pulse 120, small and weak; the discharge the same in quantity. At the suggestion of a relative, for whom Mr. Cotton attended, he introduced the catheter, and drew off eight ounces of high-coloured urine; but this effected no mitigation of the symptoms.

Cataplasms were ordered to the abdomen, and an enema with Ol. Terebinthinæ ℥jss. exhibited.

This had the effect of opening the bowels, which greatly relieved the tender and painful condition of the abdomen. The pulse also became fuller, 108.

Pulv. Ipecac. Comp. gr. x. were ordered at bed time.

On the sixth day she was much improved; her pulse soft, 100; the discharge was also lessened, soaking but ten napkins in the twenty-four hours. Light farinaceous nutriment was allowed, and saline medicine exhibited. On the eighth day, gradually getting better; eats with appetite, and the discharge has diminished, although she still uses from five to ten napkins a day; it continues limpid, and devoid of odour. Owing to Mr. Cotton leaving the country, he resigned the charge of the case; but he learned that the discharge became by degrees lessened, till at length what was left was little in quantity, and very thick in substance.

It is worthy of observation in this case, that the lochia were suppressed after the second day; and there was never any secretion of milk.

Case IV.—Mrs. T., aged 32, of a spare habit, and delicate state of health, was delivered in the forenoon of August 2d, of her first child, a fine healthy living girl, after a severe but natural labour of thirty hours' duration. The placenta was expelled by the uterine action in about half an hour. The first two days the lochial discharge was very profuse, and rather offensive, with considerable disturbance of the system; the pulse from 100 to 120; the countenance pallid, and the lips almost exsanguineous. There were profuse perspirations; the abdomen was soft; and there was no pain produced by pressure; the secretion of urine was scanty, but passed without difficulty.

On the evening of the fourth day she complained of considerable enlargement of the abdomen, but without pain or tenderness; and on the following morning the nurse was hastily called to the bed-side, the patient supposing sudden hæmorrhage had taken place; but on examination, it was found a large flow of colourless fluid had been discharged, to the amount of about a quart. This continued for eight days, in a quantity sufficient to saturate a dozen napkins in the twenty-four hours. The discharge does not dribble away, but escapes in a sudden flow, preceded by a sensation of itching, and a slight bearing down.

9th day.—The discharge is evidently abating, as not more than three napkins have been wetted with it.

Mr. Burn remarks in his letter to me, dated fourteen days after the labour, "that his patient is still greatly debilitated, and has never experienced any secretion of milk. The lochial discharge still continues in small quantity."

13, Devonshire-Square, City,
August 20, 1834.

THE CONNEXION BETWEEN THE UTERUS AND THE PLACENTA EXAMINED.

To the Editor of the Medical Gazette.

Sir,

Having, within the last few days, succeeded in obtaining permission to examine the body of a woman who died at the full period of utero-gestation, I beg to transmit the following account of the dissection, as bearing upon the controversy relative to the vascular connexion between the uterus and the pla-

centa, which has of late been revived with so much interest.

Mrs. Shawe, aged 32, of —— Street, in this town, of delicate habit, was in momentary anticipation of her confinement, when, on the 19th instant, she was attacked by a sudden and severe invasion of phrenitis, which terminated fatally on the evening of the following day. On the 21st, the body was examined by Mr. Anderson, surgeon, of this town, and myself. Having exposed the cavity of the uterus, by a longitudinal section of its anterior portion, we separated the fœtus, and then directed our especial attention to the junction of the uterus and placenta. For the purpose of more particular examination, we removed the upper portion of the uterus from the abdomen; and having washed its inner surface, we proceeded to effect the separation of the placenta. On taking hold of the sheet left by the destruction of the ovum, and tracing it to the placental margin, the splitting of the membranes (almost united at the full period) was beautifully observed; the amnion and chorion passing over the fœtal aspect, and forming the sheath of the umbilical vessels, and the decidua evidently separating from them at the margin. On commencing with the separation of the placenta, the decidua, at the margin and for a short distance, came along with it; and a considerable number of minute vessels were seen running from the lining membrane of the uterus to the decidua, and the decidedly greater calibre of certain vessels at the margin was distinctly observed. At a short distance from the placental border, the decidua ceased to separate from the uterus, and remained in connexion with its lining membrane; as the substance of the placenta separated from the decidua, the same abundance of minute vessels was observed as between the uterus and decidua; but whether these were prolongations of the vessels proceeding from the lining membrane, or minute ramifications of the umbilical vessels, did not appear quite clear; for the rupture of the decidua by these vessels, during the separation, threw some little confusion upon the examination;—however the imperforate character of the decidua, which in some places we could observe, would, I think, lead to the inference that the minute vessels observed in separating the placenta from the decidua, were *not* prolongations of those noted between the uterus and this membrane, but extreme ramifications of the umbilical vessels adhering to the decidua. In two or three situations, a space of somewhat larger dimensions than the *superficies* of a half-crown piece was left, with the decidua attached to the lining membrane of the uterus, without the slightest laceration; beneath the decidua in this situation, a blow-pipe was introduced, and air was insufflated between it and the common lining membrane; the decidua was raised, and, to every appearance, there was not the slightest perforation. A director was then introduced, and the deciduous membrane divided; and, underneath, were observed two or three of those celebrated valvular orifices in the lining membrane of the uterus, most incontrovertibly closed by the apposition of the deciduous membrane. In another situation, after a portion of the decidua had been ascertained to be imperforate by the insufflation of air, it was separated a little more roughly; and upon attempting to pass a probe into the uterine sinuses, through one of their now well-known openings, a portion of decidua was found to have adhered to the margin of the orifice, and to close the aperture. On pushing the probe, however, through this portion of membrane, it readily entered one of the great uterine veins, whose disposition and relation to the connected structures, were precisely as described by Mr. Millard, in a communication which appeared in the Gazette a few weeks ago.

This case was considered by myself, and also by the gentlemen associated with me in the examination, to afford an additional corroboration of the views, relative to the utero-placental connexion, promulgated, within the last two or three years, to the medical world by Dr. R. Lee. The details, if considered by you to be of sufficient importance, are offered to the profession through the medium of your widely-circulated journal; and as unanimity upon this much agitated question is as yet far from being attained, it has appeared to me that the above account would not be devoid of either interest or utility.

Whilst upon this subject, if I shall not be presuming too much upon your liberality, I will offer a few remarks upon the communication of Mr. Hughson, which appeared in the Gazette of

the 12th ultimo, the main point of which appears to be, an implied charge of inconsistency against Dr. R. Lee, the basis of which rests upon an assertion of yours, in a previous number of the Gazette, that you had not heard of any change of opinion on the part of Dr. Lee, upon the subject of the placenta, placed in contrast with a statement made in your review of M. Velpeau's work on "Embryologie." If this eminent physiologist, as Dr. Lee is justly styled by Mr. Hughson, had really discovered, and confessed that any of his previous views were erroneous, and yet that his opinions were unchanged, there would indeed have been ground for Mr. Hughson's charge. But how stand the facts? Dr. Lee, in his celebrated paper published in the Philosophical Transactions, denies, in opposition to the Hunters, that the placenta, in the human subject, is composed of a maternal and fœtal portion; or that there are any cells in its structure in which the uterine and umbilical vessels terminate; or that the deciduous membrane is perforated at all by any large blood-vessel running into the substance of the placenta; and he asserts, in direct contradiction of the opinions till then almost universally entertained, that the openings, observed in that portion of the uterus to which the placenta adheres, are natural openings, existing in the lining membrane, and communicating with the great uterine veins; and that, so far from these latter perforating the decidua, and running into the placenta, their open mouths, or orifices, are closed during pregnancy by the apposition of the membrana decidua. He states, in addition, that a large number of minute arteries may be observed running, in a tortuous manner, from the common lining membrane of the uterus to the decidua. And which of the above propositions, I would ask, (comprising, as they do, the substance of Dr. Lee's communication to the Royal Society,) does your statement in the review of M. Velpeau's work negative? You observe, that in an examination made by Dr. Lee, in the presence of Mr. Lawrence, that "coagula of the maternal blood extended from some of the openings in the lining membrane of the uterus into canals formed by (layers of) the deciduous membrane on the margin of the placenta. These vessels or channels in the decidua could be traced only a short distance along the margin of the placenta, and between the lobes."

Is there a single assertion in the above statement which negatives any of Dr. Lee's views, expressed in his paper in the Philosophical Transactions? That, herein, we have an *addition* to the facts which he had previously communicated to the world, I admit; but an extension of knowledge is not identical with conviction of previous error.

Dr. Lee, in the examination you have alluded to, did not observe a perforation of the decidua, by large blood vessels running into the placenta; but as you have stated that there were canals along the margin of the placenta, *formed by the decidua*, and in communication with some of the great openings in the lining membrane; and having, about the period of this examination, had frequent personal communication with Dr. Lee, I am enabled further to state, that he had every reason to regard these canals as containing venous blood, and as being analogous in their function to the rest of the venous system. By this examination, Dr. Lee is enabled to supply a *deficiency* in his published views, but not to rectify an *error*. Suppose Dr. Lee to have been advocating his peculiar doctrines relative to the placenta, prior to his observation of the canals at the placental margin, with Professor Lauth, of Strasburg, who maintains that the communication between the uterus and the placenta is by lymphatics — suppose that Lauth had said to Lee, "You are aware, that wherever we have blood conveyed by arteries to any tissue, we have veins returning it; where are the veins returning the blood from the decidua, conveyed to it in such abundance by those little vessels which you describe as minute tortuous arteries? You do not discover any. My views of the lymphatic nature of these little vessels involve no such difficulty." What to this must have been the answer of Lee? Why, plainly, that he was in a little perplexity. But, by the investigation to which you have alluded in your review of Velpeau's work, the difficulty vanishes, and Lauth is disarmed of his most powerful weapon.

I am, sir,
Your obedient servant,
DANIEL NOBLE.

Manchester,
August 25, 1834.

CASE OF TETANUS SUCCESSFULLY TREATED.

To the Editor of the Medical Gazette.

Sir,

I send you for publication a case of tetanus, accompanied with some rather anomalous symptoms, indicative of great irritation of the medulla spinalis, and successfully treated by the exhibition of mercury, together with opium, taken internally, and applied externally.

August 12th.— —— King, æt. 30, a labouring man, received a severe contusion of his ring and little finger of the right hand, about three weeks ago. The accident occurred from the blow of a crane, and a contused wound of the second and third phalanges was the consequence; but we have no memoranda of the precise nature of the injury. He continued to attend as an out-patient at the London Hospital, and was apparently doing well, until about a week since, when, on returning home, he complained of pain in his back, and suddenly lost the use of his lower extremities. He was attended by a surgeon in the neighbourhood, who bled him largely twice, and ordered him some medicine. He states himself that he was no better for the treatment; but the partial recovery of his extremities indicates at any rate some amendment. He came to the hospital again yesterday, and this day I saw him, and found him in the following state:—He was scarcely able to move himself, from the partial loss of the use of his lower limbs. His mouth was nearly closed, so that he could scarcely put out the tip of his tongue, the moving powers of the lower jaw being excessively hard, and rigidly contracted. The sterno-mastoid muscles were somewhat stiff, as were also the muscles in front, and at the back of the neck. The abdominal muscles were literally as hard as a board. He presented the indescribable aspect of one affected with tetanus, the angles of the mouth being rather drawn apart, and the muscles of the face were tolerably well defined; and every now and then he was irresistibly led to close his eyes spasmodically. Whilst he was in the ward, and sitting on the bed-side, he was suddenly seized with a spasm of the muscles of the lower extremities, by which they were forcibly extended, and he slipped from off the bed. His bowels had been opened by medicine. The wounds of his fingers presented a sloughy appearance. He was now ordered—

> Calomel, gr. ij.; Opii, gr. ½, quartis horis; et Empl. Vesic. Nuchæ.

14th.—Is rather better, and is enabled to open his mouth wider. The muscles of his neck are not so rigid, but the muscles of the abdomen continue in the same state. The *tetanic* countenance is still present; his pulse is softer to-day, and not so rapid; his gums indicate the decided influence of mercury. He is ordered to omit the calomel, and to take—

> Mist. Camph. c. Tinct. Opii, gtt. xij. quartis horis; and the blistered surface is ordered to be dressed with an ointment composed of Ung. Cetacei, ℥j. et Acetat. Morph. gr. vj. Some of the same ointment is to be rubbed into the abdomen.

August 15th. — Somewhat better; muscles of abdomen and neck by no means so rigid; mouth very sore; he cannot open it to a greater extent than yesterday. He complains to-day of great numbness of the fingers of the left hand, which came on during the night; but he possesses the power of moving them. The condition of the wounds of his fingers is improving.

His bowels being costive, he is to take some cathartic medicine, and to proceed with the mixture as before.

It would be tedious to continue the details of the case from day to day. His tongue remained for some time exceedingly sore. The rigidity of the muscles of his throat and jaw gradually subsided, and he is now enabled to open his mouth completely, but the spastic state of the abdominal muscles still continues in a slight degree. He is enabled to walk about the ward, and is, in fact, in a state of complete convalescence.

I leave the case without comment, remarking only that it is one certainly not of ordinary occurrence, and one which, perhaps, some might be disposed not to consider as a case of tetanus. I think, however, that I am justified in so denominating it.—I am, sir,

Your obedient servant,

John Adams.

31, New Broad-Street,
Aug. 25, 1834.

TREATMENT OF CHOLERA BY STIMULANTS.

To the Editor of the Medical Gazette.

Sir,

The disease which, for the last two years, we have had to combat in an epidemic form, has now assumed that of the sporadic; and as medical men appear as much at issue on its treatment as ever, I feel it my duty (having seen much of it) briefly to offer my opinion. I do so with the less reserve, as the conclusions to which I have arrived are strengthened by the practical remarks of a master-writer* in our profession, which, though promulgated long before the appearance of cholera in England, will be found sufficiently applicable.

I rest my claims to attention upon the following *facts*, copied from my note-book.

In the first year of the disease (1832), when every new remedy incautiously offered as a specific was eagerly tried, out of 107 cases of malignant cholera, with the *characteristic dejections* and collapse, *only* 23 recovered — rather more than one-fifth. The recoveries were effected by stimulants.

In the second year (1833), out of 41 cases, all treated by stimulants, 17 recovered—*nearly half.*

I need merely, in this statement, allude to upwards of 150 cases of severe diarrhœa, almost approaching to cholera, *all* cured by stimulants, aromatics, and chalk.

The treatment I have found successful, and propose to adopt, is as follows:—When a patient is seen in a state of collapse, with the characteristic evacuations, and a pulse scarcely, if at all, perceptible; in addition to the external applications of warmth, mustard cataplasms, and friction, one table-spoonful of a mixture like the following should be given every ten minutes or quarter of an hour; alternating it with port-wine or brandy *ad libitum.*

℞ Confect. Aromat. ℥ss.; Spt. Æther. Sulph. Co. ℥ss.; Spt. Ammon. Arom. ℥ss.; Mist. Camphor. ℥v. ft. mistur.

The addition of Spt. Lavand. Co., or Tinct. Cardam. Co., or Ol. Menth. Pip. will be beneficial; and occasionally two or three drops of tinct. opii, will assist its retention on the stomach; but the less of this the better. An enema of one pint of hot water (from 100 to 108 deg.), thrown slowly up the bowels every two, or three, or four hours, will be found to act as a powerful stimulant. The occasional addition here of 3ss. or 3j. tinct. opii, relieves the spasms, without producing so much stupor as when taken into the stomach. Strong beef-tea is an useful adjunct to these remedies, which, if sedulously adopted, will produce reaction *more frequently than any others;* which object being effected, the cautious practitioner will know when to stop, and change them for calomel, salines, and topical or even general depletion. It is to the state of collapse I wish to direct my observations at present. Now, from whatever cause cholera may arise, is it not "a state of prostration without re-action?" "I have called," says Mr. Travers*, "this state prostration, because the term is descriptive, without involving an hypothesis to explain it. It is a state not actually amounting to, but threatening, or nearly approaching, a cessation of vital action."

"What are the means by which we can hope to rally the flagging power, and restore the natural harmonies of the system? The state of the circulation inferred from the pulse, complexion, breathing, and temperature, is our chief, if not our only guide. If the shock be unattended by hæmorrhage, and as the argument supposes, by lesion of any vital organ, immediate or accessory, cordial drinks, as spirituous and vinous liquors and compounds, and what are called diffusible stimuli, as ammonia, æther, camphor, &c. with the aromatic distilled waters, and warm and gently stimulating enemas, are the most obvious internal remedies."

The above description of prostration wants but few symptoms to make it the collapse of cholera. The treatment will be illustrated in the following case.

About three o'clock on Monday, August 25, 1834, a poor woman brought her little girl, aged four years, into the Dispensary, with the following symptoms:—Constant vomiting; deadly cold skin; countenance collapsed; eyes sunk; pulse scarcely perceptible at the wrist. She had not been

* Mr. Travers on Constitutional Irritation.

* On Constitutional Irritation, p. 454.

in the laboratory many minutes before she had passed a characteristic rice-water dejection: indeed, the symptoms came on so severely, I expected to see her expire in her mother's arms. The child was attacked suddenly at ten in the morning. I gave a teaspoonful of the mixture above prescribed every ten minutes, and sent the child home, accompanied by my young friend, Mr. J. Brett, who administered the same doses alternately, with a teaspoonful of port wine, until half-past four, when reaction was commencing. At six (three hours after I first saw it) the child was sitting up in bed playing.

The next day, August 26th, the secretions were restored. The child had passed urine for the first time for twenty-four hours, and indeed it was convalescent, without so much febrile excitement as I expected.

Mr. Travers continues further on:—"No nurse can be qualified to superintend, or at least to direct the administration of the stimulus. It is the devotion of a few hours to the life of a fellow-creature*."

Again: "It is idly premature to talk about the secretions. Medicines directed to this object cannot act beneficially in such a state of the nervous system, if, as I much doubt, they act at all. It is, besides, of importance to avoid putting any thing into the stomach but what is essential to support the faultering action of the heart and diaphragm. Since it is upon the stomach we place our chief reliance, we should endeavour by every means to keep it in temper."

I will not occupy your pages by extracting more from a work, to which I refer your readers; trusting I have quoted enough in confirmation of my own facts to prove that the more rational our treatment in this, as well as in every other disease, the more successful will be the result.

I am, sir,
Your obedient servant,
Daniel Norton Stable.

General Dispensary,
36, Aldersgate-Street,
August 27, 1834.

* On Constitutional Irritation, p. 456.

PUERPERAL PERITONITIS,

WITH SOME ANOMALOUS CIRCUMSTANCES, TERMINATING FATALLY.

To the Editor of the Medical Gazette.

Sir,

Should you consider the following case as sufficiently interesting to merit a place in your columns, may I request the favour of its insertion.

I remain, sir,
Your obedient servant,
T. Ogier Ward, M.D.

Birmingham, Sept. 1, 1834.

Mrs. Burrows, Ball-Street, æt. 19, tall and slim, general health good, was confined on the 11th ultimo with her first child. The labour was so easy, that the child was born before the surgeon arrived. She continued perfectly well till the 18th, when, at six P.M., she complained of chilliness, want of appetite, and general feverishness.

When Mr. Porter, the surgeon in attendance, saw her, she was lying on her back with her legs stretched out, complaining only of chilliness and general uneasiness. There was no sickness nor pain in any part of the abdomen on pressure, nor any distention of the bowels, which were open, but not relaxed. The secretion of milk had stopped, but the lochia continued. Pulse 90, moderately strong; tongue brownish.

℞ Liq. Ammon. Acet., Mist. Camph., Ant. Tart.

19th.—Passed a good night; much better; bowels open; no pain in abdomen on pressure; tongue cleaner, red at the edges; pulse as before.

20th.—The same state.

21st.—A great deal better; the milk had returned.

22d.—Continued better.

23d, Noon.—Some pain above pubes on pressure, which had come on about twenty minutes before, and which was almost immediately removed by fomentations. In other respects she was much the same.

24th.—Last night, at 10 P.M., a most profuse diarrhœa came on, which lasted till 6 A.M., when it abated, and from this time till 9 P.M. she had only three motions. At half-past 10 P.M. I found

her lying on her side, with her knees drawn up in an easy posture; belly tympanitic, but with no pain on pressure; hands cold and clammy; face pale and haggard; eyes staring; tongue dry, with black sordes on teeth and gums; great thirst; respiration hurried, but not difficult; pulse just perceptible, but countless from its rapidity. Though rather delirious, she answered rationally; and I learnt that there had been a little delirium at night throughout the course of the disease. The stools she had passed were dark and offensive. As she was evidently sinking fast, I merely ordered food and stimulants. She died at seven next morning.

Inspection, twelve hours after death.—Epigastric region still quite warm; tympanitis; lungs healthy, with old adhesions at upper and back part of the right; no effusion in the chest; heart healthy; whole surface of peritoneum of a rosy hue; shreds of lymph, recently effused, filled up all the angles of the viscera; about twelve ounces of straw-coloured rather opaque fluid in cavity of the abdomen; peritoneal covering of the uterus pink, but the colour did not extend deeper into the substance of the organ; broad ligaments and ovaries of a deep red; interior of uterus lined with blood (lochia), and some vegetations where the placenta had been attached; uterus about five inches square, and its parietes three-quarters of an inch thick; bowels distended with air, but void of fæces.

Remarks.—In the works of Hey, Gooch, Lee, and Marshall Hall, in the Cyclopædia, on the inflammation of the peritoneum after delivery, pain is always mentioned as being present in a greater or less degree; and by the last-named author it is considered as the only pathognomonic sign of that disease. Andral, also, and the other French writers, speak of pain as being the most prominent symptom of peritonitis; and when conjoined with tension of the abdomen (also absent till just at the termination of this case), as forming its surest diagnostic mark. It may, therefore, be a question, when the inflammation of the peritoneum commenced—Was it on the 18th, when the first febrile symptoms appeared?—or was it at noon of the 23rd, at which time pain was first experienced? The effused lymph was quite recent enough in its appearance to warrant the latter supposition; but if so, the absence of pain is rendered still more extraordinary, besides that it is rare for diarrhœa to supervene till the close of the complaint; and this idea would not account for the febrile state of the five previous days.

Another source of difficulty, in forming a correct diagnosis, arose from the consideration of the prevalence of fever in this town at the present time, which, in the cases I have witnessed, has proceeded gradually till diarrhœa or pneumonia has thrown the patient at once into a state of prostration. The former of these (diarrhœa) has been and is still so prevalent in this part of the kingdom, that I have been informed that in some places a fourth of the population have been attacked, and several deaths have occurred. Mr. Porter, while attending another case (of fever and diarrhœa, with pain over the uterus after delivery) with me this morning, mentioned a third in which pain came on in the left inguinal region in the same insidious manner; but it was removed by fomentations, and the fever cut short by freely leeching the part.

NOTE ON THE

MODE IN WHICH THE UTERUS CONTRACTS AFTER DELIVERY.

By Samuel Malins, M.D.
Lecturer on Midwifery, and Physician-Accoucheur in Liverpool.

In the 46th of his excellent lectures, at page 740 of this volume, Dr. F. H. Ramsbotham, speaking of hour-glass contraction of the uterus, declares his conviction of its extreme infrequency, and that in reputed cases of that nature "the contraction of the uterus, indeed, is of the globular kind; that its whole cavity is considered the upper chamber; the os uteri being taken for the constriction of the central fibres of the body, and the dilated vagina,—having in it a coagulum of blood,—for the lower chamber." This opinion, however, is stated to be in opposition to that of Mr. Burns, who, in treating of hæmorrhage after delivery, thus expresses himself:—"Generally a circular band of fibres contracts spasmodically about the upper part of the cervix uteri, enclosing the placenta above it, whilst the rest of the

fibres become relaxed. This has been called, though not very aptly, the hour-glass uterus; and if I did not know the hazard of establishing a general rule, I would say, that in almost every instance this contraction takes place. I have scarcely ever introduced the hand into the uterus, in a case of flooding, without meeting with it, whether the placenta had or had not been expelled."

There is a fact, not, I believe, generally known, with regard to the manner in which the uterus naturally contracts after disburdening itself of the fœtus, which, applied to the discrepancy in question, will go far towards shewing that it is rather seeming than real; that proper hour-glass contraction is, as Dr. Ramsbotham supposes, very rare, and yet that a circular contraction of the uterine fibres at the upper part of the cervix, is, as Mr. Burns describes, very frequent. Every anatomist knows that the cavity of the uterus, in the unimpregnated state, consists of two portions, of a different shape, divided by a contraction of the organ, considerable enough, internally, to produce what has been often called the *os internum uteri*. The distinctness, however, of the cavities, disappears under gestation, and the whole uterus then forms but one spheroidal cavity; but as soon as the removal of the distending causes allows the uterus to contract, it at once recovers, in a great degree, its original shape; two cavities again exist, and they are divided by that contraction of the os internum uteri, perfectly natural and normal, indeed, in character, but to which the name of hour-glass contraction, as denoting a preternatural state, has been in error so constantly applied. The statement of Mr. Burns is therefore correct as to the fact, but incorrect as to the cause assigned for it, which is not spasm, but uterine action regulated by the ordinary law of contraction in the part.

The statement of Dr. Ramsbotham as to the rarity of inordinate contraction of the circular fibres of the body of the uterus, while those of the fundus and cervix are relaxed, is also correct, and not at variance with the remark of Burns; but he has evidently fallen into error, by overlooking the usual and healthy action of the internal os uteri. This it is which divides the upper part of the genital canal into two chambers, one of which may contain the placenta, while the other, formed by the cavity of the cervix uteri, may either be filled with coagula, or, as frequently happens, remain flaccid and empty.

Liverpool, Aug. 30, 1834.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

The Dublin Practice of Midwifery. By HENRY MAUNSELL, M.D., *Accoucheur to the Wellesley Female Institution, Lecturer on Midwifery, &c.*

THIS, we think, will be found to be a very convenient manual for students. It is expressly written to supply the defect of a suitable work to occupy the young practitioner's leisure in the lying-in room; and for this purpose it seems peculiarly qualified by its commodious form. The contents embrace all the most useful subjects connected with midwifery practice, commencing with a description of the parts concerned, describing the duties of the accoucheur on ordinary and extraordinary occasions, and concluding with practical remarks on the maladies and accidents belonging to the puerperal state. The style is plain, and without pretence to any thing but utility. We are glad to find that the author has had the good sense to eschew all those newly-coined absurd obstetrical terms, which are so disgustingly ridiculous in some modern manuals.

Illustrations of the Effects of Poisons. By G. L. ROUPELL, M.D. *The Plates from original Drawings, by* ANDREW M. M'WHINNIE, M.R.C.S. Part II.

IN noticing Part I. of Dr. Roupell's Illustrations, we observed that a great desideratum in the study of pathological appearances was likely to be supplied by this work; and we are glad to find that the present fasciculus is fully equal to, if it does not surpass, the first. The plates now published exhibit vividly—1, the effects of concentrated sulphuric acid taken into the stomach; 2, the

effects of a dose of oxalic acid; 3, the morbid appearances in the stomach of a dog poisoned with corrosive sublimate; and, 4, the effects of alcohol on the mucous membrane. This last would make an excellent frontispiece for the volume on dram-drinking, contemplated by the parliamentary drunkenness committee. It represents the stomach of a dog, to which an ounce of rectified spirit, diluted with an ounce of water, had been administered. The greater portion of the organ is in a state of intense deep-crimson vascularity.

As to the style of execution of the plates before us, nothing of the kind can be better: they are more like original drawings than impressions from copper or stone; and in the colouring, the greatest pains seem to have been taken to render the work what it should be—an exact representation of nature. To medical jurists these illustrations must be exceedingly valuable.

MEDICAL GAZETTE.

Saturday, September 6, 1834.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."
CICERO.

PARLIAMENTARY EVIDENCE ON MEDICAL REFORM.

WE have so fully and freely expressed our opinions regarding medical reform, that it must be needless, even if we were so inclined, to recur to the subject in order to state once again what are our views. Our readers, we fancy, will be much better pleased to gather for themselves, from the documents which we shall submit to their notice, how far those opinions are supported by the best authorities who have *since* been examined before the Parliamentary Committee. Never, perhaps, was there a great question like this, involving the interests and feelings of so large and respectable a portion of the community, on which so much unanimity has prevailed, so far as the fundamental point of *the absolute necessity of reform* of some description or other is concerned. Here all denominations of medical politicians may be considered as of one mind—from the most pertinacious conservative to the most reckless radical; at least, so far as we can judge from the piles of evidence heaped up before us, and which we have scrutinized with no ordinary care, there is not to be met with one voice dissentient from the generally expressed desire to effect a fitting change in the constitution of the profession. In details, there are of course wide and almost irreconcileable differences of sentiment; but, regarding the subject as a whole—the great aim of which is to establish a new and improved standard of qualification for entering the profession, and maintaining rank in it—all are agreed.

Let us see what says the learned president himself of the College of Physicians on this point. The evidence of Sir Henry Halford is voluminous, and in a great degree technical—relating to the duties and prerogatives of the College officers: enough, however, has been elicited from him to show how strongly even he has felt the expediency of reform. Sir Henry has for some time been known to have entertained ideas of reforming, so far as his jurisdiction and influence extend, the corporate body over which he presides. We have him here publicly referring to transactions and measures discussed in the College under his sanction. After being questioned at great length on points connected with the office of President, the examination suddenly turning on the subject of education, Sir Henry gives the answers to the following questions:—

"There have been plans under the consideration of the College for regulating the period and course of study to be pursued in future by persons applying

for the licence of the College?—There have.

"Are there no doubts whether the College possesses the power or not to exact this course of study?—No; there is no doubt about that, I believe.

"You think it expedient to enjoin a period and a course of study from all who come to solicit the licence?—I think it is necessary now, particularly from the improved knowledge of the day, that we should increase the claims of information.

"You say, "now, particularly from the improved knowledge of the day;" do you mean that medical men now are better informed than they were?—I think so.

"Are you of opinion that the improvement is not confined to physicians, but extends to the humbler grades of the profession?—Yes.

"You think it therefore necessary that physicians should keep pace with this great improvement?—Yes; I think we should enlarge our claims for knowledge.

"In what state of progress are those new propositions; have they been submitted to the College for consideration?—Yes; and have passed three times."

Respecting the propositions here alluded to, we have further information in the evidence of Dr. Maemichael, from which we take the following extract:—

"Have any propositions for those reforms been brought forward for discussion in the College, since the question of medical reform has been agitated by the public?—Yes, for two or three years before that, it had been talked of.

"When did the reform committee, if it may be so described, begin to sit?—I do not recollect precisely, but some time ago.

"Are you a member of the reform committee?—Yes.

"Have the committee kept minutes of their proceedings?—I think so.

"At what periods have the various clauses of the Reform Bill been discussed?—I understood the questions to refer to the new curriculum; that has been much discussed.

"Will you inform the Committee to what objects the attention of the reform committee has been directed?—To the drawing out a scheme for medical education, which would chiefly enable the student to learn his profession in London, and to come to the college to be examined for a licence to practise. That has been considered the great object.

"Did you also discuss in that committee the proposition of throwing open the election, and removing the disqualifications which have been adverted to in the course of this examination?—Chiefly medical education.

"The matters discussed before that committee will appear upon the minutes of their proceedings?—I suppose so; but I am not the secretary.

"Who is the secretary of that committee?—I think the registrar, Dr. Hawkins.

"What has been the fate of the propositions recommended by the committee?—The propositions recommended by the committee have not been finally adopted; they have been only discussed; because I think the College judged very rightly, that whatever alteration they might make now in their laws, might be supposed to be the effect of fear and intimidation, but they have not given them up; they began them three or four years ago.

"Were not certain propositions brought forward lately which were negatived?—They were not negatived; they were only postponed.

"*Sine die?*—No, not *sine die.*

"How many times has the Reform Bill been read?—I beg to say, that I have given my faith not to reveal the secrets of the college.—I can only say several times; I cannot say how often.

"Have not certain propositions, which have been brought forward, been rejected; not those on medical education, but certain other propositions?—There has been a great diversity of opinion on certain points; some of the proposals have been rejected, but the precise points I cannot say."

But to return to Sir Henry Halford. Presently, after the above questions, we find the examination proceeding to the following most important subject:—

"Are you of opinion that there should be in London, as being the first medical school in the country, from its large hospitals, in which every form of disease is sure to occur, some authority

empowered to grant medical degrees? —I am firmly of opinion that is necessary at this time of day?

"What is the constitution that you are disposed to recommend for such a board or body, empowered to grant degrees?—It appears to me that the best and most effectual of the plans proposed would be, to give the College of Physicians the power of granting that medical distinction; for, from whatever part of the world persons come, they have no right to practise without the sanction of the College. When the College has granted its licence, let it have the power of granting also the title. With regard to any private schools, it would be wrong to lodge it there; there would be a competition for the maximum of acquirement in the minimum of time. Any one of the schools should be considered as a particular college. If you were to grant that particular college the university honours, or to all the colleges the power of granting degrees, they would have the cognizance only over their own pupils, and would hold out an inducement to get pupils. 'We will grant you a degree at the end of two years; we will grant you a degree at the end of one year.' It would create such an abuse as to endanger the respectability of the profession. Our business is, not to teach physic, but to find out the qualifications of those who have been taught.

"What induces you to think such a power would be best lodged in the College of Physicians?—Because we are disinterested in the medical schools; the other would, I think, be liable to abuse. By giving their own pupils the degrees, they would get a large acquisition of pupils; but it would be in their power to say, 'We will be content with less qualification.'

* * * *

"You are not in favour of a Board, consisting of physicians, surgeons, and apothecaries, to examine in their several departments of medicine?—I should not object to there being a Board of that description to license general practitioners, but not physicians. The College asks for no extension of power. Under those circumstances, if they choose to say, 'We will not expect a degree, but only certificates of attendance on certain lectures; we will then examine you; we will not ask for a degree;' which will be absolutely necessary, if the College of Physicians had not the power of granting the degrees. We ask for no extension of any authority, nor of the power we have at present. We have determined that a person shall come here, and present his certificates of five years' attendance at the various hospitals, a course of anatomy and botany, and so on. What are we to call him, when we have examined him? We cannot call him doctor, though he is from thenceforth M.D. to all intents and purposes. We have nothing to do but to annul the statute which requires two years to have been spent at a university, and then we can examine on the certificates. The College of Physicians ask, therefore, no extension of authority in asking that power; and, I have reason to believe, the universities have no objection to that."

The triple Board is again suggested to Sir Henry in a subsequent question, and as distinctly rejected:—

"Do you not think some advantage would attend the appointing a general Board, composed of the three classes of practitioners, to examine all parties who come for a degree; inasmuch as apothecaries may be supposed to know much more of pharmacy, and surgeons much more of surgery, than physicians ordinarily do?—Not for the doctorate; perhaps it would be desirable to make a Board to examine general practitioners; but for the degree of Doctor of Physic, I think the thing should remain as it is, and that those who wish to be surgeons should be examined by the College of Surgeons, but let the general practitioner make himself amenable to a general Board, if you please."

We are tempted to give one more extract from Sir Henry's evidence, as it serves to shew his sentiments still further on the necessity for grades in the profession:—

"Do you think, that considering the numbers of those for whom the general practitioner practises, it is of more consequence that he should be well educated than the physician?—No.

"You must be aware that the num-

ber of patients for whom the general practitioner prescribes is much larger than those for whom the physician prescribes?—Yes; but there are persons who will not appreciate those advantages; the multitude will not.

"It is an object to secure the attendance of well-qualified men on the public in general?—Undoubtedly.

"The object being the public good, is it not highly desirable that this preliminary knowledge shall be extended also to the lower grade of practitioners?—As much as possible, and I should be very glad if that could be done; but there must be many persons who, from particular circumstances, cannot be expected to have bent their minds to those abstruse inquiries that would enter the mind of a physician, and it is not absolutely necessary.

"Why should it be necessary to a physician, who is principally called upon to prescribe for the rich, and not extend it to those who prescribe for the poor?—It is impracticable to obtain it; there is a number of practitioners wanted, and there are men who cannot afford funds to procure this kind of information. Physicians will not be prevailed upon to expend pounds upon their education, when they are to expect to receive only pence in return."

Sir H. Halford, in another part of his evidence, expresses himself strongly in favour of the general practitioner being paid by fees for his time and trouble, rather than by the amount of his bill for medicines prescribed.

"Do you approve of the practice of paying apothecaries by the amount of their bills for medicines, and not for their attendance?—No; I set my face very much against that; I had rather they were paid for their trouble, for every apothecary who enters a house enters it with that mark upon his forehead. 'You are to take as much medicine as I can get you to take, and that procured by me at as cheap a rate as I can procure it.' It is a bad system only to be paid according to the proportion of physic they can get a person to take.

"Do not apothecaries now make a charge according to their visits?—In some instances, I believe, but not universally.

"Is not the practice increasing?—Perhaps it is; I wish it were universal."

On the subject of the connexion between the English Universities and the College of Physicians, the learned President is copious; and we are rather disposed to smile when we observe how strongly he upholds the *exceeding goodness* of the education in physic given at Oxford and Cambridge.

"Is not more attention paid to medical education at the Scotch Universities than at Oxford and Cambridge; are not the students required to attend more courses of lectures, and are not those courses of longer duration?—I have been at both of them, and I really do not see that there was a less good opportunity of acquiring information at the English University than at the others: there are clinical lectures, and anatomical, and exceedingly good lectures.

"It has been stated, that the duration of the courses at Oxford and Cambridge is by no means equal to that of the same courses at the Scotch universities; that consequently many subjects treated in detail at the latter, cannot be so fully investigated as the former?—Let the superior merit as a place of teaching at the Scotch universities be what it will, the English graduates go there; so that I contend for it, that the Fellows of the College (those who come to be candidates in order to be fellows hereafter,) have all the advantages that the people educated at the Scotch Universities have, besides that of the preliminary education."

To this and other points we may have occasion hereafter to refer: at present we wish to confine ourselves to the indications of a strong desire for reform manifested by parties commonly supposed to be obstinately conservative.

Dr. Seymour's evidence is excellent: it is always shrewd and to the point; and it abounds in information on a great variety of subjects. It will be seen, by the first question in the following extract, that the Doctor does not profess to keep such a term as "medical reform" in his vocabulary: yet, for all that, if we read on, we shall find that *he* has his reforming projects as well as the rest of his professional brethren.

"Did you take any part in the re-

cent proceedings of the College, with regard to medical reform?—I do not know what is meant exactly by medical reform.

"The question refers principally to alterations in the bye-laws regulating admission into the College?—I have. In the first instance I opposed the abrogation of those laws. I should mention first, that there was an education committee appointed, of which I was one of the persons who promoted the measure; and a scheme of education was drawn out, taken from the practice of all universities at home and abroad, and more especially in reference to the longer degree of hospital attendance for a physician; those were printed. Subsequently to that, it was moved in the College, that the laws with reference to the restrictions as to the English universities, as they exist, should be repealed. I did not think the reasons adduced sufficient for it; and I think those laws, provided there is a sufficient admission by the other laws, wholesome, and extremely useful to the profession.

"Have there not been two committees appointed, one on the subject of education, and another at a more recent period, on the subject of some new propositions?—For the purpose of inquiring whether those laws should be altered; I was not a member of that committee.

"Have you seen a copy of the propositions on that subject reduced to print?—I have."

"Were those propositions, in your opinion, wholesome, or otherwise?—Not wholesome.

"You opposed them?—To the utmost of my power.

* * * *

"Not approving of the proposed changes in the statutes of the College, are you of opinion that any changes are necessary in its constitution, or statutes?—Yes; I think much might be done.

"What changes do you think expedient?—I think it would be very desirable that the gates should be set open wider for the admission of Licentiates, but that the regular road should still be kept through the English Universities.

"In what way do you think the doors should be thrown open wider for the admission of Licentiates?—I think there should be an election, either annually or biennially; and I am inclined to think it would better meet the justice of the case, that the nomination should take place from the Licentiates, and a certain number of persons so nominated should be elected by the College.

"Who, according to your proposal, would, in the first instance, nominate Licentiates?—The Licentiates should nominate a certain number, and the Fellows elect from them; but I beg to be understood that this is not a proposition which has been made in the College.

"What number would you have proposed in that way?—I should say six nominated, and three elected.

"You mean these in addition to the Graduates of Oxford and Cambridge, admitted in the usual way?—Yes."

We have only room for one passage more from the evidence of Dr. Seymour, with which we must close our extracts for the present.

"With reference to any changes which might possibly be made in the bye-laws and regulations of the College of Physicians, might not that matter be pretty safely left with the College itself; or is the system so bad that there is a necessity for legislative interference?—I think that great advantages will result from this inquiry, and I think any alteration by the College, before this inquiry, or immediately preceding it, was ill-judged; but when this Committee has terminated its sittings, its suggestions, or its Report, will enable the College to decide very much better; and as the law now stands, the College has the law in its own hands, and nothing but an act of parliament can take it away.

"The system of the College is not so bad as to require the interference of Parliament?—Certainly not; I think it might be very well changed by the College itself.

"Are they likely to turn a deaf ear to any suggestions that may result from this inquiry?—Certainly not: I think they would consider them with the utmost patience, and with the best goodwill.

"Is it not the object of a public medical institution, such as the College of Physicians, to confer a sanction upon practitioners, by which the public may be able to distinguish who are those properly qualified: does the College, in fact, answer that object?—I think it

does; at the same time, I think there is nothing so perfect but that it might be made something better.

"But that it answers that object as well as can be expected, considering it to be a human institution?—*Of a very great age.*

"But not in a state of decay, or decrepitude?—*No, a green old age!*"

In the preceding passages we have endeavoured to present a fair view of the opinions of those from whose evidence we have made extracts. We feel it to be a duty, however, by way of postscript, to protest, in the strongest terms, against the spirit in which the same task has been performed by a contemporary journalist; and we do so to put the medical profession on their guard against receiving the statements of the writer alluded to as correct. The editor of the Lancet had pledged himself to "shew up" Sir Henry Halford's evidence, even before that evidence was given; and now that the time for redeeming his promise has arrived, finds the task more difficult than he anticipated. How is the evil to be remedied? Simply thus:—When Sir Henry's answers do not suit the desired purpose, they are altered to the writer's mind: for example, at the opening of his examination, Mr. Warburton remarks, "In the annals of the College, June 12, 1812, there is a resolution said to be inserted in the 'Book of Rules and Standing Orders;' that would appear to imply that there are books of rules and standing orders?" To which Sir Henry replied, "I have a book in my possession as president, by which I am informed of the course of my duties." And again—"The book is for the governance of the president himself." But Mr. Warburton still urged, that the book appeared to be intended for the governance of the Fellows also; and to this renewed observation Sir Henry Halford answered—"I do not remember to have read that in the book to which I allude, of directions for the President's conduct." Now this rejoinder, which is in perfect keeping with, and, in a manner, follows up, the original answer, is actually quoted by the Lancet as contradicting it—nay, the two are set down opposite to each other, to render their inconsistency the more striking. But in order to accomplish this, Sir Henry Halford's second answer is remodelled. By a cunning device, the situation in the sentence of the word *that* is changed, and thus the meaning of the whole is perverted. For, "I do not remember *to have read that in the book* of directions for the President's conduct," our honest contemporary substitutes, "I do not remember *to have read in that book* the directions for the President's conduct." The point is a miserable one at best, and shews how hard the writer was pushed for something to find fault with: but the trick resorted to, to make good a foregone conclusion, is that of the most paltry nature, and altogether indefensible. It has this advantage, however,—that it shews, at the very outset, the *spirit* in which the evidence before the Parliamentary Committee is to be dealt with.

DR. WILSON PHILIP.

This distinguished practitioner was lately appointed a Fellow of the College of Physicians.

CHOLERA.

It was stated last week, by a contemporary medical journalist, that the malignant cholera had again broken out in London, "with a degree of virulence which was unprecedented in the two previous pestilential visitations." Statements of this kind, even if true, are injudicious, calculated as they are to alarm the public mind; while, being (as in the present instance) unsupported by facts, they become altogether unwarrantable. The truth is, we are happy to say,

that cholera, though it has returned every summer since its first introduction, has steadily diminished on each occasion, and, in keeping with this general rule, has been decidedly less prevalent this year than in any of the preceding. The cases, indeed, which have occurred, have been for the most part rapid; and many of them having been in what is called the *better* ranks of life, they have been brought prominently before the public attention. In other instances, accidental circumstances have given importance to a single case—as that of the matron of St. George's Hospital; whose death, from the manner in which it has been announced, led many to suppose that she was but one of many victims in the institution with which she was connected; whereas we find that no other case has occurred in that hospital during the whole season. The number of deaths, as given in the Bills of Mortality, have decreased this week by thirty.

AN AUTOPSY BY STEAM.

One of the steam-carriages on the New Road, we observe, has been styled "The Autopsy;"—an ominous name, some medical men may think, having notions of death and dissection in their head the moment they hear of it; and some may even fancy it absurd. But we do not agree with them; for autopsy is surely just as good a name for a steam-coach as for the thing it has latterly been applied to in medicine. It properly means what one has seen with one's own eyes—just as autograph does what one has written with one's own hand; and how is it, in any one respect, more aplicable to a *post-mortem* than to a steam-coach? Whenever, in future, we see autopsy prefixed to the report of a post-mortem examination, we shall scarcely be able to divest our minds of the steam-coach, with its hurry, noise, and smoke. We wish the worthy steam proprietors would borrow a few more hard and unmeaning words from us; for, thanks to the medico-pedantic spirit prevailing in some quarters, we have latterly had a heavy overflow of them.

CASES OF STONE IN THE BLADDER; With Remarks.

Given at St. George's Hospital,

By Mr. Cæsar Hawkins.

1.—*Three cases cured by Lithotomy.*

2.—*Case of Stone fatal, from refusal of Operation, with Disease of Kidneys.*

Mr. Hawkins has recently had four patients at one time under his care, with stone in the bladder. Three of these were little boys lying in contiguous beds, and all about five or six years old, in whom the lateral operation was performed, and who have now all left the hospital cured. Two of these cases have already been spoken of*, and got well without any thing remarkable: the third was operated on July 31st; but this boy required some care after the operation, as the water did not pass away very readily by the wound; the consequence of which was that he was restless the night after the operation, and even had a slight rigor the following morning. To relieve this a catheter was kept in the wound the greater part of the succeeding eight or ten days, as, whenever it was taken out for a short time on account of a little irritation in the bladder, a partial retention of urine in the bladder took place. When the catheter was finally removed, however, the sides of the wound immediately united, and all the water passed by the urethra two days afterwards, and he was wholly cured in as short a time as either of the others.

The fourth case was unusual in an hospital, as the patient died of the effects of the stone a few days after his admission, without giving an opportunity for operation; the poor boy literally falling a sacrifice to the obstinacy of his father, who had always refused to send his son to any hospital, though he had been repeatedly asked by friends and neighbours to let something be done for the dreadful sufferings the boy endured for several years.

This boy, Robert Bird, aged fourteen years, was admitted into the hospital July 30th, presenting a most frightful appearance of emaciation, and suffering from pain, as, while his arms and legs were scarcely any thing more than bone and skin, the muscles of the abdomen were of very great size, from constant straining to make water; the exact course and divi-

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* See p.621 of the present vol.

sions, and almost all the fibres of the muscles, being visible to the eye, and in a state of almost unceasing contraction; and the muscles of the face, being enlarged and strong, were expressive of the agony he endured. This state of things had lasted for four years in an aggravated form; but the stone had no doubt existed for some time before this, as he had long before been observed to have his hands constantly in his pockets, from irritation of the penis and prepuce, which were very much swollen and inflamed at the time of his admission, from the violence with which he pinched and grasped these parts whenever he made water. Two years ago, indeed, his symptoms were relieved for a time by the passage of a small calculus through the urethra, but his sufferings returned again, worse than ever. When admitted, the urine was incessantly dribbling away, so that it was only once or twice during the time he lived that as much as half an ounce could be obtained for the purpose of examination. He had a good deal of tenderness of the lower part of the abdomen, though none in the loins. Lately he had experienced repeated attacks of diarrhœa, which had contributed still further to weaken him. His pulse was quick and weak; the skin dry and harsh; the tongue generally moist, but very red towards the sides and tip, as if from disease of mucous membrane. It was obvious, in short, that if he could not be very quickly relieved from his present condition, he could not support his sufferings much longer. This was attempted to be done by a warm-bath, some castor-oil, laudanum and starch injection, and a mixture with some soda and magnesia and laudanum; together with some spirituous lotion to the inflamed penis; and sounding him was postponed till he was in a little quieter state.

August 1st.—He has been, on the whole, considerably easier; so that (to use his own expression) he was in heaven, compared to what he used to feel; and a little water was collected, which was acid, but mixed with nearly one-third of pure pus, with a very small quantity of mucus.

Mr. Hawkins passed a catheter into the bladder, and found it almost entirely filled with a large calculus, without containing any water; and the passage gave such intolerable pain that the instrument was withdrawn without attempting to pass it along the stone, so as to measure its size. Examination by the rectum shewed, however, that it was nearly as large as an egg, and that the bladder was quite contracted round it, and a good deal thickened. This examination also gave very great pain.

Mr. Hawkins gave the foregoing account of his examination, and at the same time remarked that the case was one which well deserved attention, as exhibiting the termination of stone in the bladder, when left entirely to itself, a state which was not often seen in young persons. It was very desirable to ascertain, if possible, the condition of the urinary organs; which could, however, only be partially made out in such cases. There could be little doubt, from the excessive pain excited, not only by the stone but by the gentlest touch of the catheter, or of the finger in the rectum, that there was some ulceration of the neck of the bladder. In the next place, the appearance of the water made it highly probable that there was disease of the kidney; since the quantity of pus was very great, with very little mucus. A great quantity of pus might, indeed, be secreted by an inflamed bladder, but then it was commonly mixed copiously with the ropy mucus of the same surface; whereas there was in this boy scarcely any mucus, neither was it probable that so much purulent secretion should come from an ulcerated surface of the bladder; so that the pus probably came either from the kidney or from a local abscess connected with the bladder. In general, when the kidneys are much altered in structure, and secreting constantly a large quantity of pus, the urine was alkaline; whereas this boy's was still acid. Still this rule was not uniform, and therefore he would not pretend to say from which source the purulent matter came. In consequence, however, of finding so much pus in the water, he should not continue the alkaline remedies which he had prescribed before he knew what the state of the water was, because an acid condition of urine is most frequently met with in young subjects, and an alkaline medicine serves to relieve irritation of the bladder arising from this cause; whenever much pus is formed, however, especially if conjoined with mucus, there is reason to apprehend a tendency to phosphatic deposits; and an alkali, in any quantity, is injurious. In a stone case, in an old person, on this account he would not prescribe alkaline medicine on an uncertainty, as the most probable form of stone would be alkaline. The absence of pain in the back was no argument against the existence of disease of the kidneys, since that disease might easily be masked by the greater suffering arising from the state of the bladder.

His object, Mr. Hawkins said, would be, if possible, to bring him into a state fit for operation, though it seemed very likely that he would sink before an opportunity

was afforded for the removal of the stone; at present, certainly any operation must necessarily be fatal. If the pus came from the kidneys, it was most probable, indeed, that he would survive, even if he was in a much better state than at present; if it came from an abscess about the bladder, he would have somewhat more chance, though in any case but a small one. Mr. Hawkins operated on a case of stone some years ago, in a man who had an abscess of considerable size in the prostate gland, which was relieved by appropriate treatment before the operation; and the cavity in the gland, which was large enough to lose the end of the catheter for some time in it, and draw off a good deal of pus before the point entered the bladder, was pretty well filled up. This man recovered with great difficulty indeed; and Mr. Hawkins had recently seen him quite well. As to the kind of operation, this boy was evidently (like the three others on whom he had recently operated) not a proper case for lithotrity, as it was clear, with a contracted bladder, not holding half an ounce of water, and an ulceration of its coats, or at least a very high state of inflammation, the only possible chance of success was by immediately and at once getting rid of the stone; and this would hold good even if it had been a small stone instead of a very large one.

The temporary relief experienced by the boy did not continue, however; constant recourse to the internal use of opium, and to opium injections, was necessary; he became weaker, and some wine was called for on the 3d; and late in the evening of the 4th he was attacked somewhat suddenly with partial stupor, such as Mr. Hawkins had spoken of in a recent clinical lecture, in connexion with the subject of diseased kidney. The next day, some fulness being apparent in the perineum, a lancet was plunged in deeply, on the chance of finding an abscess in this situation. This fulness, however, was only the consequence of the violence with which he had been accustomed to grasp these parts with his hands. In this state he lay for nearly thirty hours, when death took place, about a week only after his admission.

The bladder, which was thickened, was found closely contracted around a large stone, about three and a half inches long, and one and a half broad, the end of which, near the fundus, was round, while the other end, resting against the prostate, was more pointed, and rough; and all round this end of the stone the inner surface of the bladder was slightly ulcerated, and deprived of its mucous surface, from whence, no doubt, the intolerable pain felt by the boy had arisen, as the rest of the bladder was not much inflamed. The external surface of the stone was evidently phosphatic and porous; and a section, which we saw, exhibited a beautiful appearance of an oval stone, hard and condensed, and composed of numerous layers of lithic acid and lithate of ammonia, with apparently a mulberry nucleus, having a central perforation in which some lithic acid had entered. Around this stone, which was of the size of a large walnut, a great mass of the phosphates had been deposited; but it was curious, that while there was a thin layer only around the sides of this stone, the two ends had a great quantity, giving the stone its elongated form, the phosphatic layer at the fundus being nearly half an inch, and that at the apex about an inch in thickness, as if the lateral contraction of the bladder had prevented its accumulation at the sides of the calculus, after the phosphatic diathesis had succeeded the acid formation. The ureter on both sides was very much enlarged, and both kidneys and the infundibula were distended to a very great degree; and the cortical substance much expanded around these cavities. In the right kidney, the contents of the large membranous bag formed by this dilatation were clear and healthy urine; while, on the left side, half of the fluid was a thick pus, which a little pressure on the kidney made to flow through the bladder and urethra. It is remarkable, too, that in the cellular texture around the left kidney, some recent purulent lymph had been deposited, perhaps about the time the stupor came on; and one little piece of lymph, of the same kind, was also found in the peritoneum, without any sign of general inflammation.

WESTMINSTER HOSPITAL.

To the Editor of the Medical Gazette.

Sir,

Should you deem the following cases and remarks worthy the pages of your valuable journal, an early insertion would oblige,

Yours obediently,

Jesse Leach,

Clinical Assistant.

August 24, 1834.

Bark and Colchicum in Rheumatism.

Few maladies have produced such diversity of opinion, or originated so many remedies, as rheumatism; and yet some of these remedial agents, particularly the two most active, viz. bark and colchi-

cum, are diametrically opposite in their action to each other, so that what would be indicated and proper in one form of the complaint, would be a highly dangerous remedy in another. This state of things calls for an impartial inquiry into the symptoms and cases which would justify the use of either colchicum or bark, as each has its separate merits.

In the last week's Gazette I gave a general summary of symptoms which indicated the use of bark; and it is only necessary here to notice, that the same train of symptoms are present, and that the pains are erratic, following the course of the muscles; and the parts thereby affected are usually unattended by increased heat and swelling, and no accompanying fever. Colchicum operates on the kidneys, bowels, and capillary system; and its only precedence over the other drugs of the Materia Medica, is its possessing the power of unloading the turgid state of the capillary system, without great expense to the general strength. In all states of the system, whether they arise from organic or functional causes, provided the following symptoms be present, colchicum is useful: the surface hot and dry; constitution strong and vigorous; pulse strong, thrilling and bounding; bowels costive, and other functions partially suspended. Its action may be either through the medium of the nervous system, without affecting the bowels; or, as in the great majority of cases, by its first operating on the bowels and kidneys, and afterwards an improvement of the disease. Occasionally cases occur where its use is necessary, and yet circumstances exist which render its employment a dangerous expedient. In these cases the physicians of this institution combine it with some preparation of opium, according to existing emergencies. If the bowels be much relaxed, Dover's powder is a good substitute; and as rheumatism is generally attended with a rancid smell and acid secretions, carbonate of soda, with a mild laxative, as rhubarb, materially assists its action. When the system is full, and admits of much depression, the wine of the bulb of colchicum is the most proper form, which has more drastic properties than that of the seeds, which have a decided preference when the constitution will not admit of that depression which is the invariable effect of the wine of the bulb. Colchicum has the same disposition to accumulate in the system as digitalis, and without any precursory signs shews its virulent effects, so that its use ought to be closely watched, and given in small and gradually increased doses; but if largely given, care ought to be taken not to depress the system by copious bleeding and warm bath; but before this drug be given, where circumstances will admit, its employment ought to be preceded by a venesection and a warm bath, which render its action more speedy.

The following case is one which is still in the hospital, and which serves to exemplify the use of bark:—

Case I.—*Bark employed with success.*

John Smith, ætat. 50, shoemaker, admitted into the hospital under Dr. Roe, on August the 14th, 1834. Frame emaciated, and of blanched appearance; veins of the upper extremity large, bluish, and of a sluggish character; pain and tumefaction of the wrists, knees, and ankles, which are sometimes benefitted by warmth, at other times by cold; heat on the surface of his body natural, and bedewed with perspiration. Bowels naturally costive; tongue clean, moist, and tremulous; pulse 96, quick and soft. He states about five weeks since he was exposed to much wet and cold, neglected suitable raiment and necessary food, which was followed by general rheumatic fever, for which he underwent medical treatment without an entire subsidence of disease.

Middle diet.

℞ Quinæ Sulph., Ferri Sulph. aa. grs. viij.; Acid. Sulph. ♏xv.; Mist. Camph. ℥viij. ℥j. 4tis horis.

Pulv. Cath. st.

Aug. 15th.—His bowels are freely open; a great diminution of the pains in the knees and ankles, but the left wrist and synovial membranes of the phalanges are more painful, and with an indolent swelling; he expresses a desire for more food.

Rice pudding.

Ext. Conii, gr. xx. pro cataplas. carpo sinist.

16th.—The man's general aspect is improving; pulse 84, quick and soft; appetite good. Carpal pains much abated; passes sleepless nights; bowels freely open.

Pulv. Ipecac. c. gr. x. o. n.

17th.—The whole pain is now centred in the left wrist, but much relieved since the 15th.

18th.—His tongue is clean and less tremulous; digestive apparatus in good order; appearance improving, and less pain. Pulse 82, fuller and soft.

19th and 20th.—He suffers no inconvenience except occasionally pain in the left wrist, which is of a puffy swollen look, but unattended by increased heat or redness.—Convalescent.

Dr. Roe in his clinical lecture remarked, that when the rheumatism is confined to one joint, and a lancinating pain expe-

rienced thereby, he has often found the use of a cataplasm of conium and bread, in the quantity of ℈j. to the bread applied to the part, productive of great relief; and that the best way of removing the local pain and swelling in this case was correcting the debilitated state of the constitution.

The following case is a good contrast to the one given, both in age, strength, and appearance, and requiring an opposite line of practice.

CASE II.—*Employment of Colchicum.*

John Cocks, tailor, æt. 24, admitted under Dr. Roe, on August the 14th, 1834, of a healthy and well-developed muscular frame. The hands, knees, and ankles, are affected with heat, pain, and tumefaction, which are relieved by warmth; the surface of his body is hot and dry; the tongue is white and yellow; nausea, thirst, and aversion to food; bowels open; pulse 64, full and soft. He states, whilst drinking a fortnight ago, he was exposed to much wet, and neglected changing his clothing; the consequence was, rheumatic pains in the shoulders, which were succeeded by the present symptoms.

Low diet.
Vin. Colch. ℥iv.; Sodæ Carb. ℥ij.;
Mist. Camph. ℥viij. Sum. ℥i. ter die.
Pulv. Ipec. c. gr. x. o. n.

August 15th.—His bowels are much relaxed this morning, with a great diminution of heat, pain, and swelling. He sweats freely; tongue moist, and clearing of the yellow scurf; pulse the same, and he expresses a desire for more food.

Omit. Mist. Col.
Rice pudding.
Pulv. Ipec. c. gr. v. ter die.

16th.—His aspect is better; his skin less arid, and bedewed with greater perspiration; tongue clean and moist; bowels continue freely open.

Perst P. Ipec. c.

17th.—The looseness of the bowels is stayed, and the rheumatism now confined to the carpal joints; urine more copious; pulse the same.

19th.—The rheumatic swelling is the same; bowels are confined; pulse 60, full and compressible; and the man is greatly improved since he entered the hospital, and may be considered convalescent.

Cap. Pulv. Rhei. c. Sod. Carb.

Dr. Roe remarked, that when the bowels are relaxed, the colchicum ought always to be discontinued for a time, and Dover's powder substituted.

The following case shows the propriety of treating the low typhoid state, with delirium and marked arachnitis, with mercury. Though Dr. Elliotson is a warm advocate of mercury in this form of the disease, I am not aware that he has stated what the pathological conditions generally depend on. A case precisely similar to the following occurred in this institution about three weeks ago, which was treated without mercury: the patient died; and on a post-mortem, which I performed, there was vascularity, effused lymph, and serum, with thickening of the arachnoid membrane, and all the other viscera were healthy. That the calomel and opium are not so active as the proto-ioduret of mercury, will be seen by the case; and that the fact of mercury being introduced along with iodine would favour the idea of its in a great measure counteracting any scrofulous diathesis which might be present, and which, when present, forms such a principal objection to the exhibition of mercury: but generally speaking, where this tendency is present, the cases favour the Bronssaian system, the head being then not the part affected.

CASE III.—*Acute Rheumatism, with Delirium and marked Arachnitis.*

John Collington, æt. 20, labourer, admitted under Dr. Bright, on August 4th, 1834. Countenance flushed, with a low typhoid stare; a tendency to dose, which is interrupted by a low muttering delirium; much agitation; congested state of the eye, with contraction, and rather insensible pupil; surface hot, dry, and alternating with paroxysms of chilliness; tongue dry and brown, and lips covered with a darkish brown scaly cuticle; very thirsty; bowels regular; abdomen sunken, and not tender on pressure; breath very offensive; pulse 102, quick and soft; urine scanty and high coloured. His friends say he has been ill ten days, being first ushered in by nausea and head-ache, exposure to wet, and inattention to regular diet.

Hirudines xxiv. temporibus.
℞ Calomel, grs. iij.; Opii. Pulv. gr. ss.;
Conf.-Arom. q. s. Cap. 2nda horâ.
Acid Muriat. ℥i.; ad aquæ Oj. pro potu.

August 6th.—There appears no manifest amendment in the symptoms. His bowels have been moved twice since admission.

Head shaved.
Bladder filled with ice, to be kept constantly applied to the head.

7th.—His thirst is a little abated; in constant delirium and agitation.

Omit. Pil. Cal.
℞ Proto-Iod. Hydr. gr. ij. ter. horis.

8th.—There is no evidence of salivation, and the system bears the dose without inconvenience.

9th & 10th.—The gums are rather spongy, and there is a slight moisture on the circumference of the tongue, with a slight diminution of delirium, restlessness, and morbid heat. Bowels continue freely open.—Perstet.

11th.—The tongue is raw at the edges; abdomen tender on pressure; a tendency to diarrhœa, with a greater diminution of delirium and agitation. His skin is moist, and more of the natural temperature; increased flow of urine; pulse 98, quick and soft.

Omit. Iod. Hydr.; Hirudines xxiv. Epigas.; Mist. Efferves. ℥j; Ant. Tart. gr. ⅛ 4tis. horis.
Inject. Amyli. c. Tr. Opii. m. lx.

14th.—The diarrhœa, tenderness on pressure, delirium, and agitation, are entirely gone. His eyes, which were suffused, and pupils contracted, are now glassy; the pupils fully dilated. Heat of the natural temperature, and gently bedewed with sweat. Tongue clean; gums spongy, with increased flow of saliva and coppery taste. Bowels regular; pulse 96, and soft.

18th.—His countenance is now more expressive of tranquillity of mind. The body of a natural temperature, and of a gentle perspiration. His urinary secretions are increased in quantity and improved in character, with furfuraceous depositions. The pulse assumes a more even and softer beat—94 per minute. Bowels regular; teeth ache, from the effects of salivation.

19th.—The man is perfectly rational, and sat up this morning; and with the exception of debility and a slight cough, caught by tossing the bed-clothes off him during sleep, he is suffering a progressive convalescence.

Case of Dropsy, relieved by Acupuncture.

Wm. Brown, æt. 60 (shepherd), admitted under Dr. Bright, on August the 6th, 1834; of a short stumpy stature and ruddy complexion; enjoyed very good health till ten years ago, when he suffered a severe laceration of his right hand, from incautiously exposing it to a chaffing machine; which disabled the part so much as to require amputation, from which he suffered a slow recovery (four months): the consequence was anasarca of the lower extremities, with a diminution of the urinary excretion. The left foot became so prodigiously swollen, that, in the course of three years, ulceration of the integuments took place, giving exit to the serum, and leaving a foul ulcer behind, which thoroughly drained the system. In the course of fifteen months, being tired of the ulcer and discharge, he placed himself under a respectable practitioner, who healed it; and the consequence was his present symptoms. His legs and thighs are greatly swollen, pitting deeply upon pressure, with ascites and scanty flow of urine. The left lobe of his liver is greatly enlarged; his bowels are naturally torpid, and stools defective of bile. His breathing is much oppressed, and he cannot recline in the recumbent position. He has a troublesome asthmatic cough, with a sparing bronchial expectoration; all which symptoms are aggravated towards the evening. There is dullness on percussion from the clavicle downwards; duller on the left than right side, and more particularly so in each axilla, where it has a peculiar leaden sound. The respiratory sound is audible throughout, but at a distance, and more particularly so in the left axilla, where it is attended with the moist râle crepitant; the heart is heard rather to the right of the sternum, but scarcely audible, and intermittent. The chest is well formed, and intercostal spaces convex; digestive organs unimpaired. Pulse 60 per minute, scarcely perceptible, and intermittent.

Low diet. Pil. Hyd. Pil. Scillæ, a. a. grs. iij. ter die sumend. Pot. Supertar. 3j. Pot. Nit. ℈ss. Aquæ, ℥xvj. quotidie sumend.

August 8th.—The symptoms are not improved. Sir G. Tuthill suggested the use of acupuncture needles to the calf of the left leg, which was the most swollen, and a bran poultice to be applied afterwards.

Adde Potui Oxymel Scillæ, ℥ss.; Syrup. Papav. ℥ss.

9th.—The legs, which were prodigiously swollen, have discharged much by the punctures, with great relief to the pectoral symptoms. The cough, dyspnœa, and oppression at his chest, are much improved, and he states having had more water.

19th.—The man is surprisingly improved, being enabled to walk in the pleasure-ground, passes good nights, enabled to lie on either side, or for a short time in the recumbent position, without dyspnœa. His pulse is still tolerably small and intermittent; his digestive organs are in excellent order, and the man complains of suffering no inconvenience. Convalescing.

Remarks.—This case exemplifies that nature points out many useful resources, and that, though ulcers do exist, and are an inconvenience under certain circumstances, they are best treated either by some palliative measure or left entirely to nature; and, secondly, it shews that the

absorbents will not act under certain pressures, but when partially unloaded of their superincumbent weights, they take upon themselves an increased action.

The use of the needles has preference over large incisions, on account of the tendency to erysipelas which the latter are found to have in the confined and mephitic air of an hospital, whilst the former are innocuous in effect; and provided they answer no other purpose but unloading the pent-up serum, and thereby causing the absorbents to act more speedily, they relieve the parts which would necessarily otherwise fall into a state of mortification, &c. and give temporary relief to urgent symptoms.

A warm bran-poultice applied to the parts, favours the discharge of serum, and soothes the irritation caused by the punctures. The punctures generally heal in two days, when it becomes a question whether to re-apply the needles, or, if the kidneys have taken on an increased action by the use derived from the former punctures, to allow the increased excretion, "viis naturalibus," to drain the system, assisted, according to contingent circumstances, by other diuretics. In the preceding case there is no doubt that there was dropsy of the left lung, both pleuræ, and probably the pericardium—which I think exists still, although he feels no inconvenience by his symptoms—ascites and anasarca; and nothing gave relief till the needles were used.

EXTRA-UTERINE PREGNANCY,

Terminating favourably after Twenty-one Months' Duration.

By Dr. M. B. Hanius, of Strelitz.

The patient, Mad. C., æt. 26, a healthy middle-sized female, of a vigorous mind, who had been married five years, and had one child three years ago after a natural labour, became pregnant about January 24th, 1830, and was soon after attacked, whilst at the water-closet, with a very acute pain in the pelvic region, which recurred each time the bowels were moved, and alternated with a feeling of internal uneasiness. She had recourse to different postures with but little benefit; the horizontal position in bed alone seemed to afford relief. Mad. C. thought it might be the result of cold, especially as, after three months' continuance, it seemed to subside on the approach of warm weather. It returned, however, very severely. Some fears of abortion were created by the discharge of a small quantity of blood from the vagina.

This painful state lasted until the end of the fifth month. On the 24th of June, at 6 A.M., she was attacked with acute pain in the lower belly, more severe than before, which was followed in a few hours by slight hæmorrhage, vomiting, universal shivering, forcing efforts to pass urine and fæces, with pallor and coldness of the face. Some antispasmodics were administered in a clyster, and the patient obtained relief; instead of the acute pain she felt a forcing down, as though the contents of the abdomen would fall out, which obliged her to remain constantly in bed, or on a sofa; this was the more tiresome as she could only lie on the right side (when the head of the child could be felt) with any comfort, any other position occasioning severe pain.

The dysuria continued long; the bowels could only be kept free by clysters, and each motion caused pain, and the appetite was very small. The nervous system was so much shook that she remained in a very excitable state, subject to hysterical attacks. In this state she continued up to the end of the ninth month.

As to the child, it was impossible to ascertain whether it lived after the first attack, as Dr. H. could never discover any movement on examination, and the patient had only observed a very slight sensation of that kind twice.

From an attentive consideration of the symptoms already enumerated, Dr. Hanius diagnosticated an abdominal extra-uterine pregnancy.

About the latter end of October (the normal termination of her pregnancy) she was attacked by uterine hæmorrhage, followed by a discharge of water from the vagina, "which," says the author, "would have been mistaken for the liquor amnii by one ignorant of midwifery." She suffered no pain, and there were no uterine contractions.

After this, regular lochia were discharged, and in two or three days the breasts became turgid; milk was secreted, and she had some fever, terminating in sweating. She recovered gradually, without accident, and left her bed the fifth week, looking delicate, but healthy. In fact, she felt so well, that she came to the conclusion that she had not been pregnant, but merely suffered from excessive menstruation. The catamenia recurred in December following, and continued regular for four months. At each period a considerable quantity of water was discharged along with the menses. Thus she continued until March, when she was attacked with continued

fever, which lasted some time, but was relieved by the means employed.

The head of the fœtus, which had formerly been discovered in the right iliac region, now moved anteriorly, and became fixed in the pubic region, forming a tumor which pressed downwards into the vagina in front of the uterus, the orifice of which organ was discovered posteriorly. The tumor gradually diminished, and passed over to the left side, where it could only be discovered on minute examination.

In the beginning of July she had diarrhœa, which lasted some time; and along with the fæces there passed into the "commode" something in weight and consistence resembling laudable pus. This was succeeded, in a few days, by a discharge of pure blood; and the day after one bone of a phalanx was found in the night chair. This was discovered to have passed through a perforation in the coat of the rectum, exceedingly small, and only just within reach of the finger. Through the same opening there passed, at different times, both the femurs, the ossa ilia, the scapulæ, the clavicles, an ulna, five ribs, and some of the bones of the wrist and ankle, and some bones of the cranium. Many others were doubtless passed with the fæces and lost. The patient recovered *.

RUPTURE OF THE BLADDER NOT IMMEDIATELY MORTAL.

A STRONG constitutioned man, aged 30, had been drinking white wine nearly a whole day in a tavern, without attending to the state of his bladder. On coming out he quarrelled with his comrade, who knocked him down, and gave him a kick over the pubes. He instantly felt violent pain. He was carried to the Hôtel Dieu, and bled; leeches were applied over the hypogastric region; fever set in. Although the bladder did not appear distended, M. Dupuytren sounded it to evacuate its contents; for, from the history of the case, rupture of the bladder was suspected. There came away only a small quantity of turbid reddish urine. In directing the beak of the catheter in various directions, M. D. thought that in one point, corresponding to the antero-superior paries of the bladder, it penetrated far more deeply. The rupture was supposed to have occurred in that point; and, to confirm the diagnosis, the sound gave exit to a new quantity of fluid like the former. There was evidently extravasation of urine into the abdominal cavity. Leeches were again applied over the pubes, with emollient poultices; a small quantity of bland drinks was allowed, and strict regimen. Under this treatment the inflammation did not appear to extend beyond the region of the bladder; pain was not felt in any other part of the abdomen. For four days he was in a very dangerous state. On the fifth day the pain and tension of the hypogastrium diminished; the pulse was less frequent, and fuller; the skin softer, cooler, and inclined to a gentle diaphoresis; appetite returned. But notwithstanding the restrictions in regard to diet, the patient ate a quantity of bread and sweat-meats, drank some wine, and in the course of the seventh day he sunk. On dissection, forty-eight hours after death, adhesions of cellular membrane, forming very firm bands, were found between the parietes of the abdomen, the lateral parts of the bladder, and the other viscera contained in the cavity of the lesser pelvis. Other bands formed a pouch behind the bladder, containing a turbid urinous fluid, mixed with flocculi of albumen. The bladder was torn to the extent of two inches at the postero-superior part, and in the direction of its greatest diameter. On beholding the admirable provision of nature to establish a new reservoir for the urine, and seeing the advancement of its organization, every one must agree with M. Dupuytren, that death was caused by indigestion, consequent on the greedy and gluttonous appetite of the patient, rather than by the rupture of the bladder and the extravasation of urine. The stomach and small intestines presented some patches of engorgement of their vessels. There was only a slight effusion of serum at the base of the brain *.

* Hufeland's Journal and Dublin Journal.

CASE OF AMNESIA CURED BY LOCAL DEPLETION.

By Dr. Zabriskie.

PATRICK HART, æt. 45, of a thin spare habit, phlegmatic temperament, who had been very intemperate, had been troubled for a long time with loss of memory, frequently forgetting the most common things occurring to him. He would often leave part of his clothes in the field where he was at work, or on the road, and could not remember where he had left them; in conversation he would repeat the same thing

* Archiv. Generales, June 1834.

several times, and would often forget what was told him, so that frequently he would appear almost like an idiot. As he complained of pain and distress in his head, cups were applied to the temples, which relieved the pain. About three weeks after this he again came to me, stating that his memory had been much better since he had been cupped, and requesting that this might be repeated. It was accordingly; and since this, he has frequently told me that he could remember as well as ever he did; and his companions have also assured me that his memory was restored*.

REPRODUCTION OF THE EEL.

It has been commonly supposed that eels are viviparous; but Mr. Yarrell, who read a paper on this subject before the British Association at Cambridge, thinks otherwise. He states his belief that the common opinion has been owing to the fact that eels are subject to numerous intestinal worms, three species of which he describes as of frequent occurrence. The sexes are distinct in eels, and the females are oviparous. They are migratory; at least, in tide rivers, they extend their migrations as far as the brackish water: this takes place in autumn. In winter they lie imbedded in the mud. The restlessness of eels during thunder-storms, when quantities of them are caught, seems to be owing to the high degree of muscular irritability that exists in animals which have a low degree of respiration. Fishes that swim near the surface die soon when taken from the water, having a higher degree of respiration, and less muscular irritability, than those that swim near the bottom; and *vice versâ*.

HABITS OF THE SPIDER.

Mr. Blackwall (Report of British Association, 1833) says that in attempting to drown a small spider (*Erigone atra*), for the purpose of taking its dimensions accurately by measurement, he was astonished to find that, at the expiration of two days, though it had remained under water the whole of the time, it was as lively and vigorous as ever. This extraordinary circumstance induced him to submerge numerous specimens of both sexes in cold water, contained in a glass vessel with perpendicular sides, on the 21st October, 1832, in which situation they continued till 22d November following, an interval of 768 hours, without having their vital energies suspended.

* American Journ. of Med. Sciences.

WEEKLY ACCOUNT of BURIALS,

From Bills of Mortality, *Sept.* 2, 1834.

Age and Debility	40	Heart, diseased	1
Apoplexy	4	Hooping-Cough	7
Asthma	8	Inflammation	19
Cancer	1	Bowels & Stomach	11
Childbirth	4	Brain	8
Cholera	73	Lungs and Pleura	4
Consumption	47	Insanity	1
Convulsions	34	Jaundice	2
Croup	1	Liver, diseased	2
Dentition or Teething	6	Measles	3
Diarrhœa	2	Mortification	7
Dropsy	14	Paralysis	2
Dropsy on the Brain	11	Small-Pox	3
Epilepsy	1	Sore Throat and Quinsey	1
Fever	7	Thrush	2
Fever, Scarlet	7		
Fever, Typhus	1	Stillborn	11
Gout	1		
Hæmorrhage	1		

Decrease of Burials, as compared with the preceding week . . . 210

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

Aug. 1834.	Thermometer.		Barometer.	
Thursday . 21	from 56	to 76	29 67	to 29·79
Friday . . 22	58	75	29 85	29 86
Saturday . 23	51	70	29·80	29·70
Sunday . . 24	46	63	29 57	29·51
Monday . . 25	41	61	29 50	29·54
Tuesday . . 26	40	63	29 61	29 63
Wednesday 27	45	64	29 65	29 72

Prevailing wind, S.W.

The three first days generally clear, frequent heavy showers during the 24th and 25th; a thunder storm on the afternoon of the 26th; the 27th generally clear.

Rain fallen, ·95 of an inch.

Thursday . 28	from 46	to 64	29·77	to 29·70
Friday. . . 29	47	65	29·64	29 60
Saturday . 30	4)	67	29·55	29·59
Sunday . . 31	49	68	29·67	29·73
Sept.				
Monday . . 1	51	66	29·79	29 83
Tuesday . 2	54	67	29 88	29·93
Wednesday 3	51	65	29 97	29·99

Wind, S. by E. on the 28th; since, S.W.

Generally cloudy, with showers on each day; frequent intervals of sun-shine.

Rain fallen, 1 inch and ·125 of an inch.

Charles Henry Adams.

NOTICES.

We will not give insertion to any letter from an anonymous *soidisant* "Governor" of Christ's Hospital. Our correspondents on such subjects are not afraid to give their names.

T. D.—The letter has been received, and will be attended to.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, SEPTEMBER 13, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XLIX.

COMPLEX LABOURS.

CONVULSIONS *(continued.)*

Treatment.—Our first duty, on the accession of a fit, should be to protect the patient from injuring herself by the violence of her struggles; and then to endeavour to prevent a recurrence of the paroxysm. With the first intention, one or two strong assistants should restrain her, so as to preclude the possibility of her throwing herself off the bed, and striking her head or arms against any hard body. The lower jaw must be depressed, and a piece of firewood (as good an instrument as can be used, and which is generally at hand), wrapped round either with a handkerchief or some other soft material, inserted between the molar teeth, and kept constantly in its place by an assistant: if allowed to slip out for a minute, during the acmè of a paroxysm, the jaws may be violently closed and extensive injury sustained. I have many times known the tongue so swollen by inflammation, consequent on laceration, that the teeth could not be brought together for some days.

Means must next be taken to relieve the patient effectually. Believing that the cause most commonly consists in pressure to which the cerebral mass is subjected, the same treatment must be adopted that we would have recourse to under ordinary apoplexy—viz. the abstraction of blood, and acting briskly on the intestinal canal. Bleeding is our great reliance—the lancet is our sheet-anchor—and blood may be taken to a very large extent; forty, fifty, or sixty ounces, nay even more, may require to be taken in the course of a very few hours. If ten or twelve ounces only be abstracted, the patient seldom obtains much benefit; depletion will avail us little unless a decided impression be made on the system generally. We observe that a patient will bear the loss of a larger quantity of blood under apoplexy, as well as puerperal convulsions, without fainting being produced, than in almost any other affection. Venesection, however, had better not be attempted during the violence of the paroxysm, for the struggles of the patient will most likely prevent its being properly and beneficially performed. We may content ourselves with guarding her as perfectly as possible until the fit subsides; and when it has passed over, and the patient lies in a state of coma, or consciousness is somewhat returning, the operation will be easy. Her head and shoulders should be raised as high as conveniently may be, a free opening made in one or both arms, and the blood allowed to run in a full stream. At first it will probably flow sluggishly, and dark in colour; afterwards it will come more freely, and of a more natural appearance; and it should not be restrained until a sensible effect be made upon the pulse, or commencing pallor of the lips indicate approaching faintness. The probability is, that from twenty to thirty ounces will be abstracted before this effect is produced. The quantity, within a certain moderation, should not be regarded; graduated vessels in such a case, to measure the loss by, are not required; any thing nearest at hand will serve our purpose equally well.

Our next indication is to procure copious evacuations from the bowels as early as possible. If the patient be sensible, there will be no difficulty in administering medicine for this object by the mouth; but if she remains still under coma, she

may perhaps be unable to swallow. An attempt may, nevertheless, be made to get some cathartic into the stomach; and with this view, ten or twelve grains of calomel may be mixed with a little sugar, and put upon the tongue; and a tablespoonful of infusion of senna and jalap may be exhibited every half hour, till stools are produced. The probability is that some part will pass down: for in most instances, if we watch the proper opportunity, deglutition may be accomplished. In case, however, this cannot be effected, a strong purgative enema may be injected, and repeated if necessary. By some, emetics have been recommended; but unless there were indications of the stomach containing undigested food in some quantity, I think emetics not called for. Though the patient may be relieved for a time, we are not to expect an immediate cessation of the fits; a fresh attack will most likely occur, moderated or not in intensity, according to circumstances; for which, after the lapse of a short time, another bleeding may be required; nor should we hesitate to have recourse to the lancet a second or even a third time, if the arterial system regains its power. As an auxiliary of no mean consideration, the hair may be taken off, and cold applied to the head; and the shoulders should be kept in an elevated position. Both Gooch and Blundell speak of the advantage sometimes derived from drawing the woman's person partly over the edge of the bed, and pouring water unsparingly on the head.

I think it useless, while the violence of the convulsions lasts, to attempt the application of cupping-glasses to the back of the neck, or even leeches to the temples; or to blister the shaved head, or nape of the neck. The contortions of the patient's body would prevent the glasses being fixed, and there would be a great chance of their being broken even if properly applied. It would certainly be less difficult to apply leeches, but they are too slow in acting for our present purpose, and the urgency of the case requires more prompt and effectual means. The same disadvantages attach to blisters even in an increased degree. Not that I object to local depletion; it is certainly desirable to relieve the vessels of the brain by any method in our possession; but general bleeding is far preferable to the less powerful resources. Provided, then, the symptoms are but little alleviated, while delivery is impossible, or would be attended with much hazard, we may open the temporal artery, or (what I think better) the jugular vein; and thus secure the advantages both of general and local depletion at the same time.

But we may be fearful of taking any more blood either from the arm or nearer to the seat of distress, while yet the convulsive fits continue unabated in their severity: under such a case, we have only one other resource—delivery, if it can be effected. Emptying the uterus will usually put a stop to the fits at any rate for a time; and if there be no permanent injury done to the brain, it will generally mitigate them most materially. According to the progress the labour has made, must be the means we employ for this object. Thus, if the fœtal head be down in the pelvis, so that we can feel an ear, we may have recourse to the short forceps; if it be not within their scope, the long forceps may be employed; and if it remain entirely above the brim, we may be driven to the use of the perforator. Again: should the membranes be unbroken, we may turn the child and deliver by the feet. Of all these means, we should much prefer delivery by the forceps, if it could be effected without injury; but, unfortunately, the operation is rendered very difficult, and in no small degree hazardous, by the violent contortions accompanying each fit, and the incessant movements of the person in the interval of the paroxysms, which is mostly present when delivery is required. Although, then, the alternative offered by craniotomy is painful to contemplate, we should have recourse to it rather than run the risk of inflicting extensive injury on the mother's person. The child, indeed, as I have just said, is frequently born still, after the mother has suffered from convulsions; but the chance of its previous death would not warrant us in taking the perforator in hand if delivery could be accomplished safely by any other means.

Neither is the operation of turning under convulsions free from objections. It would be most unwise to attempt its performance if the head were engaged in the brim of the pelvis—if the membranes had been ruptured for some hours, and the uterus was strongly contracted round the child's body; because of the difficulty we must experience, and the danger we must necessarily encounter. Nor would it be judicious to attempt the forcible dilatation of the os uteri by the hand; especially if it be rigid. Bearing in mind that the remote cause probably exists in the uterus, and that the fits owe their origin to irritation propagated from that organ to the brain, we should be most cautious not to add another source of irritation by our manual endeavours. Under such a state of things, bleeding should be carried to its fullest extent, rather than delivery be attempted. If, indeed, the mouth of the womb be open and flaccid, offering little or no resistance to the passage of the hand—if the membranes be still entire,

and delivery became necessary—turning might be undertaken with every prospect of a happy termination.

Even the emptying of the uterus, however, does not always put a period to the fits; though they generally become less violent when the labour is perfected. If they continue equally as strong after the birth as before, whether the delivery has been natural or artificial, I should suspect that some lesion had taken place within the brain, and should look upon the case as almost hopeless. Still, a continuance of the same means may be used, in a modified degree; leeches, cupping (if it can be accomplished), and blisters, may now be had recourse to, or mustard cataplasms to the feet or calves of the legs, in conjunction with cold applications to the head, and a continuance of purgative medicines by the mouth, or in enema. The same plan will also be found efficacious in relieving the violent headache, which often remains for some days after a convulsive attack; and which gradually disappears under such treatment, aided by perfect quietude, a darkened apartment, elevated position of the upper part of the trunk, and the sparest diet consistent with the due performance of the various functions of the body. When recovery takes place it is mostly perfect, gradually brought about, and no trace remains of the serious attack the patient has suffered. Merriman, indeed, mentions having known two or three instances of mania occurring as soon as the convulsions ceased, and remaining for some weeks, yet the patients ultimately got well; and another of true chronic epilepsy, which continued for some years, until the woman died of pulmonary disease. In one case that I attended, furious mania came on on the ninth day after delivery, and continued for a week: the woman subsequently perfectly recovered. Chronic epilepsy has not happened as a sequela of puerperal convulsions under my own observation; but I have known one instance in which the fits appeared in three successive pregnancies. Dewees mentions a case in which the third and fifth labours were attended with convulsions, as well as the first; and he attributes the return to neglect of proper management during the last weeks of pregnancy. Both Perfect and Portal, also, as well as Baudelocque and Capuron, have put instances on record, of convulsions attacking the same patient in subsequent labours.

The contrast between the fatality of the cases now met with, and those put on record by Saviard and others in the seventeenth century, and Smellie, Perfect, and Spence, in the last, cannot but be a subject of high gratulation to the practitioners of the present day. Hunter used to say, that more than one-half of the patients attacked with this disease died. From Parr's Medical Dictionary we learn that six out of ten of the cases terminated fatally. Jacobs tells us that none recovered; and in a small work, "The Clinical Guide," published by Dr. Nisbet in 1800,—a book not generally known, though containing many highly useful practical remarks,—we read, that when coma accompanies the fits, the disease "generally, though not always, proves fatal."

Few, comparatively, under good care, now terminate unfortunately; and the favourable results are to be attributed to the extent to which bleeding and other evacuant means are carried. Gooch used to say that he never had lost a patient under convulsions, when free bleeding had been practised; but that all the women who had died under his observation had been bled insufficiently. Eight, ten, or twelve ounces of blood used to be considered as much as it was safe to abstract; and the principal reliance was placed on antispasmodic and *nervous* remedies, as they were called, consisting principally of æther, ammonia, camphor, musk, castor, and opium. Such medicines, as being stimuli, must add to the danger, by increasing the power of the circulating organs, and throwing more blood on the already over-loaded brain; and all, especially opium, seem perfectly inadmissible during the continuance of the convulsive paroxysms.

I perfectly agree with Professor Burns that "opium is highly dangerous," and, indeed, "to be avoided, as the most fatal agent that can be employed." The whole of the chapter on convulsions in his work I would recommend to your most attentive perusal.

My father was one of the first practitioners in London who strongly insisted on the depleting practice being carried to the extent now almost universally adopted; and the advantage of this system is acknowledged by every practical man. Some of the earlier French authors, however, were strong advocates for the advantage of large bleeding, and Puzos particularly insists on its necessity. The accidental loosening of the bandage tied round the arm after venesection, seemed to give Dr. Denman the first idea of the value of larger losses of blood than were at that time practised. Highly valuable, however, as the lancet is under such a state, it may still be abused: rashness must be deprecated here as well as in other diseases; and no more blood should be taken than is sufficient to produce the effect desired, whatever the quantity may be.

We should be prepared to expect that a patient, after having suffered a convulsive

seizure, would have no remembrance of any thing that occurred subsequently to the attack until the time that she regained her sensibility; and we not only find this to be the case, but the disease seems frequently to wipe away all recollection of events that happened some time before the accession of the fits, while she was perfectly conscious. Thus I have known more than one instance of a woman apparently well when delivered, who (having become the subject of convulsions a few hours after) on the restoration of her faculties, had no recollection of her labour, and was only convinced that she was delivered by her child being brought to her. My father mentions a case in which, "although the lady at the time of her delivery appeared in perfect health, she had no recollection whatever, after her recovery, of the occurrences during her labour, or indeed of those of *some days* preceding that event: they appeared a blank in her existence." Blindness and deafness, continuing for some days, are no uncommon consequences of convulsions. Denman mentions, that in almost every case which he had seen, there was evidently, after delivery, a greater or less degree of abdominal inflammation. Gooch also states, that peritonitis often follows puerperal convulsions. I cannot say that I have made the same remark.

Of forty-three women whom I have myself seen under puerperal convulsions, in ten cases the fits attacked the patient before labour pains came on; in twenty-four, during labour itself; and in nine, after the birth was perfected. Thirty of these women were delivered naturally; five by the forceps; three by craniotomy; four by turning; and one by the blunt hook. Thirty-six recovered perfectly; seven died: one was a twin case. Of the children, twenty-one were born living; twenty-three dead. About two cases out of three were first pregnancies. Of those who died, in one instance I found a large quantity of blood extravasated under the arachnoid membrane, which had escaped from a ruptured vessel in the pia mater, situated between the convolutions of the posterior lobe of the right hemisphere of the brain.

Hysterical convulsions.—Nervous and irritable women are liable occasionally during labour, but more particularly under pregnancy, to convulsive fits of a much less dangerous kind than that which I have just described, which seem not to originate in pressure sustained by the brain, and for the removal of which such violent remedies are not required. In these the spasmodic affection is confined to the muscles of the trunk and extremities, seldom affecting the face: there is not the same violent disturbance of the sensorium, nor the same turgescence of the vessels of the head, nor the same hideousness of aspect. There is a sensation of globus, palpitation of the heart, and a discharge of flatus on the termination of the fit. The muscles of the back seem to be the principal seat of spasm, so that the trunk is bent backwards, in the form of an arch;—a state of things mentioned both by Dewees and Burns, as strongly characteristic of the hysterical kind. Such cases frequently depend on irritation existing in the intestinal canal, and may generally be relieved by brisk purging, the dashing of cold water on the face, and warm frictions, or stimulating applications, to the stomach, abdomen, and back. Some of the medicines which I have just enumerated may be of service in this variety, and an assafœtida injection has sometimes at once cut short the disease. This latter remedy I have also occasionally found useful in removing the head-ache generally subsequent to an attack of true convulsions.

Apoplexy under labour, and during the last few weeks of pregnancy, unattended with convulsive action, is sometimes, though rarely, met with.

The symptoms are those characteristic of the same disease under ordinary states of system; nor does the case require any other than the common treatment. Both during the continuance of the convulsive paroxysms, as well as after their cessation, while the patient still remains in a state of imperfect consciousness, it is quite necessary that the bladder should be carefully attended to; as it may become inordinately distended, and perhaps serious mischief may ensue. Some, with La Motte, have even attributed an attack of convulsions to over-distention of this viscus, as a cause.

RUPTURE OF THE UTERUS.

Average frequency;—situation and direction of the rent.—Occasionally the uterus bursts, its structure gives way, and a rent is formed in its substance: an accident of the most formidable nature, and which, by far most generally, terminates fatally. Rupture of the uterus is certainly a very rare accident; but there can be no doubt both that it has often been the undetected cause of death, and also that, when known to the attendant, it has not unfrequently been concealed from mistaken feelings of policy. Burns states that its frequency has been calculated at 1 in 940 cases. Out of 33,348 cases, however, delivered by the midwives of the Royal Maternity Charity within the last thirteen years, partly under my father's superintendence, and partly under my own (when an occurrence of the kind could not have happened without our knowledge), we have only had eight instances of rupture of the uterus or vagina, being one in about 4168

labours. The rent may take place at any part of the uterine structure—the fundus, the body, the cervix, or the mouth, may give way. It varies also considerably in its direction, being sometimes longitudinal, at others transverse, or oblique;—specimens of each variety are before you. The vagina may be implicated or remain uninjured. The laceration may pass through the whole structure of the uterus, and involve both membranes, an extensive communication being made at once with the abdominal cavity; or the peritoneum may be lacerated, and the parenchyma only slightly torn;—or, again, a large rent may extend through the inner membrane and the parenchymatous structure, while the peritoneum continues entire, the blood which is effused being pent up below it, and not extravasated into the general cavity of the belly. I have seen instances of each of these kinds. It is most usual for the laceration to take place through all the structures at once. The rupture may be instantaneous or more gradual; a large rent, sufficient to allow the child to escape into the abdomen, may happen in an instant, or a small aperture may first be made, and gradually increased with each return of uterine contraction, until it has acquired a size sufficient to permit the passage of the whole fœtal body out of the uterine into the peritoneal cavity.

Causes.—Rupture of the uterus may be produced by the violence of the uterine efforts themselves—the viscus bursting under its own inordinate action;—or it may be the consequence of forcible and improperly-conducted attempts to turn, under a shoulder or other presentation; of which sad catastrophe I have unfortunately seen more than one instance;—or, again, it may be caused by instruments, in the hands of the ignorant, the careless, or the inconsiderately rash. I cannot think with La Motte and Levret, that the struggles of the child can ever occasion it.

Occurs to all kinds of patients, and at all periods of labour.—This accident may occur to women bearing a first or subsequent children; to the young, as well as those more advanced in life—to the plethoric and the debilitated—to the healthy and the ailing. But of the many cases to which I have been called, I have only known one instance in which it happened during a first labour. It may take place as well under a head, a breech, or a transverse presentation, and at any period of the labour. It has been known to happen at the very commencement of the process, when the os uteri had not acquired a dilatation equal to the diameter of a shilling. A preparation of ruptured uterus was once sent to my father, in which the child and membranes had passed into the peritoneal cavity before the os uteri could admit two fingers. Mr. Kite published a case, in 1795, of rupture of the uterus at seven months, occurring at the very commencement of labour: the case terminated favourably. Most likely, in both these instances, there was disease in the uterine structure—a softening or thinning of texture, for instance, consequent on inflammation; for we cannot suppose that the healthy womb would, by its own powers, lacerate its substance before the membranes of the ovum had given way; and while its mouth was undilated.

Laceration of the uterus is most likely to happen to a patient who has had three or four children, who possesses a slightly distorted pelvis, and who has been in strong labour a number of hours.

Although the rent may take place in any portion of the organ, its most frequent seat is at the neck, either at the posterior part, opposite the prominence of the sacrum, or anteriorly, behind the symphysis pubis. The direction is also mostly transverse, or slightly oblique. I think it is not difficult to account for this being the most usual situation of the injury; for since, during the latter part of gestation, the womb rests upon the pelvic brim, if the promontory of the sacrum dip too far forward, or the ridge of the pubes be preternaturally sharp, it is reasonable to suppose that the uterine structure may be affected, that inflammation may occur as a consequence of pressure, and that a thinning or softening of the substance may be induced; and under these circumstances, should the structure give way at all, it is likely that the weakened part will be the first to suffer.

Denman, indeed, says, that "independently of disease, the uterus may be worn through mechanically, in long and severe labours, by pressure and attrition between the head of the child and the projecting bones of a distorted pelvis; especially if they be drawn into points, or a sharp edge." One or other of these causes also may explain why we more frequently meet with laceration of the uterus when the pelvis is slightly contracted, in the conjugate diameter at the brim, than when the distortion is excessive. It must not be forgotten, however, that by a fall, or other accident, the uterus may be so much injured as to induce a degree of disease that will predispose it to lacerate at that spot where the blow was inflicted, when it takes on itself expulsive action.

Symptoms.—The symptoms of ruptured uterus are strongly characteristic of some violent injury sustained; and they may be

divided into the local and more general marks. The history of the case will be somewhat of this kind:—

A woman who has probably borne two or three children—who has generally suffered lingering labours—who we know possesses a small pelvis—and for whose safety we are consequently more than usually solicitous—is most likely going on well in labour, having borne, with fortitude and good spirits, a number of strong expulsive pains; when, in the acmè of one of these powerful contractions, she suddenly shrieks, cries out that something has given way within her, and expresses herself as being in violent agony. It is said that this rending sensation has been accompanied by a noise distinctly audible to the attendants in the room; but as I was never present when the accident happened, I have no opportunity of verifying or refuting the assertion by my own observation. From that time, all proper uterine action ceases, or becomes very much diminished. If an extensive rent is formed at once, the probability is that the labour-pains will be instantly suspended; but if it be only slight in the first instance, they will most likely be continued for some little time, though their character will be more feeble, and with each return of contraction there will be an increase in the laceration. Should the pains of parturition entirely cease, their place will be supplied by a new pain, referred to one fixed spot, constant, most agonizing, and much more difficult to bear than the throes of labour.

There is seldom observed, consequent upon the accident, a copious flow of sanguineous discharge. You would suppose, *à priori*, as the vessels of the uterus are so large, that when they are torn, blood would be poured out rapidly from the lacerated surfaces, in a somewhat similar way as when the placenta is partially separated before or after the child's birth. But this is not the case; there is seldom considerable flooding as a consequence of lacerated uterus, and sometimes there is no increase of discharge whatever. Even should the vessels bleed freely, their contents need not escape externally; for the head of the child may be so blocking up the pelvis as to prevent the exit of the fluid through the vagina, and it may be effused into the cavity of the abdomen.

On making an examination soon after this new pain is complained of, we shall usually find that the head, which could be easily detected at our previous examinations, can now only just be touched, or it may have receded completely out of the reach of the finger, so as to elude our search. This is owing to the admission of the child's body more or less within the peritoneal cavity, through the rent thus artificially made. We are not, however, to expect this as an universal symptom; though when it does occur, it may be considered the strongest diagnostic mark we can observe; because it is not unlikely that the head may have previously become locked in the pelvis by the contractions of the uterus; and if it be firmly jammed, it is impossible that it can free itself so as to recede. We must not confound the entire disappearance of the head out of the finger's reach under this accident with the alternate advance and retreat that takes place in natural labour, which I have already told you is highly desirable, in order that the soft parts may be at intervals relieved from pressure. I have known some embarrassment produced in the mind of a young practitioner by such a circumstance; but those who have attended to the phenomena of natural labour, can scarcely fall into this kind of error.

Occasionally, then, it will happen that the whole of the child's body at once escapes through the rent into the abdominal cavity; nay, the same strong contraction that caused the rupture has expelled both child and placenta into the peritoneal sac; and the uterus continuing to act, they have both been enclosed in a shut cavity, to which there is no outlet. In a case that occurred within the knowledge of my father, the fœtus was expelled into the belly through the rent, and by the same uterine effort the placenta was thrown into the world through the vagina. I was called to a case lately, in which, although the head originally presented, the child was born by the breech. The laceration took place at the cervix uteri, implicating also the vagina, while the head was entirely above the brim; it escaped through the new-made opening, and the fundus continuing to contract, the breech was forced down into the situation the head originally occupied: an evolution was thus spontaneously made, and the breech passed into the pelvis: the child was eventually extracted with some difficulty. Cases are on record in which the same contraction that caused the laceration expelled the child into the world.

Whenever the fœtus has thus escaped more or less out of the uterus into the cavity of the abdomen, its limbs may be traced through the abdominal parietes; the breech, legs, and perhaps the arms, may be felt tolerably distinctly.

The symptoms I have just enumerated are particular signs, and belong exclusively to the case we are considering; but there are others of a more general character, which soon take place, and are themselves also highly characteristic of the accident. The

general symptoms, then, are exactly such as we should expect to meet with in cases of violent injury to any of the abdominal viscera. The pulse soon flags, it becomes very quick, irregular, and so feeble as to be scarcely perceptible; the respiration becomes hurried, laboured, and painful; the countenance anxious and dejected; the eyes sunken, dull, and inexpressive; the belly swells rapidly, and almost immediately becomes very tender to the touch. Vomiting of a dark-coloured matter supervenes, sometimes almost instantaneously, sometimes at a later period; there is generally hiccough; the extremities become cold and insensible; a cold sweat breaks out on the face, forehead, neck, and chest; and the patient gradually sinks in a very few hours from the accident.

Prognosis.—Although this accident is the most dangerous that can happen to any of the pelvic viscera under labour, except rupture of the bladder, still it is not to be considered necessarily fatal; many cases of recovery are on record, detailed by Douglas, Heister, Mad. La Chapelle, Kite, (which latter I have already cited,) and others; but, to come nearer to our own times, Hamilton has met with one, so also has Blundell. My father has seen three cases, detailed at length in the second part of his Practical Observations;—all these women became subsequently pregnant; one of them he himself attended twice afterwards in labour; another died of flooding, undelivered, between the sixth and seventh month of gestation. On opening the body, we detected in the uterus a cicatrix, running in an oblique direction, at the anterior part, which evidenced the union that had taken place after the rupture. Dr. Collins has detailed two fortunate cases in the Dublin Medical Transactions. Mr. Dunn, of Scarborough, related a case in the Edinburgh Medical Journal of last year, in which the woman got well, and had two children afterwards. I have been unfortunate in the cases I have been called to, not one patient having survived. Two I have delivered whom I expected might recover, one living a week and the other six days. Thus, although the accident must be considered as one of a very formidable character, yet we are not to give the case up as hopeless: we are both warranted in and bound to make some efforts to preserve the patient.

Treatment.—There is only one means, however, that offers a tolerable chance of life—and this is *immediate* delivery. The moment I knew the accident had occurred, I should that instant proceed to extract the child, as being the most likely way to save the mother, and the only means of preserving the infant. If the head has entered the pelvis, and has not receded, so that the long or short forceps can be used, the child may be extracted by their agency. But we generally find that the head has receded beyond the reach of that instrument, and we are compelled to introduce the hand into the uterus, follow the child's body through the rent made into the cavity of the abdomen, if it have escaped, search for the feet, draw it by their means back through the same opening into the cavity of the uterus, and extract it per vaginam. If it should happen that after the breech and shoulders are born the head remains above the brim, and will not pass in consequence of the contraction of the pelvic bones, we must open it behind the ear, and extract it as I have before directed. If perforation become necessary for the child's extraction, even were it alive, I should not hesitate to perform it, however revolting to the feelings; but in almost every instance of ruptured uterus on record, and in all which I have myself attended, the fœtus has been born dead. Baudelocque is the only practitioner of authority, whose writings I am acquainted with, that disputes the propriety of delivery by the feet. He states, that in most instances the fœtus may be extracted by the forceps; but that, if that instrument be unavailable, the abdominal parietes must be divided in preference to the performance of the operation of turning. He limits delivery by the feet to those cases where they are found at the os uteri, or where the child remains entirely within the uterine cavity, or where the vagina only is ruptured, the uterus itself being uninjured. I think myself the British practice far preferable to that inculcated by Baudelocque. During the passage of the child from the abdomen through the uterine rent, great care must be taken lest any folds of intestine be brought down with it, and involved in the opening; because, on the uterus contracting, they would necessarily be strangulated, add very much to the present suffering, and dissipate the slight chance of safety still remaining. An instance of such a complication of the accident is recorded by Baudelocque, as having happened to M. Percy.

Every circumstance connected with rupture of the uterus is agonizing to the utmost extent; the suddenness and awful nature of the accident, the rapid sinking of the vital powers, and the almost certain loss of the infant, all combine to render this a case of most aggravated distress. To these may be added the horrible feeling, experienced in the delivery by the feet, at the hand being introduced into the centre of the abdomen of a living person. Nothing can be more appalling than the sensation communicated by the intestines

encircling and coiling round the fingers; but, however horrifying the idea, all feelings of repugnance must give way before the necessity of the case. It is seldom under laceration of the uterus that the perforator can either be necessary or available; for, if the head be locked in the pelvis, which is not often the case, the delivery may most probably be accomplished by the forceps; and if it remain entirely above the brim, it will either have receded out of reach, or will be pushed up on the application of the instrument, and not afford sufficient resistance to enable us to perforate the cranial bones; so that the forceps, or turning, are the means to which we must generally have recourse.

As soon as delivery has been effected, a large dose of opium must be given, the utmost quietude must be observed, every thing stimulating avoided, and the restorative powers of nature must be trusted to for the recovery. I know no medicines but those of the soothing kind that are likely to be of service; and no other specific means can be adopted until inflammatory symptoms appear, when the case must be treated upon common principles.

But it is not always possible to deliver the patient by the natural passages. The uterus may have expelled the child and placenta out of its cavity into the abdomen, and contracted so strongly as to prevent the introduction of the hand. Under such a state it would be most injudicious to endeavour to extract the child in the ordinary way; both because of the additional hazard which must attend on any attempt to pass the hand into the uterine cavity; and because, even were the hand admitted, the rent through which the child had escaped would be so much diminished in extent, by the contraction of the parietes, as to preclude the possibility of bringing the fœtal body again through it without considerably increasing it, and adding to the original danger.

In these perplexing cases, it becomes a point of much nicety to determine whether the patient should be left to the resources which nature may supply, or whether any means should be taken for relieving the abdomen from the presence of the fœtal body.

There are many cases of reputed rupture of the uterus on record, in which the child has been left in the cavity of the abdomen, and has been evacuated in a putrid state by abscess, the woman perfectly recovering. I am quite of the opinion of Dewees, that almost all these cases have been instances of extra-uterine conception, such as we shall have an opportunity of discussing hereafter; and not cases of uterine impregnation, attended with rupture of the organ. I do not mean to deny the possibility of such an occurrence; but I have little doubt that most of those I have read were mistaken.

Feeling as I do that to leave the child in the cavity of the belly is almost certain death to the mother, I should seriously entertain the question whether the parietes of the abdomen should be divided, and the child extracted by that means, or whether the patient should be abandoned to the chance of what nature might effect; and the answer must depend entirely on the circumstances of the individual case. If the patient were in tolerably good spirits,—if she had not suffered so great a shock as usual by the accident,—particularly if, after explaining to her what had occurred, she were anxious for the operation to be performed, I should have no hesitation in acceding to her wish. But if I found her sinking,—if the powers of life were ebbing fast,—and particularly if twenty or thirty minutes had elapsed since the accident, and the movements of the fœtus had quite ceased,—I should by no means sanction the incision, because of the painful nature of the operation, and because I should presume it would avail nothing, and might probably hasten her death. Much, then, must be left to your own judgment; and your determination must depend entirely on the state of the patient, and the probability of the child's being saved. For the sake of the infant, I would urge the operation immediately after the accident with greater force than if a quarter or half an hour had elapsed, because, while there is a chance of the child's survival, its welfare must be considered as well as that of the mother; but after its death, the mother, of course, alone interests us. Baudelocque informs us, that M. Thibault, a surgeon of Mans, published a case of the recovery of the mother after gastrotomy had been performed, in 1768; that two others of the same kind are recorded in the Journal de Médicine for May in the same year; and that M. Lambron, a practitioner at Orleans, twice operated successfully on the same woman, who in each labour suffered laceration of the uterus. According to Dewees, Hamilton furnishes us with another; Burton gives us one in which the child was extracted alive, although the mother died; and Mr. Haden relates one in which both were saved.

Premonitory symptoms.—Generally laceration of the uterus takes place, without any symptoms indicating even the probability of its occurrence; but many premonitory signs have been noted by Crantz, Levret, and others, as forerunners of the accident: these are all most unsatisfactory, and not in the least to be depended on. It would be most desirable,

indeed, if some infallible precursor of this dreadful occurrence were discovered, that delivery might be effected before the laceration happened, and thus the peril averted. I have seldom known a case in which the uterus ruptured where the attendant was not more or less blamed; and that, as you may gather from what I have just advanced, most unjustly.

We certainly may fear that laceration will ensue, if the woman possessing a small pelvis is in labour of a third or fourth child; if her previous labours had been lingering, and more than ordinarily painful; if for six or eight hours she have been suffering strong expulsive throes, attended with little progress; if she complain of a violent crampy pain in one particular part of the uterus, increased under a contraction, but never entirely disappearing, particularly if that should be the spot opposite the promontory of the sacrum. With the presence of such symptoms, I should say there was a probability that the uterus had received some injury; and I should fear that if the labour were allowed to go on unassisted, the organ might rupture. Under these circumstances, I should consider myself warranted in having recourse to delivery before the woman's powers began to flag, provided the child could be extracted by the forceps, without injury either to itself or its parent.

Though as much averse as any person can be to unnecessary instrumental interference, I have applied the long forceps with great success in some cases where such a state of things was present,—not because the patient was sinking, but because of this fixed pain,—dreading the possibility of rupture of the uterus.

LACERATION OF THE VAGINA

is often complicated, with rupture of the uterus; but occasionally the whole of its coats burst while the uterus remains entire, and the child escapes more or less into the abdominal cavity. I was called to a case once in which the anterior part of the vagina had given way below the os uteri, and the child had escaped between the uterus and bladder into the cavity of the belly; both the latter organs remaining uninjured. I should scarcely have supposed one or other could have escaped a participation in the accident, if dissection had not positively convinced me of the fact. Such cases are usually attended by symptoms similar to those that accompany rupture of the uterus, and are to be treated exactly on the same principles.

But a laceration of some of the fibres of the mucous membrane and muscular coat at the back part of the vagina, sometimes takes place, while the head is occupying the pelvis. This is most usual in first labours, when rigidity exists, and the parts do not distend with their usual degree of ease.

The medical attendant may perhaps be sensible that a laceration has occurred; but it may take place when neither the medical man nor the patient are at all aware of what has happened, the pain which the parts are suffering being but little increased by the fibres giving way. After the birth inflammation will supervene, the healing process will be established, and in the next labour a small cicatrix may perhaps be felt, which may give the first indication of the previous occurrence.

Treatment.—If the laceration were to a great extent—if we feared it might run into the rectum, or up to the os uteri, we should hasten the delivery of the child by the forceps, provided they could be used with advantage; but if it were trifling, the labour must be allowed to proceed in the natural way, the laceration being carefully watched, and the perineum most assiduously supported when the head comes to rest upon it. After labour is completed, a poultice may be applied: if there be no contra-indicating symptoms, a full dose of opium may be given; and the bowels should be early relieved.

RUPTURE OF THE BLADDER.

A more fatal accident even than rupture of the uterus, is the bursting of the bladder under labour, and the evacuation of its contents into the peritoneal cavity. I cannot help thinking that this accident must almost always be the effect of neglect or improper interference; it is very seldom that it can occur in the hands of a careful and judicious surgeon. The kidneys under lingering labour seldom secrete the same quantity of urine as in the ordinary states of the body, because much of the fluids are carried off by perspiration; and we know that the secretions of the skin and urinary organs are in a great degree vicarious, but at the same time the action of the kidneys is by no means suspended, a certain quantity of urine is constantly distilling through the ureters, and the bladder becomes at length distended. If this distention is allowed to proceed beyond a certain point, it will burst, and the case becomes perfectly hopeless.

Another cause of laceration may be the use of instruments under a distended state. If the forceps are applied while the bladder is full, the action of the instrument is very likely to occasion it to burst; and for this reason I have before particularly inculcated the necessity of thoroughly evacuating this viscus before any attempts at delivery are made.

Symptoms.—When laceration of this organ has taken place, the symptoms are exceedingly strongly marked, and are very much like those characterizing a rupture of the uterus; the recession of the child, however, the being able to trace its limbs through the abdominal parieties, and an increase of discharge through the vagina, being wanting;—they are, the appearance of a sudden and violent pain in the region of the bladder, accompanied with a shriek and a declaration by the patient that something has burst within her; a rapid sinking of the powers of life; a general tumefaction of the abdomen, and great tenderness of the belly. The labour pains, which usually cease on a rupture of the uterus, continue for some short time, till they decline as a consequence of exhausted powers. The particular symptoms present in this case, and absent in rupture of the uterus are, a loss of the vesical tumor which before could be felt distending the abdominal parietes, and in its stead a general tumefaction of the whole cavity.

Since rupture of the bladder is so universally fatal, and since it can usually be prevented if proper attention be paid, it becomes our duty, under lingering labour particularly, to keep a watchful eye over its state; and if it become immoderately full, to relieve it by the catheter.

It is possible, however, that the urethra may be a little turned to one or other side, out of its regular straight course, by the pressure of the head, and difficulty may be experienced in introducing a silver instrument; if such an impediment should exist, it must not be overcome by force, but a flexible male catheter must be used instead. I shall, in an after part of the course, speak more at length on the application of this instrument.

It has never occurred to me to meet with a case in which it was necessary to puncture the bladder, owing to an inability to introduce the catheter, but such may doubtless possibly occur; and if so, the puncture should be made immediately above the symphysis pubis, in the hope that the peritoneum may escape injury; for if the bladder be much distended, the peritoneum will be drawn up, and there will be a space above the symphysis pubis not occupied by the membrane.

Treatment.—I should be unwilling to believe that a rupture of the bladder could take place under the immediate superintendence of any of you present; but it is possible you may be called to such a case, and it is therefore right that we should not leave the treatment unconsidered. Regarding the accident as unavoidably fatal, and considering that the woman will most certainly die, I think that our principal duty is to look to the preservation of the child, and to endeavour to extract it before its death by the forceps, or by turning. If there were indications of its being still alive, I should consider the use of the perforator in most cases unjustifiable, and it would become a question whether, if no means of delivery *per vias naturales* could be resorted to, compatible with its safety, the Cæsarean section should not be performed. Such a means, however, should never be resorted to while the woman's powers remain at all vigorous, or the uterine contractions continue active; for so long there is a chance both of the child's life being preserved in utero, and of the labour being naturally terminated. The death of the child, when it does occur, is dependent on the exhausted state of the mother's system, and not upon any destroying influence existing within its own person.

The child, then, having been extracted, although our solicitude for the mother's preservation be wrought up to the highest pitch, I fear any further efforts to save her will be fruitless and disappointing. I cannot coincide with a great authority in this city, who has suggested the possibility of opening the abdominal cavity, sponging out the extravasated urine, cleansing the peritoneum by ablutions of warm water, drawing up the bladder, placing a ligature around the lacerated opening, and hoping for a successful issue. I would prefer abandoning the woman to her fate, certain and fearful as it is, to attempting such a means of prolonging her existence, upon the principle that I would rather sit by quietly, and watch her gradually sink by the hand of nature, than myself be the instrument of hastening her end.

OBSERVATIONS ON THE

STRUCTURE AND FUNCTIONS OF THE NERVOUS SYSTEM;

Being an Abstract of a Paper read before the British Association at Cambridge, in 1833.

By James Macartney, M.D.

Professor of Anatomy and Surgery in the University of Dublin.

The author begins by stating the received opinion respecting the structure of the brain, as consisting of two substances; the one an opaque white pulp, which is considered to be the nervous matter; the other a coloured substance, in some places inclosing the white, and at other places being imbedded in it.

It has been long known, he adds, that the white substance in many parts assumes the shape of bands or bundles of fibres.

Dr. Spurzheim did not hesitate to call these fibres nerves, and was more successful in tracing their course in some parts of the brain than his predecessors had been.

But the author has employed a method of dissecting the brain, which has enabled him to discover that all our former ideas with respect to the structure of the cerebral organ fall far short of the intricacy with which its several parts are combined.

In order to perceive the real structure of the brain, recent specimens are necessary. The sight should be aided by spectacles of a very high magnifying power; and as the different parts are exposed in the dissection, they should be wetted with a solution of alum in water, or some other coagulating fluid. By these means it will be observed that all the white substance, whether appearing in the form of bands, cords, or filaments, or simply pulp, are composed of still finer fibres, which have a plexiform arrangement, and that all those fibres, to the finest that can be seen, are sustained and clothed by a most delicate membrane. By the same mode of dissection, also, it is possible to make apparent the existence of still finer interwoven white fibres in all the coloured substances of the brain, in many of which the nervous filaments are so delicate and transparent that they are not visible until in some degree coagulated by the solution of alum or by spirits.

Dr. Macartney has thus been enabled to see twenty-six plexuses not hitherto described in the brain, the fibres composing which assume two arrangements, the one reticular, the other arborescent.

The membrane mentioned as pervading the entire substance of the brain, and supporting its delicate organization in every part, has heretofore escaped the observation of anatomists, and yet when the fact is declared, we at once perceive that such a membrane must exist. It cannot be supposed that a mass of the magnitude of the brain, and possessing so definite an organization, should form an exception to the fabric of all the other parts of the body, and be left unprovided with a membranous support. This membrane is analogous to the cellular membrane; and if we admit that the filaments of the brain are similar to the fibres in other parts of the nervous system, we may consider the membrane which sustains and connects the cerebral plexuses as their proper neurilema.

The pia mater, or vascular integument of the brain, is composed of two layers; the external of which passes over the convolutions of the cerebrum and the gyri of the cerebellum, and the internal is reflected between these forms, and gives all their exterior surface an intimate covering. The blood-vessels seen on the brain are inclosed between these layers, and are conducted on the inner layer to the substance of the organ. The inner portion of the pia mater is continuous with the membrane of substance of the brain, but becomes so delicate on entering the structure of the organ, that it is readily detached from the brain without apparently injuring the integrity of its surfaces. When the inner layer of the pia mater is obtained in connexion with a portion of the vessels and membrane which penetrate the brain, it has the appearance of tufts or shreds, and as such has been described by Ruysch and Albinus under the name of *tomentum cerebri*.

The neurilema of the brain appears in the adult to be only furnished with colourless vessels, except in those parts where red vessels are seen to pass into the substance of the organ; but in the fœtus, the coloured substance of the convolutions may be injected so as to appear quite red. This fact is consistent with the structure of many other organs during fœtal life, which in that period of existence receives red injection, yet only admits afterwards colourless fluids. The great degree of vascularity in the fœtus is particularly remarkable in the eye, the lining of the labyrinth of the ear, the periosteum, &c.

The author has ascertained that the actual quantity of the sentient substance existing in the brain and other parts of the nervous system, is extremely small. The bulk of these parts is not materially diminished by removing their nervous matter, provided their membranous structure be left behind; and whenever we meet with the sentient substance in connexion with a highly-attenuated membrane, as in the retina and in several of the cerebral plexuses contained in the coloured matter of the brain, it is absolutely invisible until it has undergone some degree of coagulation. It is, perhaps, not assuming too much from these facts, to suppose that the whole nervous system, if sufficiently expanded, and divested of all coverings, would be found too tender to give any resistance to the touch, too transparent to be seen, and probably would entirely escape the cognizance of all our senses. Consistently with this view of the matter, the author thinks that we can hardly take upon us to say that the simplest animals, and even plants, may not have some modification of sentient substances incorporated in their structure, instead of being collected, as in the higher classes of animals, into palpable membranous cords and filaments.

The term plexus has been generally employed to signify an interweaving or crossing of filaments; but Dr. Macartney is satisfied that there is an actual union or

intermixture of substance in both the plexuses of the brain and of the other parts of the nervous system. He has discovered that the roots of the spinal nerves, instead of being connected with the medulla by mere contact or insertion, as hitherto supposed, actually enter into the composition of the filaments of the spinal marrow, and that these roots of nerves (as they are called) form communications with each other within the substance of the medulla. With regard to the cerebral nerves also, it can be shown that they are continuous with the cerebral plexuses in their immediate neighbourhood.

Many of the communications formed between the right and left sides of the nervous system are well known, such as the commissures of the brain, the crossing white filaments of the spinal marrow, the decussation of the pyramids, and the interchange of the two optic nerves in fishes. The author has found so many communications to exist between the origins of the nerves on the right and left sides of the body, that he is disposed to believe it to be a general fact. The optic nerves in the human subject do not decussate, as some have supposed, but form a very intricate plexus where they come into contact. This mode of conjunction accounts for atrophy of the tractus opticus being in some instances found on the same side, and at others on the opposite side to that of the eye affected with blindness.

The facts already observed would justify the opinion that the sentient substance is in no place distant or isolated; that it is essentially one and indivisible; and consequently the nervous system differs from all the other systematic arrangements in nature.

It appears to the author that this view of the sentient system will alone serve to explain the numerous sympathies which exist in animal bodies, the occurrence of disease in the higher orders of animals from indirect or remote impression, and the operation of all remedies which act through the medium of the sensibility.

The mode in which the sentient substance is arranged, its more or less minute subdivision, and the degree of arterial vascularity, determine the phenomena of sensibility as they come under our observation. Hence, we find that the brain, even different parts of it, the spinal marrow, the trunks of the nerves, and their sentient extremities, are so differently endowed, that we might be almost led into the error of supposing them all composed of different materials.

It is well known to surgeons and to experimental physiologists, that the brain is not endowed with any *feeling*, in the common meaning of the word. It may be wounded without any sense of pain to the individual.

The trunks of the nerves not possessing the arrangement of the sentient substance suitable to common sensation, can only transmit the feeling of pain. Thus, patients after amputation often complain of pain in the part that has been removed; but the author believes that in no instance have they experienced natural or agreeable sensations, or have expressed a consciousness of the presence of the removed limb unattended with pain.

The sentient extremities of nerves are alone capable of being affected by narcotic poisons. Half a tea-spoonful of the essential oil of almonds introduced into the substance of the brain of a rabbit did not produce the least effect on the animal, nor was any effect produced by placing the end of the sciatic nerve in a spoonful of the essential oil of almonds during half an hour, although the animal was afterwards killed in the usual manner by a few drops of this liquid on the tongue.

Impressions on the extremities of nerves sent to the organs of sense and to the external surfaces of the body, are attended with consciousness in the individual, whilst those naturally made on the interior surfaces cause no perception. These surfaces, however, are amply supplied with nerves, and possess a high degree of local sensibility, by which they not only discern mechanic forms, but qualities in food and medicines that the perceptive powers of the individual cannot distinguish. These internal and unperceived sensations are continually, though secretly, influencing the condition of the whole nervous system, and are often the cause of remote morbid actions. Under some circumstances, movement follows impression made on the *external* parts of the body after consciousness has become extinct. It is known that the ordinary actions of the iris correspond with the impressions of light on the retina; and the author has observed that the iris continues to move under the same law after the animal's head has been cut off, or the eye taken out, as long as the retina retains its local sensibility: similar effects take place in other parts of the body.

The mutual influence of the nerves and spinal marrow seems to be all that is necessary during fœtal life, as the absence of the brain in the acephalous fœtus does not interfere with any of the functions of the creature until the moment of birth.

The offices which the coloured substance performs in the nervous system, have been matter of speculation with anatomists. One obvious purpose of its existence is to give support and security to the finest sub-

divisions of the sentient substance; we therefore find that it affords such protection in proportion to the necessity: hence, in the brain, the coloured substance is soft and tender, while in the ganglia of the nerves it is generally dense and firm. Besides, however, forming a nidus for the ultimate plexuses of the sentient matter, the coloured substance would seem to fulfil some other use not yet ascertained, as wherever it exists it exhibits the same character with respect to colour, varying from yellow to green or brown. Dr. Macartney considers the yellow spot in the retina of the human eye, and in that of the monkey and lemur, as a ganglion, having discovered that it contains a more intricate reticulation of the nervous filaments than exists in the other parts of the retina.

The coloured substances of the nervous system in no degree derive their peculiar tints from the blood that circulates in them, since the colours are palest in the fœtus, and grow darker as the nervous system approaches its perfect organization.

It is a generally received opinion that the ventricles of the brain are cavities or hollow spaces containing some liquid. This error has arisen from the common modes of dissecting the brain, which necessarily separate the surfaces of the ventricles from each other. If, however, the dissection be performed without disturbing the natural position of the parts, not the slightest appearance of cavity or interspace presents itself. The sole use of the ventricles, therefore, seems to be, merely to gain an extent of surface necessary to the development of the peculiar organization of the brain. Apparently there is less superficies in proportion to the magnitude of the mass of the brain in man than in that of animals; but if we calculate the depth of the surfaces between the convolutions of the cerebrum and on the branches of the arbor vitæ in the cerebellum, together with the internal surfaces, we shall find that the superficies of the human brain is greater in relation to its bulk than that of any other animal. In addition to the surfaces already known, Dr. Macartney has ascertained the existence of ventricles (so called) in the bulb of the olfactory nerves, and in the optic thalami of the human adult brain. In the thalami the distinction of surface is obscure, but in the olfactory tubercles it is sufficiently plain.

The author concludes with stating his belief, that every assemblage of the nervous filaments in the form of plexus is destined to fulfil an especial purpose, and with the anticipation that at no distant period we shall be able to understand many of the phenomena of sensation which have been hitherto veiled in the utmost obscurity.

ON THE COMPARATIVE MERITS OF DR. BELLINGERI'S AND SIR C. BELL'S WRITINGS AND OPINIONS ON THE FUNCTIONS OF THE FIFTH AND SEVENTH PAIRS OF CEREBRAL NERVES.

By G. Negri, M.D.

[Continued from p. 737.]

Bellingeri's Opinions on the Functions of the Ganglionic Portion of the Fifth.—Part II.

In the preceding article we have seen that the ganglionic portion of the fifth, according to Bellingeri's opinion, exerts, in the first place, a direct influence over the organic life of those parts and organs which receive branches from it. Moreover, by its connexions with the other nerves of the face, and by its distribution round the smallest arteries, it serves principally to depict over the face the physiognomical expression of the different passions; when, by its anatomical relations, through the sympathetic nerves, with all parts of the body, the numerous branches of the ganglionic portion of the fifth may be also sympathetically called into action.

Although these statements may appear reasonable enough to those who read and understood the anatomical and physiological arguments brought forward by Bellingeri, still they did not satisfy Mr. Alexander Shaw, who was quite astonished to learn that the *Italian physiologist* had termed the fifth "the nerve of *physiognomical expression*."—(See Med. Gaz., July 19, p. 501.)

Sir Charles Bell, when defining the seventh pair of nerves, in his fourth paper already mentioned, says,—"The seventh is the auditory nerve, and the division of it called *portio dura* is the motor nerve of the face,..... and that on which the expression of the face depends."—(See last work, p. 217.) Therefore, according to Mr. Shaw's doctrine, Bellingeri must be wrong; and has then committed, says he, "the fatal blunder of making the *sensitive* root of the fifth pair preside over the actions of the muscles of the face." (See Med. Gaz., July 19th, p. 502.)

When Mr. Alexander Shaw is speaking of the seventh, he informs us that

" this nerve has *but one function;* it is a motor nerve; it can carry a mandate *only outwards* to the muscles; it has *no power of carrying a sensation inwards.*"—(Med. Gaz., July 19th, p. 560.) Moreover, " *It...has no share in contributing to the sensibility of the surfaces on which it is distributed.*"—(Id. p. 561.)

Now I should like to know, if the seventh have no sensation or power to carry any impression from without inwards, how is it to be considered the nerve of expression of the face?

Looking at Dr. Johnson's definition of the two words, *sympathy—sympathetic*, I found one to be, " *mutual sensibility;*" the other, " *having mutual sensation; feeling in consequence of what another feels.*" Then, how are they to be applied to the seventh, which has no sensation, rather than to the fifth, which is the nerve of feeling of the face? How the lachrymal gland may be put into violent action by sympathetic impression from the seventh, if it merely receive its nerves from the lachrymal branch of the fifth? Is it then so extraordinary, if the principal nerve of feeling of the face, and of reciprocal connexion with all its parts and organs, be called by the Italian physiologist " the nerve of physiognomical expression?" Who then made " the fatal blunder?"

Leaving time for Mr. Alexander Shaw to give us an answer, I shall now proceed with Bellingeri's exposition:—

§ XX. Involuntariis præesse actionibus par quintum hucusque demonstrare adgressi sumus, partium scilicet organicæ vitæ, pathetieis, et sympathicis affectionibus; sed et multiplicibus inseritur in partibus motu donatis, in cunctis scilicet faciei musculis, in musculis palati, et superioris pharyngis, in musculis linguæ, et maxillæ inferioris, in nonnullis capitis musculis; nasi denique, et aurium; in iride tandem, et in internæ auris musculis. Quanam ratione hosce moveat, inquirendum remanet. (p. 148.)

§ XXI. Physiologi partiuntur motus in voluntarios, involuntarios, et mixtos; qui autem proficiscuntur motus a quinto pari, involuntarios esse, crederem; quod ut comprobem, a demonstratis incipiam. Et primo quidem involuntarius fere penitus est iridis motus in humana specie. Sed nervos iridi quintum largitur, tertium quoque per ganglio prius efformato. Perraro equidem, sed aliquando a voluntate dependens observatus est iridis motus quibusdam in individuis; ita in Fontana vidit Mascagni. Sed jam in anatome animadvertimus, et interdum nervos ciliares nullos a quinto, sed unice a tertio pari fuisse progenitos; defuisse et semel ganglion ophthalmicum Gunzius asseruit. An physiologicus inde motus iridis, a generali lege recedens, cum peculiari conveniret anatomica ciliarium nervorum constructione? An quando tantummodo a tertio pari nervi ciliares procedunt, iridis motus ganglii deficientiæ iisdem in individuis debetur? Putarem, namque in avium quibusdam speciebus, psytaco, et noctua, in rajis etiam iris voluntatis obedit imperio, quod clariss. Sprengel ganglii ophthalmici defectui tribuendum esse, subdubie proponit. (p. 149.)

Pupula autem a luce in statu salutis non directe propria actione movetur, sed sympathice a consensu cum nervo optico. Sed non semper sympathice ab optico movetur iris, interdum citra optici influxum movetur; cæci fuerunt a vitio nervi optici, in quibus pupilla lucis movebatur actione. Involuntarius autem iridis motus, præsertim a quinto, non autem a tertio pari fovetur, quod in reliquis suis propaginibus voluntatis obedit imperio. Revera Whytt animadvertit in amaurosi unius oculi, ipsius pupulam a vario lucis gradu neque constringi, neque dilatari; moveri tamen ipsam, si lux in oppositum agat oculum. Hyoseyamo ventriculo excepto, mydriasis suboritur, sive integer sit visus, sive cataracta, sive amaurosi detineatur oculus. Utroque tamen in easu sympathicus iridis motus neque ab optico producitur, neque a tertio, sed a quinto perficitur.

Sed non omnimode involuntarius est iridis motus, aliquatenus etiam, leviter quidem, subjacet voluntati; id et innuere visus est Whytt. Hinc, quum attente objectum conspicimus, vel voluntatis imperio sub hac animi contentione movetur iris, vel major ad motum inducitur aptitudo; hoc autem a filamentis tertii paris peragi putarem, qui in cæteris propaginibus fere ex integro a voluntate dependet; hinc admirandum naturæ consilium, quum enim duplicis ordinis motus in iride perficeretur, et nervis ipsam ditavit, duplici e fontis petitis. (p. 151.)

§ XXII. Constructionis, et func-

tionis æqualitas postulat, ut de nervis auris interiora ingredientibus, et auditui famulantibus dicamus. Sicuti involuntarie pupula a luce, ita et a sonoris aëris undulationibus musculi internæ auris præter voluntatis influxum fere semper moventur . . . . Musculi autem internæ auris suas nervosas propagines a quinto una cum septimo accipiunt . . . . Sicuti enim iris in oculo a quinto, et tertio pari simul nervis ditatur, ita et a quinto, et septimo musculi auditui suppetias ferunt. Sicuti iris maxima ex parte citra voluntatem movetur, ita et musculi internæ auris: sicuti iris, pariter et isti musculi sympathice ab intercostali afficiuntur; commode autem hæc sympathia explicatur per ramum petrosum nervi vidiani, qui alio ramo carotideo ad intercostalem efformandum concurrit . . . . Involuntarius igitur maxima ex parte, et aliquatenus voluntarius est musculorum internæ auris motus, et natura etiam quintum, et septimum par in aure copulata est, musculorumque nervos duplici e fonte desumsit.—P. 153.

§ XXIII. Modo ad ramos palatinos deveniemus, qui in uvula, velo pendulo palati, et pharynge superiori distribuuntur, et musculis circumflexo palati, et elevatori palati mobilis, sicuti, et superiori pharyngis parti prospiciunt; ibi solæ sunt quinti paris propagines, ipsorumque motus ab animalis nutu nullomodo dependent. Moventur quidem in deglutitione, quæ licet prima fronte a voluntate omnino perfici videatur, attamen, quæ a posteriori oris cavo perficitur, solummodo ab alimentorum, vel salivæ inducitur stimulo; hinc ore penitus vacuo, et ipso adnotante Magendie, deglutitio impossibilis, vel nulla. (See Précis Elémentaire de Physiologie, t. ii. p. 66.)—P. 153.

Here I shall omit to transcribe what Bellingeri states about the lingual branch of the fifth, as I shall return to this subject on another occasion.

§ XXIV. P. 156. Jam vero ad maxillarem inferiorem gradum facimus, quem vidimus in anatome ramos tribuere musculo mylo-hyoïdeo, ventri anteriori digastrici, omnibusque muculis in mento, ipsiusque viciniis existentibus: aperit inde maxillam inferiorem, et concurrit ad oris occlusionem, et diductionem; famulatur quapropter alimentorum sumptioni, masticationi, atque deglutitioni, qui certe motus quandoque involuntarii a solo instinctu peraguntur, namque et in infante exercentur. Involuntariis autem destinari actionibus comprobant reliqua ipsius filamenta, quæ dentibus, glandulisque labialibus inseruntur. Quum porro et voluntarie moveantur omnes musculi menti, hinc vix e proprio foramine egresso maxillari inferiori, ibi traducta septimi paris filamenta multimode connectuntur, omnibusque cum ramis conjunguntur."—P. 157.

§ XXV. Infraorbitalis rami propagines eamdem sequuntur rationem; quum enim in musculis palpebræ inferioris, labii superioris, oris, et nasi insumuntur, vel nullommodo, ut sunt musculi nasi ut plurimum, vel dumtaxat involuntarie cunctos movent. Præsunt et ipsi nonnulli ciborum assumtioni, et masticationi, alii oculos tuentur, et ex parte claudunt, narium musculi quandoque in hominum morbis, et sub cursu in ipso, et in equo præprimis, respirationem coadjuvant, qui certe omnes involuntarii, vel mixti sunt motus. Sed quum et obediant voluntatis actioni, hinc ubique in omnibus hisce musculis filamenta septimi paris cum quinto natura commiscuit. Eadem omnino dicenda sunt de ramis nervi frontalis, infratrochlearis nasalis, deque filamentis nonnullis lacrymalis, qui in musculis occipito-frontali, superciliari, pyramidali nasi, orbiculari palpebrarum, et levatori palpebræ superioris finem habent. Demonstravimus superius sympathice, et pathetice moveri, ac affici frontalis, infratrochlearis, et lacrymalis propagines. Involuntarie autem moventur in palpebrarum motibus, hinc nobis fere insciis palpebris nictamur dum vigiles sumus, oculos, ingruente somno, claudimus, aperimusque expergefacti, hæc omnia exercet et infans. Verum quum frontis, nasi, et superioris palpebræ musculi jubente voluntate diversimode moveantur, septimi, et tertii paris propagines ipsis alicubi natura est elargita, et præcipue in exterioribus musculorum frontis, et orbicularis palpebrarum stratis quinti, et septimi paris anastomoses fieri, in anatome observavimus.

Quintum denique par omnes hosce involuntarios motus absolvere, comprobat observatio monstri a Lawrence descripti, et superius relata, in quo *deficiente septimo pari, et præsente quinto*, ciborum assumtio, suctus, masticatio, et deglu-

titio perfecta est. Sed non tantum involuntarie agunt variæ maxillaris superioris, et inferioris divisiones in ciborum sumptione, et masticatione, verum et in voce, et diversis vocis modulationibus suam opem conferunt; plorat inde non solum, sed et ejulatus emittit, et clamitat infans, os diducit, distorquet, circumagitat linguam, tremit ipsa, et labia.—P. 158.

§. XXVI. Secretionibus quoque, et multiplicibus, quæ certe organicæ vitæ munera sunt, omnibus in facie præest quintum par; ipsius influxu lacrymas glandula lacrymalis secernit, humor meibomianus a palpebrarum glandulis plorat, mucum pituitaria narium membrana separat, salivam glandula sublingualis, maxillaris, atque parotis, lubricum mucum mucosæ glandulæ palati, totiusque oris, buccarum, linguæ, et labiorum, tonsillæ ipsæ quinti actione proprium exequuntur officium, cerumen in auribus a quinti secernitur auxilio. Vidimus quoque sanguinis circulationem in facie a quinto recte dirigi, modo accelerari, retardari, impediri, aut perverti; hinc, cur constanter comitetur arterias, ut summa cum patientia, et labore de omnibus quinti ramis demonstravit Wrisbergius. Organicas inde usque adhuc ostendimus quintum par exequi functiones.—P. 159.

§. XXVII. Hisce porro constitutis, inquirere pergimus, an, et quommodo quintum par sensibus tactus, olfactus, et gustus inserviat.

This will be the subject of my next article, which I hope to have ready for next Saturday.

September 6, 1834.

ON DOUBLE GANGLIA OF SOME OF THE SPINAL NERVES;

AND ON A DIRECT COMMUNICATION BETWEEN THE SPHENO-PALATINE AND LENTICULAR GANGLIA.

To the Editor of the Medical Gazette.

Sir,

It has been supposed that the anterior and posterior bundles of the spinal nerves have served for uniting or controlling different sets of muscles; but it is not a very rare occurrence for some of the ganglia of the lumbar and sacral nerves to be double; and in every example of this sort I have carefully examined, the anterior bundle has also divided equally to join the anterior point of each divided ganglion.

In one subject I traced a very distinct nerve passing upwards from the spheno-palatine to the lenticular ganglion; but whether this be constant I cannot at present determine.

I have stated the first fact, because I am desirous of having principles in physiology established, as far as can be done with safety; and as this subject affords me the opportunity, I cannot help expressing that, in the present state of knowledge, it may be more than conjectured that if the structure and functions of the brain and nerves were perfectly understood, a simple principle, consistent with that of every other system, would be found to direct and govern them. But as there are modifications of the several parts of every animal, and particularly of those connected with the vital and instinctive organs, so there must necessarily be a modification of this principle, which will be manifested in a different form of the brain, and a different disposition of the nerves, their plexuses and ganglia. If experiments be instituted on one animal, with the expectation that the functions of its organs correspond with those of man, or even of other animals of the same order, made in different forms, and having different habits, and a different distribution of the nerves, any reasoning founded on such conclusions will frequently be erroneous. That a too limited induction has in many respects been fallacious, changes and modifications of opinion have sufficiently proved; and that this will continue to be so under the same system, the labours of future anatomists and physiologists will also determine. Those who have carefully cultivated comparative anatomy, even on a limited scale, cannot fail of being convinced that order can only be brought out of confusion, and the present differences reconciled by the most compre-

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* In Dr. Negri's paper, in our No. for August 30, p. 785, l. 48, col. 1, *for* "formulari," *read* "fæmulari;" same page, col. 2, l. 28, *for* "anatomis*is*," *read* "anatomis;" p. 786, col. 1, l. 11, *for* "nervorum," *read* "nervum;" p. 787, col. 1, l. 28, *after* "atque ita ratio," *insert* "redderetur, quommodo sympathice organicæ perficerentur," cum, &c.

hensive plan of investigation; and until this shall have been instituted, the physiology of the nervous system can never acquire the harmonious perfection it is capable of exhibiting.—I am, sir,

Your obedient servant,

JOSEPH SWAN.

6, Tavistock-Square,
Aug. 29, 1834.

ARTERIALIZATION OF THE FŒTAL BLOOD.

To the Editor of the Medical Gazette.

SIR,

IT is by no means my ambition to enter into a "wordy warfare" with your correspondent Mr. G. Phillips; for I conceive that in the present state of our physiological knowledge upon the subject-matter at issue, we might proceed almost without end with suppositions and negative postulations, yet in the absence of the "*experimentum crucis*" be still very far from even the threshold of truth. Mr. Phillips, however, evidently mistakes the gist of the controversy, for he imputes to me (by implication) adverse propositions which I never employed, and which, indeed, are contrary to my settled belief. Thus he says, "I cannot give up my opinion that the fœtal blood undergoes a change in the placental vessels in consequence of its near approach to the mother," assuming that I had previously denied the fact, and forgetting that I had recorded my persuasion in so many explicit words, that "the blood of the embryo undergoes a change by being acted upon by the maternal fluid."

If Mr. Phillips will take the trouble to revert to my former communication, he will also find that it was not on account of the interposition of the *membrana decidua* that I doubted of the *complete* arterialization of fœtal blood; for I was fully aware of the proof given by Priestley, that the surface of dead blood exposed to air was purified even through the medium of a *dead membrane*. My scepticism, as I then stated, arose from the relative consideration of the immaterial quantity of maternal fluid presented to the vital current of the fœtus, when viewed with the immense extent of surface to which the blood is exposed in its passage through the air-cells of the lungs.

Your correspondent has reiterated the evidences of the unquestioned agency of the atmosphere upon fœtal organization; but he has not attempted an explanation how a dense fluid, not in actual contact, can materially affect another fluid almost equally dense in its properties. The arguments he has adduced of oviparous animals, tend only to strengthen my former assertions; and as the matter stands thus amicably between us, I need not assume the attitude of defence, nor be like one of old *gratis anhelans, multa agendo nihil agens!*—I am, sir,

Your obedient servant,

R. H. ALLNATT.

Wallingford, Sept. 2, 1834.

PHYSIOLOGY OF THE PLACENTA.

To the Editor of the Medical Gazette.

SIR,

IT is unnecessary for me to engage with a third person in any controversial discussion as to the meaning of a living author, who, as far as I have been able to ascertain, has no physical or moral incompetency to explain his own opinions, and who has hitherto evinced no peculiar reluctance to appear before the public. Without, therefore, meaning any personal incivility to Mr. Noble, who has addressed to me some observations in the last number of the Medical Gazette, I may, perhaps, be allowed to await Dr. Robert Lee's own exposition of his views, to the elucidation of which his professed admirer and advocate has not much contributed, howsoever he may have succeeded in involving him "in a little perplexity." At present, I am not aware that my opinions upon the subject of the passage of maternal blood to and from the placenta are materially at variance with those ascribed to Dr. Lee, in the notice before adverted to of Velpeau's *Embryologie*, the authenticity of which Mr. Noble appears to avow. In that review it was announced, "that coagula of the maternal blood extended from some of the openings in the lining membrane of the uterus into canals formed by the deciduous mem-

brane on the margin of the placenta," and that "*these* VESSELS *or channels* in the decidua could be traced only a short distance along the margin of the placenta and between the lobes." Whatever may be said of the novelty, at least there can be little doubt of the accuracy, of this observation. They are the same "vessels or channels" as those described by Dr. Hunter, in a passage quoted at large in my last communication, and even more explicitly by a justly-celebrated German anatomist, who says, "On remarque, sur-tout, à la circonférence du gâteau placentaire, un veine circulaire, dans laquelle s'abouchent plusieurs veines de la membrane caduque*." This avowal, that maternal blood does find its way between the lobes of the placenta, is not altogether consistent with the statement, "that the maternal blood, as it flows in the uterine sinuses, cannot possibly escape either into the cavity of the uterus, or into the substance of the placenta†," and appears to me rather an alteration of, than an addition to, Dr. Robert Lee's former sentiments; but it is evident that Mr. Noble would employ these terms in a sense very different from that which appears to be their ordinary signification.

Your obedient servant,

H. HUGHSON.

Sept. 8, 1834.

CHOLERA — TREATMENT BY SULPHURIC ACID.

To the Editor of the Medical Gazette.

SIR,

IT occurred to me that sulphuric acid, from its well-known property of coagulating albumen, might be usefully employed to arrest the rice-water evacuations of malignant cholera; and although I have hitherto had but one opportunity of trying it, the effect was so apparent and well-marked, that I am unwilling to delay communicating the result to the profession.

The subject of the case was a girl eight years old, whom I saw on the 29th of August, and found to be incessantly passing rice-water stools, with occasional vomiting. Her pulse was distinct, but feeble; the surface of the body cold, and her countenance sunk and cadaverous, with intense thirst. I ordered her the following mixture, which she commenced taking at three o'clock P.M.:—

℞ Infus. Caryoph. ℥vj.; Acid. Sulph. Dil. ℥iss.; Sacch. Pur. ℥ss.; Tinct. Opii, gtt. xxiv. M. To take two table spoonsful every hour.

I again visited her at eight P.M., and found she had passed but one evacuation since taking the medicine, which consisted entirely of coagulated albumen. I now directed the medicine to be given every two hours only. Her skin I found to be warmer, and her thirst less intense.

30th, 10 o'clock A.M.—She has slept at intervals during the night, and has passed two evacuations, both being somewhat fæculent, mixed with coagulated albumen; her skin somewhat hot, and her pulse 112; her countenance much improved; thirst trifling.

8 o'clock P.M.—Continues improving; has passed two more evacuations, now decidedly fæculent, but still mixed with coagulated albumen; has made water for the first time, and says she feels quite comfortable.

To continue the medicine at longer intervals.

31st, 10 o'clock A.M.—Continues going on well; countenance natural; skin cool; and pulse below 100. Feels some relish for her slops; has had no evacuation, but makes water freely. In the evening I found she had passed one good fæculent evacuation, with no albuminous flakes.

Sept. 1st.—Quite convalescent; able to walk about the room, but of course weak.

When I first saw this patient she was evidently sinking fast into a state of collapse. No other medicine was given than the mixture with acid: and the rest of the treatment consisted in keeping her moderately cool, with her head low; and her diet to consist of sago, arrow-root, or gruel, to be taken only lukewarm. The complete success attending this case makes me most anxious the plan should be fairly tried

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* Meckel, par Jourdan et Breschet, t. iii. p. 758.

† See in Phil. Trans. for 1832, p. 60. See also Med. Gaz, vol. x. p. 248.

upon a more extended scale, and then upon its own merits let it stand or fall.

I remain, sir,
Your obedient servant,
EDWARD GREENHOW.

North Shields, Sept. 1, 1834.

P.S. — This girl's grandmother died a few days previous, in the same room, of malignant cholera.

EXTRACTS
FROM A
JOURNAL OF NATURAL HISTORY;

Kept during a Voyage from England to New South Wales, and on the return to England, by way of Batavia, Pedir Coast, Singapore, &c.

BY GEORGE BENNETT, ESQ.

[Continued from p. 781.]

ON the 30th of May, in latitude 10 deg. 47 min. N.; longitude 23 deg. 38 min. W.; several albicores (scomber thynnus) were about the ship, and seen in ardent pursuit of the flying-fish, which rose from the deep before them in great numbers.

May 31st, in the evening (which seems, from experience, to be the best time to use the towing-net), the towing-net was found filled with various molluscas and crustaceous animals. The bottom of the net shone with a brilliant greenish phosphoric light, which was increased when disturbed. This brilliancy was found to proceed from numerous pyrosoma atlanticum. There were also numerous salpæ, porpitæ, several specimens of phyllosoma commune, smerdis vulgaris (?), and several small fish. Latitude 8 deg. 39 min. N.; longitude 23 deg. 30 min. W.

Early in the morning of the 1st of June, the net captured, besides smerdis, salpæ, and phyllosomæ, two specimens of a curious animal, varying in length, from 4 inches to 7½ in length, and ¼ of an inch in breadth. It seems allied to leptocephalus of Shaw; has a bony mouth, with teeth; eyes large, mouth pointed, and resembling in appearance the head of a fish; the body gelatinous, transparent, long, terminating in a kind of rudiment of a tail; a long rudiment of a dorsal fin, of a delicate nature, runs nearly the whole length of the back. Through the centre of the body, commencing from the head, a long white line passes down the whole length of the animal, which I consider a spinal cord; from which lateral branches are seen to be given off from each side throughout its whole length. This curious animal seems a link between the fish and the mullusca. In the Appendix to Tuckey's Narrative of an Expedition to the Coast of Africa, is the following remark, which I consider alludes to a molluscous animal similar to this:—

"Two species of a genus (which appears to be new) allied to leptocephalus, were taken off the African coast. Their head is smaller and more pointed than that of leptocephalus; their bodies are even more compressed, but are marked in the same manner, by transverse, zig-zag lines, and their teeth are similar. Rudiments only of the dorsal and anal fins exist towards the extremities of their bodies, and no pectoral fins can be discovered." p. 409.

Latitude 7 deg. 11 min. N.; longitude 23 deg. 30 min. W. I also captured several phyllosoma commune: most, or nearly all, the specimens have the third joint broken off near the flagrum: as this has been remarked also by Leach, as occurring in the specimens collected during Captain Tuckey's voyage, it is not improbable that their extreme delicacy renders them very liable to be lost from injury received in the net. The shields of the tail are represented as five in the drawing in the Appendix to Tuckey's Voyage, whereas, in these specimens, no more than four could be made. There were also several porpita, of a purplish colour.

Jun 2d.—Latitude 6 deg. 11 min. N.; longitude 23 deg. 40 min. W. Captured, in the evening, some of the genus smerdis, and a small species of medusa, the latter of a pinkish colour, and emitted a very strong phosphoric light, particularly when touched or moved; even when there was no phosphorescent appearance from it, moving the animal was certain to produce it, and the phosphorus would readily come off from the animal upon the fingers; and rubbing a portion of the animal on the hand would cover it with a phosphoric light, as if it had been rubbed with a

piece of phosphorus. A very small species of diodon was also captured in the net this evening: it was about half an inch in length, and of a light bluish colour, with black spots.

June 8th.—Latitude 2 deg. 57 min. S.; longitude 27 deg. 51 min. W. A solitary white tropic bird was seen this morning. I have never seen these birds fly with rapidity; they soar high, with a slow, steady, or jerking flight, descending and ascending slowly in the air, uttering at the same time their peculiar shrill notes. It procures fish, &c. in the same manner as the gull—that is, by seizing it only on the surface of the water. Their flight is slow and graceful.

We passed several physalia during the day: the waves would frequently "capsize them," but they readily resumed their former position, and sailed (glowing in crimson tints) as proudly and beautiful as before.

The "poor persecuted" flying-fish is an object of curiosity among the lady and gentlemen passengers who for the first time venture on the bosom of the deep, on a voyage to tropical countries; many of the former, in particular, regard them as half bird, half fish; and that the "oft talked-about" flying-fish has "the head and body of a fish, with the accompanying scales, wings, and tails, studded with beautiful feathers, and a few fins." One of the fish having fallen on the deck, was taken to a lady who had previously expressed a strong desire to see one; but this kindness and attention, although received with politeness, was not with the delight expected; for the remark made was, "Is that a flying fish? why, it is like any other fish, only its fore fins are a little longer; why, it has no feathers, or pretty variety of colours!"

The pectoral fins of the flying fish, when expanded and dried, have an elegant appearance, and mounted on card board are very ornamental. I have often preserved them in this manner, by keeping the fins extended in the natural manner by pins until dry; but it is very difficult to preserve the edges entirely perfect, from their extreme delicacy.

In all the specimens of flying fish I have examined, the air-bladder was always found inflated; this bladder has been considered as aiding in the support of the animal in the air; but as it is not in proportion larger than in other fish which have no need of such support in another element, the assertion is incorrect. The air sac extends in this fish from the pharyngeal bones to the extremity of the body; and on laying it open, I have observed the large dorsal blood-vessels are very distinctly seen through the pellucid membrane. The largest portion of the air sac is nearest to the head, and diminishes gradually as it passes towards the tail. It is considered that a particular purpose is answered by the larger dimensions of the air bladder near the head, as "the compensation of the great gravity of the animal at this part in consequence of its breadth," which is required for the support of the animal's body in the air in a favourable position for flight. The situation of the pectoral fins before the centre of gravity in this, as in other flying fish, also tends to elevate the head, as remarked by Lacèpède*."

Lacèpède considers the dorsal fin being placed opposite to the anal as an essential character of the genus Exocetus. They were so situated in the specimens I examined of E. evolans, as well as in the specimen recently captured with dark wings; but Dr. Abel mentions, that in one he had an opportunity of examining, there was a difference in the situations. "Is it," he says, "a universal character, or is the situation of the dorsal fin in my specimen a mere exception to a general law?"

But little attention has been, until latterly, given to the specific differences of this genus. At daylight of the 11th of June, 1832, in latitude 10 deg. 12 min. south, longitude 30 deg. 19 min. west, blowing a fresh topsail breeze, one of these fish came on board from the windward, at a height from the surface of the water of twelve or thirteen feet, and fell into the lee scuppers. It appears to differ in several characters from those described.

It measures in length ten and a half inches; breadth between the eyes one-fifth of an inch; pectoral fin five and a half inches in length, expansion two and a half inches; ventral fin two and one-eighth inches, expansion one inch and a half. This differs from others I have seen in the less divergence and co-

* H. N. des Poissons, tom. v. p. 406; quoted in Abel's China, 4to. p. 5.

lour of the pectoral fins, and the greater length of the ventral. The pectoral fin consists of eighteen rays, the three undermost of which are very short, and distinguished with difficulty; the third ray is the longest. The pectoral fins on the upper surface are of a purplish colour, gradually diminishing in intensity as they advance towards the extremities of the fins, which are of a bluish white colour. From the sixth ray the centre is white, which is continued to the last ray, and has the appearance of a large irregular whitish spot upon the fins. The very small terminating rays are colourless. The under surface of the pectoral fins is of a bluish white, intermixed with dark shades; base of the rays silvery. The pectoral fins of this species, when expanded, have not the convexity of form seen in the E. evolans; the pectoral fins of this species opening in a straight direction. The ventral fins are situated five inches lower down, about an inch distant from the anus. The rays are of a bluish colour. The membranous part of the fin perfectly colourless and transparent. The tail of a bluish colour, with the rays purple. The upper part of the head and back deep purple, diminishing on the side in the deepness of colour; of a faint pinkish tint, mixed with silver, over the abdomen. Branchial rays, 10; ventral, 6; dorsal, 11; anal, 11. Distance between the pectoral fins, one inch and two-eighths. Irides bluish; branchial operculums silvery.

My specimen accords the nearest, from the position of the fins, to E. exiliens, or E. mesogaster. Like the latter species the fins are also bluish; but it does not accord with it in general colours, and the ventral fins are longer. The rays of the pectoral fins, when expanded, do not describe a curve, as I have remarked in others, but open in a direct line. No scales upon the head in this specimen. I have found the stomach of all the specimens of flying fish examined always empty.

The flesh of this fish, when cooked, is very delicate and well tasted.

On the 27th of January, 1834, when on a passage from Singapore to England, in latitude 28 deg. 56 min. south, and longitude 39 deg. 56 min. east, and the range of the thermometer being from 75 to 80 degrees (it may be interesting to give the range of the thermometer, for the purpose of ascertaining the lowest temperature where these fish are found to exist), a flying fish was found in the lee quarter boat, and from the situation in which it was found, must have flown a height of full twelve feet above the surface of the water, and adverse to the wind; it was found in the boat early in the morning, and most probably came on board during the night. The fish was found at the bottom of the boat, and as the boat had a covering over it, open only with a very small space at each end, the animal must have either flown in this opening when it came on board, or else have, in its struggles, contrived to fall through afterwards.

The specimen was of the following dimensions:—

	Inches.
Length from the head to the extremity of the tail	11
Greatest breadth of the body of the fish	$1\frac{7}{8}$
Length of the pectoral fin	$6\frac{2}{8}$
Greatest expansion of do. do.	$3\frac{1}{2}$
Length of the ventral fin	$2\frac{2}{8}$
Greatest expansion of do. do.	$1\frac{2}{8}$

The pectoral fins had each sixteen diverging rays, ventral six, dorsal fifteen, and anal ten rays. It differed in many of its characters from any described species. The back was of a deep purple; sides and abdomen silvery. The pectoral fins were above of a dark bluish black, with an oblique, narrow, nearly transparent ray, extending across them. The under surface of these fins was also of a dark bluish black colour, but probably not more than a shade lighter to the upper surface. The rays above were of a similar colour to the intervening membrane, but below they were lighter, and more inclinable to blue. The ventral fins were situated posterior to the middle of the abdomen, and were perfectly transparent, with an irregular and large deep black spot upon them, and when the fins were expanded had an elegant appearance, contrasted by the beautiful transparent membrane around it. The dorsal fin was also similarly transparent, with a large irregular black spot upon it. The anal fin was perfectly transparent; the tail dark blue, edged with light black. The black patch on the dorsal fin was nearly half its length at the posterior part.

The stomach contained much digested matter, but the nature of the food I could not make out. The specimen was a female, and contained a fine roe. I preserved the pectoral, ventral, and dorsal fins, in a dried and expanded state.

June 12th, latitude 12 deg. 36 min. south, longitude 32 deg. 11 min. west. Several sepiæ of small size were seen leaping from the water near the ship; they came only a distance of a foot above the surface of the water, and the motion was rapid. Physalia still seen. The towing net was overboard about half-past ten P.M., and, when drawn in, contained only a crustaceous animal, a species of *squilla*, which moved with a slow steady motion around the glass in which I placed it immediately on taking it out of the net, and at other times would dart with velocity about the glass.

[To be continued.]

MEDICAL GAZETTE.

Saturday, September 13, 1834.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

THE ENGLISH UNIVERSITIES AND THE COLLEGE OF PHYSICIANS.

PARLIAMENTARY EVIDENCE.

Few points elicited in the evidence given before the Medical Parliamentary Committee are possessed of more interest, both in an historical and medico-political point of view, than the information obtained relative to the connexion between the College and the Universities of Cambridge and Oxford. It is now put beyond dispute, that this connexion has been one from the commencement rather of expediency or convenience than of any real necessity. The charter of the College prescribes no exclusive privileges for the members of either University; neither does the confirmatory act of parliament; and the partiality shewn to English graduates is explained simply on the ground of the claim which their residence at one or other of the "great seats of learning" gives them on the score of thus having their *moral* propriety duly tested. To such an extent, however, has this partiality been carried, that it has amounted to an almost absolute exclusion of all others: the approach to the fellowship, except through the special favour of the President, has long been known to be virtually interdicted to all who come not by the favoured route. We find Sir Henry Halford giving his testimony on this subject as follows:—

"Has the College any reason to doubt the legality of confining the choice of the electors to the graduates of the two English Universities, there being no mention of any such restriction in the charter, and the act of Parliament confirmatory of it?—I apprehend the College is convinced that it does not necessarily confine itself to the members of the Universities of Oxford or Cambridge: there is a *certain preference* of those members in the mind of the fellows of the College, for they are people who have undergone *a moral and intellectual trial* in the universities from whence they came, to which they are not subject at the foreign universities. They are domiciliated at Oxford and Cambridge, but not at Edinburgh or Aberdeen, or any other universities from whence they may come; and the reason, I apprehend, for the probation of ten years is, that those people who have not been thus domiciled may have an opportunity of giving a proof of their moral qualifications as well as their intellectual ones. That is the reason of the preference of the members of the English universities, in the mind of the fellows, for the election of licentiates from their body." Again—

"Is it the principle of the College to admit into the College as many as possible of the faculty practising in London, who are qualified by their moral character, scientific, literary, and medical acquirements; or is it their object to restrict the admissions as much as pos-

sible to graduates of Oxford and Cambridge? I am sure it is not their object to restrict it to Oxford and Cambridge; but generally they have *a prejudice* in favour of those people, who they know must have been well educated previously to commencing the study of physic."

But this "preference" and this "prejudice," as Sir Henry gently denominates them, have been sanctioned by certain by-laws; respecting these the learned president gives us the following explanation:—

"Have you ever had occasion to look into the origin of the *by-laws* of the College which restrict the admission into the College almost exclusively to the graduates of English universities?—Yes, I am acquainted, I think, with the general principles upon which the College is governed under that head."

"Out of what circumstances do you think the practice referred to, which is not according to the original by-laws of the College, arose?—From the original connexion between the Universities and the College. The first founder of the College, Linacre, established lectures at Oxford. The successor of Linacre founded a college at Cambridge, Caius College; and there has been a natural connexion between the University and the College of Physicians ever since."

"Has the College any reason to doubt, at present, whether the by-law restricting the admissions, except under certain circumstances, to the graduates of Oxford and Cambridge, is a lawful restriction or not?—I take it to be an election, and quite at the option of the College. If a person is once licensed, they may take that person into a fellowship whenever they please, after ten years. That is the rule; he is eligible into a fellowship after ten years."

We shall now extract from the evidence of Dr. Macmichael some particulars relating to the early connexion of the College of Physicians and the two Universities. This gentleman is known to have devoted much of his attention to the history of the College, and of the distinguished persons who have belonged to it: from his acquaintance, also, with the foreign schools of medicine, and with the comparative merits and comparative rank of continental and native practitioners, we must listen to him with considerable deference while giving his opinions on these subjects; a certain allowance, however, must obviously be made for associations with which it is natural to presume that the witness's mind is not a little imbued.

"It has been said that the doctors and first presidents of the College were closely connected with the two English Universities?—I believe the founder of the College, Linacre, founded medical lectures at Oxford, and was himself a professor there.

"Is it not reasonable, therefore, to consider the object of those distinguished persons to have been, not merely to create a medical institution, but the advancement of medical science in this country, and its establisment on a firm basis of literature and philosophy?—I think so.

"With regard to the effect of this connexion of the College with the Universities, has it not, in your opinion, been to contribute very much to the respectability of the profession, and to raise it to a degree of consideration here, such as it has never attained in any other part of Europe?—I have no doubt of that.

"It has been said, that for some years after the first institution of the College, graduates of foreign universities were freely admitted to the fellowship?—The foreign universities, at that period, were the most celebrated schools of medicine—Padua, for instance.

"The state of medical science, at that period, was much further advanced abroad than in England?—It was.

"That will account for the admission of foreign graduates at that time?—The case is so.

"The case is very much altered now?—I should say, there is no better school for the study of physic than the metropolis, for instance, in this country now.

"The foreign schools have no advantage, in point of medical science, over those in this country?—No; not at present.

"Rather the reverse?—Yes, rather the reverse.

"Looking at the history of physic in

England, does it appear that the original connexion of the College with the two Universities has been constantly maintained?—It would appear that most of the distinguished fellows of the College, and physicians, have been connected very closely with the English Universities; I could enumerate them.

"Since when?—Since the foundation of the College itself, 300 years ago; but I will enumerate some: Harvey, for instance, was warden of Merton College, for some period; Freind was the professor of chemistry, at Oxford; Glisson, called the greatest anatomist of his age, was forty years regius professor of physic, in Cambridge; Dr. Heberden was a professor of materia medica, in Cambridge. These are instances I remember."

In addition to these, Dr. Macmichael mentions afterwards, among some of the most distinguished physicians in this country who have been fellows of the College—Mead, Sir George Baker, Warren, Heberden, Baillie, Wollaston, and Young. We now come to Dr. M.'s views of the comparative standing of medical practitioners at home and abroad.

"Have you seen much of the continental schools; of the practice of physic in other countries; and of the relative rank and influence which the profession holds in other countries, compared with our own?—I have been abroad a great deal, some time ago.

"State to the Committee, so far as your observation has enabled you to form an opinion, whether, in any country in Europe, the profession of physic holds so high a rank as it holds in this country, compared with other classes and other professions?—A physician in this country holds a much higher rank than in any country in Europe I have visited.

"Does this observation apply to every class of the profession—physicians, surgeons, and general practitioners?—I think it does.

"What is your understanding of the terms you have just made use of, that physicians have a higher rank in society: do you mean that they are men, generally speaking of them as a class, of greater ability in their profession than the similar class on the continent?—That would be very invidious to answer: they are admitted into the highest society in this country, and are not admitted into the highest society abroad.

"You have stated that the English physicians mix in the highest class of society in this country; have you reason to believe that, with some rare and splendid exceptions, that is not the case with respect to the profession in other countries?—That is the case.

"Generally speaking, the English physician is comparatively higher than the continental?—Certainly.

"To what cause, operating on a large mass, would you attribute such a result?—In the first place, the circumstance of many physicians in this country being educated at the English Universities. There they have the same education as those who fill the highest stations in life: they are brought up with those persons, and afterwards become physicians. I think the distinguished post which they hold, elevates the whole profession; that all physicians partake of the dignity which their education and their good conduct give.

"From your observation with respect to the personal conduct of physicians in other countries, and from what you know by reading also, do you believe that physicians, as a class, stand higher or lower in foreign countries than they do in our own country?—I should say, the English physician ranks highest in moral character and in all endowments, owing to his having had a longer and more liberal education.

In reply to another question, of much the same import Dr. Macmichael says that he considers the general practitioner of this country as holding a rank in society fully equal to that of the physician abroad.

The evidence which we now proceed to extract—namely, that of Dr. Warren—will, we have no doubt, be read with much interest. Its strong conservative tendency, indeed, will be immediately manifest; and the anxious desire which is uppermost in the Doctor's mind, to preserve as much as possible of existing systems, can escape the observation of few. Yet even he admits that "some abatement of the pri-

vileges" possessed by the graduates of the English Universities might be made with advantage. His opinions on the "One-Faculty" question, which will be found in the course of the following extract, we need scarcely say, recommend themselves strongly to us; because they so closely tally with those principles which have been all along advocated in the pages of this journal.

"You are aware of the early connexion of the College of Physicians with the two Universities, and how closely it has been maintained?—I am.

"What do you consider to have been the effect of that connexion on the credit and respectability of the medical profession?—My own opinion is, that that connexion has maintained the medical profession in a higher state in this country than it has attained in any others.

"Do you consider that any changes have taken place lately, either in the condition of society, or with regard to the means of education, which render it unnecessary that that connexion should be longer maintained?—That is a question upon which it is not very easy to give an opinion; but if I am asked my private opinion, I should say the maintenance of that connexion is necessary and useful, but that some abatement of the privileges might be allowed with security.

"Do you think that a better general security could be devised for the manners, and morals, and general education, it becomes a physician to possess, than is afforded by a degree at Oxford or Cambridge?—I hardly think I am capable of answering so large a question.

"You consider that some security is desirable, before the candidate is admitted to the fellowship of the College, with regard to those points of manners and morals, and general education?—Unquestionably, for the advantage of the profession and the public in general.

"Do you consider that the existing connexion has constituted the principal cause of the elevation of the medical profession in England, compared with its position in other countries?—That is my opinion.

"Do you consider that as the cause?—I do not say the sole cause, but one of the great causes.

"In order to the attainment of such a degree, and of a complete professional education afterwards, considerable time and expense being required, is it not desirable that an inducement should be held out to the medical student to employ the time, and to incur the expense for such purposes?—Unquestionably: unless some encouragement be given to such degrees by a superior advantage, such education will not be pursued, on account of its expense, and the length of time required.

"Are you not of opinion that such an inducement is actually held out in the ordinary conditions under which a fellowship in the College is granted?—I am.

"Do you think that a better scheme could be devised for admission to the fellowship, than that these conditions, (that is to say, that of a degree of an English University, and the undergoing a strict medical examination), should be required of the ordinary candidate?—I consider the present plan as a very adequate plan for the security of the College, and the encouragement of these medical degrees. If I am asked whether it may be abated in some degree with security, I have answered that question before by saying, that some abatement of the privilege might be allowed.

"What do you mean by the privileges of which you have spoken?—The privileges are these: that a person who has a degree at one of the English Universities, on passing his examination, receives his licence, and is placed immediately in the order of candidates for the fellowship.

"What is the abatement of that privilege which you would recommend?—It appears to me that the privilege might be abated thus far, that the order of candidates might be abolished, and the Oxford and Cambridge graduates admitted into the order of licentiates, be eligible for a fellowship earlier than other graduates.

"What, in your opinion, would be the effect of reducing the three classes of the profession, medicine, surgery, and pharmacy, to *one faculty*?—My opinion is, that it would be the downfal of all three; that it would reduce those, which are professions now, to a mere trade, and would be very fatal to the character of the whole medical profession, and very injurious to the public. As to setting up *one standard of qualification* for all classes of practitioners,

the effect must be this;—there could not be a sufficient supply of medical men for the public; because if there was but one standard of qualification, and that a high one, few persons would be able to pass that standard; if it was a low standard, it would be useless.

" Do you not apprehend it would reduce the profession to a low level?—Yes, in knowledge, to a low level. If a low standard qualified persons to practise, there would not be much encouragement to learn more than was required by that standard.

" You have stated that the medical profession is in a higher state in this country than in other countries. In what respect is it so?—What I believe is, that the character of physicians in this country is higher, and the character of surgeons too. They are better received in society; they are more learned and better informed men; and, in consequence of those qualifications, fill a station which practitioners abroad do not usually attain.

" Must not the character as to manners and morals of the members of the medical profession be very much influenced by the character as to manners and morals of the society for which they practise?—Some such influence may prevail; but I should apprehend that no well-educated physician, or any physician of character, would ever descend to the morals and manners of the lower order of people.

" Will a person be called on to practise for the wealthier classes of the community, unless he possesses the manners and morals usually required to be possessed by those who mix with that class of society?—If the question refers to physicians, I should say No; because I consider that that class of society look up to a physician as a superior person, and expect superior manners from him.

" Do you not attribute that higher state of manners and morals, which you consider as belonging to the physicians of this country, to the manners and morals of society in which they are called to exercise their practice?—Unquestionably there will be action and re-action there.

" You cannot attribute it solely to their having been educated at the English Universities?—I attribute the cultivation of the high character of physicians to the encouragement given to a high education, and exacted by the College of Physicians from its institution up to the present time.

" Do you think this high standard of manners and morals, which it is desirable for physicians to possess, is confined to those physicians who have been educated at the English Universities?—Certainly not; because we admit many licentiates into the College as being possessed of them.

* * * *

" Do you mean that the having passed through the English universities does imply that a man possesses high qualifications either in classical literature, or mathematics, or natural philosophy?—I would not say that passing through any university of any kind is a test; but it is a presumption.

" A fair presumption?—I do not say a fair presumption. I should think in many instances it is a fair presumption.

* * * *

" Is there not some inconsistency in making education at Universities which possess small means of teaching medicine, the *sine qua non* for admission to the College of Physicians; and in depriving those who have studied at other universities, which have ample means of teaching medicine, of the privilege of entering into the said College?—I can only say what I repeated before, that for the medical education we grant the license; and for the learned education of the Universities we grant the privilege of being sooner admitted into the fellowship.

" Would it not be better to institute an examination of all persons who presented themselves, whersoever educated, whether they possessed the requisite qualifications?—I have a great question as to such a plan answering.

" If it would answer, would it not be more just?—As an abstract proposition, that answers itself.

* * * *

" Is not the bar a profession in which the highest honour and integrity are required, good morals, and good education?—But the bar is a profession which is before the public, and every one can judge of the individual as he produces himself; the profession of physic is a domestic art, and is practised *coram non judicibus.*"

Let us now, in the short space that remains to us for extract, turn to the

opinions of one other witness—one as thorough a university-man as any of those just quoted, yet who can see no such great danger in relaxing the bonds of connexion between the College and the Universities. Dr. James Arthur Wilson has been distinguished in some of the late discussions on reform by which the conclave in Pall Mall has been agitated. But few of the fellows, we believe, are inclined to go all the lengths that Dr. Wilson would lead them,—his views, we understand, being looked upon in the College as rather ultra-radical: the better, perhaps, for our present purpose, as it will enable the reader to compare and contrast them with the sentiments above expressed by some of his brethren.

"Do you think any alterations in the statutes or constitution of the College of Physicians as desirable?—Yes, I do.

"State the nature of the changes you think desirable?—As I have frequently stated, I should think it desirable to get rid of the class of candidates and inceptor-candidates, as confined to physicians graduating at Oxford and Cambridge exclusively.

"Do you mean by that answer, that provided the persons seeking admission were in point of knowledge equally qualified, they should have equal facility to enter the College, wheresoever educated?—Yes, if we could establish an equal qualification of general education, as well as of medical attainments.

"What are the alterations you would recommend?—I would wish in the first instance to get rid of those two exclusive classes, classes into which physicians, not educated at Oxford or Cambridge, cannot be admitted under the present statutes. I would wish to render all physicians who had been approved by the College as fit to practise under its license, eligible after a time, and on the same footing, to the fellowship; that sooner or later, all should become equally eligible by ballot, to the fellowship—I mean after a sufficient period of probation, to which principle of probation I should attach great importance.

"What is the longer period of probation to which you would subject those not educated at the English universities?—If the period of probation were sufficiently long, say four or five years, and at the end of that time the physicians, still practising under the eye of the College, were approved as persons proper by character and attainments for admission into the fellowship, I, in my own private wish, would not have *any* difference made.

"You think it should be made so long as to be a sufficient period of probation for both?—I do."

Here we must close our extracts for the present. We have left ourselves no room for remarks by way of commentary: but we have said so much from time to time, by anticipation, that our readers will perhaps be pleased to compare our views with those of the distinguished persons whose evidence we now have an opportunity to lay before them.

SURGICAL REPORTS FROM THE LONDON DISPENSARY.

By R. R. Robinson, Esq.
Surgeon to that Institution.

Case I.—*Artificial Anus succeeding to the opening of an Abscess in the Abdominal Parietes — Tubercular Peritoneum — Peritonitis.*

Clara Hunt, æt. 8½, admitted 2d June, a delicate strumous girl, was seized with diarrhœa four months ago, which has continued ever since, in spite of several medicines taken to restrain it; has been once sick, but once only, and has occasionally had tenderness in the abdomen, increased on pressure. About two months ago a swelling was observed at the umbilicus, the integuments became red, and an abscess formed in that situation, into which a surgeon passed a lancet, and discharged half a tea-cupful of pus. The mother states, that the same afternoon a large quantity of fluid, exactly resembling that passed from the anus, escaped from the wound, and has continued to discharge from it daily since. The child is worn to a skeleton, and has had a cough the last two days; lungs, upon auscultation, appear healthy; abdomen large (she is naturally large in the abdomen), not hard, not much distended with flatus; there is a small wound, with slight fungus, at the

umbillicus, which discharges an inodorous clay-coloured fluid, sometimes in large quantities, but now in small; several motions daily—frequently disturbed in the night also; tongue white and moist; pulse very small and feeble.

Pil. Hydrar. gr. ij.; Pil. Sap. c. Opio. gr. ij. n. et m. Milk diet.

June 6th.—The purging was restrained upon first taking the pill, but subsequently returned, and is now much as usual, much motion, of clay colour, being discharged both by the anus and the wound.

June 14th.—Died this day.

Dissection.—Putrefaction rapidly advancing; both lungs adherent to the costal pleuræ at their upper part, particularly the left; a few tubercles, mostly small and grey—a few yellow and soft at the upper part of each; bronchial glands enlarged and caseous; heart rather hypertrophied on the left side; the abdominal viscera all matted together by chronic peritonitis, the result, apparently, of scrofulous yellow tubercle, which was extensively deposited all over the large and small intestines; the peritoneum was every where thickened, in parts granular, and of slate colour; the stomach, spleen, pancreas, and kidneys, were healthy, except a few tubercles on the peritoneal coat of the former; liver large, congested, and every where adherent to the abdominal parietes, and the intestines in its vicinity; all the small intestines so completely agglutinated, that it was impossible to distinguish one part from another, or to separate them; the orifice, at the umbilicus, led downwards into a portion of the small intestine; the intestinal mucous membrane, where examined, was of a deep speckled black colour, but not ulcerated; some of the mesenteric glands rather enlarged, in a soft caseous state.

Remarks.—In this case there is some degree of obscurity. I think in the commencement the disease was seated chiefly in the mesenteric glands, as an obstinate purging and marasmus were the most prominent symptoms, and were not relieved by any remedies; to this, I believe, succeeded a tubercular deposit upon the peritoneal coat of the bowel; to this peritonitis, by which a portion of the bowel was attached to the anterior of the abdominal parietes; and that, lastly, an abscess formed around the umbilicus. That an abscess *did form* in this situation I entertain no doubt, as I saw the surgeon who previously attended the case (a member of the College of Surgeons, and an intelligent man), and from him I learnt that the integuments around the umbilicus were swollen, red, and fluctuating, and that *half a teacupful* of pus followed the incision. That the bowel was perforated by *ulceration* and *not by the lancet*, is fairly to be inferred from the number of hours which intervened between the puncture and the escape of the fæces; in which respect there is a striking analogy between this case and the one I last related, where the opening externally, and also into the bowel, were effected by nature alone.

Although, in a professional point of view, this is a highly-interesting case, it might be very annoying to a surgeon in private practice.

Case II.—*Intestinal Tumor—Hæmorrhage from the Rectum—Recovery.*

January 21st, a roundish, fibrous tumor, the size and shape of a kidney-bean, was brought to me at the Dispensary, with the following account:—A large healthy child, æt. five, had for the last six months felt a great itching about the anus, which it was constantly scratching; there was no blueness of the eyes, emaciation, or itching of the nose; but as worms were suspected, about two months ago two large doses of turpentine were given, without bringing any worms away. On the 19th instant the child was playing on the bed, and complained of being wet. Upon examination, the clothes were found soaked in blood, partly fluid, partly coagulated, amounting altogether, it was supposed, to ℥viij., which proceeded from the rectum, and produced slight syncope. Upon looking among the clots, the above-described tumor was found. Castor oil, infusion of roses, and salts, were administered: there was no return of bleeding; no untoward symptoms; and the child now appears in perfect health.

Remarks.—I have never before seen such a case as this; nor do I remember to have read of one altogether analogous. The nature of the tumor seems to be somewhat doubtful, as several medical friends to whom I have shown it have expressed different opinions regarding it: some have considered it a fibrous tumor, others have thought it of a polypous nature (there does not, however, seem any thing like a peduncle), and others, perhaps with the greatest probability, have considered it as one of the intestinal glands in a state of hypertrophy. It is intersected with lines, and has certainly more of a glandular than any other appearance.

The copious hæmorrhage which attended its expulsion, shews either that the tumor itself was very vascular, or that it was attached to the mucous membrane to a considerable extent.

It is fortunate that the turpentine, given under the idea that worms were present,

was not productive of irritation, and may serve as a caution against administering that substance unless some parts of a tapeworm are seen, or more unequivocal symptoms of its presence are manifest than here; as some of the symptoms of worms may be simulated, and closely too, by causes of a totally different nature, irritating the primæ viæ.

CASE III. — *Stricture of the Rectum, with Purulent Secretion, relieved partially by Bougie—Prolapsus Uteri, &c.*

Mary Knight, æt. 39, a washerwoman, constantly in the damp, admitted September 2d; long subject to pain in the loins and rectum; motions always small, voided with difficulty; bowels generally daily relieved, though she has sometimes an interval of two or three days; worse of late; has lost flesh; looks sallow. Some time ago she was the subject of paralysis of the lower extremities, and also of piles; upon which a ligature was applied, which produced great agony, and no ultimate benefit, for the hæmorrhoids returned; motions sometimes flat, sometimes triangular, sometimes round; the size of a large urethral bougie; a purulent discharge, tinged with blood, constantly oozes from the rectum; attached to the verge of the anus are two warty excrescences, and, rather less than $2\frac{1}{2}$ inches from the anus, a stricture which will not admit the top of the forefinger. A hardened and ulcerated state of the mucous membrane below; uterus prolapsed, from frequent straining; appetite bad; nights restless; occasional cough; easier for the last week than for a long time previously, which she attributes to castor oil. A full-sized œsophagus bougie, with Mr. Salmon's curve, was introduced, and passed with facility, six inches; which, as it gave little uneasiness, was allowed to remain a quarter of an hour.

September 5th.—More pain in the back and rectum; motions much the same; instrument again introduced, but retained only five minutes.

9th.—Less pain since last introduction of the instrument; motions decidedly larger in calibre, though she says that has occasionally happened before; purulent discharge copious; instrument again introduced.

Rep. Ol.

12th.—Instrument introduced.

16th.—Motions nearly twice as large in calibre as they were: rather less straining when the bowels are moved. A larger bougie was introduced to-day, with tolerable facility.

30th.—Much the same; instrument regularly introduced twice a week; copious purulent discharge.

Lot. Argenti Nitratis, gr. v. ad ℥j.

October 3d.—Discharge the same; rather more costive the last day or two. The next sized bougie passed with rather more difficulty.

Rep. Ol.

17th.—Bougie regularly passed twice a week; feels altogether better; straining, however, continues.

Has taken for three days, Ferri. Carbon. ℥ss. ter die.

November 14th.—Does not mend. A rectum bougie (No. 7.) has been regularly passed twice a week, though very gently attempted; it produces pain and some faintness, which go off in a few minutes. The size of the motions, the forcing, and discharge, the same.

December 12th.—Continues exactly the same. Nothing appearing to be gained by the bougie, it has been discontinued, and the bowels have been regulated by castor-oil, from which she experiences great comfort.

REMARKS.—The stricture, in this instance, had been left to itself until ulceration had commenced; otherwise it is probable that the bougie would have been successful in completely restoring the calibre of the gut. From the irritable state of the mucous membrane, which the local application of nitrate of silver lotion, and the internal exhibition of steel (for there was great general debility), did not correct, the bougie could not be borne long enough to do permanent good, though at first some benefit was decidedly derived from it; and even now she admits that, by means of castor oil, she passes her motions with much more ease and less straining than she did. She has lately been the subject of a large sloughing ulcer of the leg, from a blow, attended with much pain and some diffuse surrounding inflammation; shewing that her constitution is exceedingly irritable, and her health far from good.

CASE IV.—*Stricture of the Rectum—Ulceration and Communication between the Rectum and Vagina—Diffuse Inflammation of the Buttocks—Phthisis.*

Sarah Hastwell, æt. 39, admitted December 23d. The mother of ten children; was delivered of her last (still born) in May last; subject to piles for some years; had them cut away, without benefit, twelve months ago; soon after which she had purulent discharge from the rectum, which has since continued; until her last confinement her bowels were regularly opened once daily, but since that time she has been costive, going two or three days without a stool, and then requiring purgative medicine; excruciating cutting pain

when the bowels are relieved. Five weeks ago, great pain, increased on pressure, and hardness in the right buttock. An abscess has formed here, which, although it appeared disposed to break outwardly, has discharged matter into the vagina. On passing the finger into the rectum, a tight membranous stricture was detected, which would not admit the point of the little-finger, about an inch from the verge. A few piles around the anus; no pain in the abdomen, or tension. The bowels have been freely opened lately, by castor-oil, salts, and senna; all the motions loose, not figured; occasional rigors; great prostration of strength; nights restless; tongue clean and moist; some dyspnœa; no cough, or thoracic pain; countenance anxious, and somewhat sallow.

Pulv. Doveri, gr. x. o. n. Mist. Cath. p. r. n. Cat. panis.

December 31st.—Abscess juxta anum broke on 27th inst. outwardly, at some distance from the verge, and has copiously discharged good-looking pus. Less discharge from vagina; bowels well opened by the medicine.

Repetantur Med.

January 28th.—More pain in the back; discharge continues at times copious from the orifice, from which occasionally air comes; skin around the rectum warty, inflamed, and painful; bowels open; motions still loose.

Repetantur Omnia.

February 25th.—Has been easier until to-day, and capable of sitting up. Now more pain in the vicinity of the anus; two or three fistulous orifices formed around the original one; numerous condylomata; a little bloody discharge from the vagina; no motion for two days previously; one daily since last report; frequent sickness; no distention of the abdomen; urine free; erysipelatous inflammation around the buttocks.

March 2nd. — Much weaker; much purged yesterday; abdomen distended with flatus; erysipelas continues; labiæ swollen; no discharge; skin juxta anum less tender; much emaciated.

25th.—Has been apparently dying the last fortnight, though still alive; worn to a skeleton; more pain in the back and in the rectum; motions passed involuntary; loose; generally two or three daily; abdomen at times tender, apparently from wind, which distends the bowels; temporary relief obtained upon its expulsion; diffuse inflammation extended over the right hip and knee, but has yielded to poulticing; integuments of the back beginning to slough. Tongue clean and moist; nights often restless, the powder making her sick; half a grain of opium was substituted, in the form of pill, night and morning, with great relief.

April 5th.—Still alive; a troublesome cough, with expectoration, apparently mucous; motions continue to be involuntarily discharged.

8th.—Dyspnœa and cough increased; she sunk, and died yesterday.

Dissection.—Not a particle of fat under the abdominal integuments. Right lung firmly adherent to the chest, occupied throughout by numerous small vomicæ, containing a purulent fluid and tubercular matter. The upper part of the left lung occupied by a moderate-sized vomica, of which softened tubercular matter formed the parietes; the remainder of this lung in a state of engorgement. Heart healthy; stomach large, but free from disease, as were the small intestines, liver, gall-bladder (which was empty), spleen, and pancreas; transverse and descending colon much distended with flatus; the former, instead of being, as its name implies, directly transverse, was angular, dipping down considerably in the centre where the apex of the angle was formed. Mucous membrane of the cœcum, and commencement of the colon, black in some parts, and numerous irregular erosions without induration scattered throughout it. An abscess in the right side of the pelvis, containing ℥ss. of caseous pus. The rectum at its first two inches healthy, but its calibre contracted: from this part down to the verge of the anus, very much dilated in calibre; its parietes lobulated, thickened to a slight degree, and of a gelatinous appearance. Mucous membrane in parts red, in others black, in others deep purple; in some places ulcerated; here and there little tumors upon it; apparently enlarged glands; a tight stricture a little above the verge of the anus, which would scarcely admit a full-sized urethral bougie. A little above this, ulceration through all the coats has made a communication with the cellular tissue on the left side, which is in a complete state of sphacelation as far as the verge of the anus, implicating the skin. An opening below the stricture from ulceration, which communicates with the vagina by a fistulous orifice that will just admit the head of a probe. Cervix uteri rather vascular; lumbar glands undiseased; a thin layer of not very recent coagulable lymph upon the peritoneal coat of the uterus, and the upper and anterior part of the rectum.

Remarks.—The disease, in this case, appears to have been the same as in the preceding, only it has gone farther. The mucous membrane became ulcerated above the stricture, and two fistulous canals

formed, the one into the cellular tissue around the anus, the other into the vagina; the lungs also were diseased. There are few diseases more frequently combined than a phthisical state of the lungs, and a diseased condition of the rectum and fistulous canals about the anus and perinæum; but difference of opinion, I believe, exists as to where the disease originates? It is generally stated that the primary disease is in the lung, but some persons consider the pulmonary disease secondary. As far as I have been able to observe, in *most* instances the diseased state of the lung has *unequivocally* preceded the diseased condition of the bowel; but in *some* instances the reverse has appeared to have happened. In the case to which these remarks are appended, although it is not improbable that tubercles might have long existed in the lungs in a latent state, it is I think clear that the irritation about the rectum was conducive to their development.

In this and the preceding case I proposed to divide the stricture, but in neither instance could the patient be prevailed upon to submit to it, from the agony experienced in the removal of the piles, an operation which it is to be regretted was ever attempted until the more serious disease had been remedied; as it was attended with much suffering, and no benefit.

I was induced to suggest the division of the stricture in these cases, in consequence of my friend, Mr. Luke, having mentioned to me a case of very obstinate stricture of the rectum, which he had divided with the bistourie caché many years ago, and then successfully and completely dilated by graduated bougies.

Another case has come to my knowledge, where a stricture of the rectum low down was divided, and a fistula communicating with the part was laid open at the same time by a common bistoury. It was some time afterwards before much relief was experienced; and just as the patient appeared to be getting well, he caught cold; a purulent deposition took place in the pelvis, peritonitis ensued, and under these he sunk.

Mr. Stafford has related, in a recent number of this journal, a case where division of a strictured rectum by an instrument in principle somewhat analogous to the bistourie caché, bid fair to have been successful, had not the patient been attacked and destroyed by diffuse inflammation of the face and scalp.

In both the cases I have related diffuse inflammation occurred; in one about the leg, in the other about the buttock, extending to the thigh. In the fatal case to which I have alluded above, the purulent secretion seemed to be the result of inflammation of the diffuse kind; and I have lately had under my care a case of malignant ulceration of the rectum, in which diffuse inflammation also occurred about one of the thighs. It appears, therefore, that these patients are very liable to be thus attacked, which is not surprising when their weakened and irritable condition is considered.

It is extraordinary how very tenacious of life some persons, the subjects of disease of the bowels, appear to be. In the case last related the woman seemed in articulo mortis full *fourteen* days before she actually died.

EFFECTS OF MUSIC.

In the *Repertoire Medico-Chirurgicale* of Piedmont, for June 1834, Dr. Brofferio relates a case illustrative of the morbid effects of music. A woman, 28 years of age, of a robust constitution, married, but without children, attended a ball which was given on occasion of a rural fête in her native village. It so happened that she had never heard the music of an orchestra before: she was charmed with it, and danced for three days successively, during which the festivity lasted. But though the ball was at an end, the woman continued to hear the music: whether she ate, drank, walked, or went to bed, still was she haunted by the harmonies of the orchestra. She was sleepless; her digestive organs began to suffer, and ultimately her whole system was deranged. Various remedies were tried to drown the imaginary music; but the more her body became enfeebled, the more intensely did the musical sounds disturb her mind. She sunk at last, after six months' nervous suffering. It should be added, that the leader of the band having occasionally indulged in a discordant *capriccio*, for the amusement of his auditors, the notes which he played produced the most torturing effect when they recurred to the imagination of the patient: "those horrid sounds!" she would cry, as she held her head between her hands. There is nothing so very extraordinary in this case, as it regards the mere repetition of sounds in the *sensorium*, in consequence of a long-continued impression originally made; but that it should be carried to the extent of causing a nervous affection, terminating fatally, is what seems to render the case unique. An anecdote is told of the celebrated Mademoiselle Clairon, which has some analogy to the preceding. A man once shot himself on her account. Ever after, as regularly as one o'clock at night came, Mdlle. Clairon heard the report of a pistol. Whether she was at a ball,—in bed asleep,—at an inn,—on a journey,—no matter; when the moment

arrived, the shot was heard: it was louder than the music of the ball; startled her from her sleep; and was heard as well in the court-yard of an inn as in a palace.

EFFICACY OF ERGOT IN EXPULSION OF THE PLACENTA.

Dr. Worrell, of the U. S. Army, gives an account of the case of a black girl, in whom the placenta was retained three days after delivery. She had severe pain in the region of the uterus; hot dry skin; pulse 120, and very small; a most disgusting stench was observed on removing the bed-clothes; the introduction of the finger gave her intense pain; the edges of the os uteri were firmly contracted and unyielding; by drawing the cord gently, the adhesion was found to be at the fundus. Fifteen grains of ergot were administered, and five such doses were given at intervals of half an hour each. Immediately after the fifth dose, the os uteri dilated, and the placenta came away. The girl did well, with the exception of a slight phlegmasia dolens, which yielded to simple remedies*.

TORSION OF ARTERIES INSTEAD OF LIGATURE.

M. Trier, Surgeon in chief to the Hospital of Altona, has written an account of his experience of M. Amussat's method. He says that at first he had no great reason to congratulate himself on his success, as secondary hæmorrhage in several instances supervened; but by practice he has been made perfect. For several years he has employed torsion exclusively, instead of ligature, and always with success. He has now *twisted* almost all the great arteries of the body; those that supply the brain, and the tongue, the thoracic arteries, the humeral, the radial, the femoral as it passes under Poupart's ligament, and as it runs in the middle of the thigh, &c. He concludes his letter, which is addressed to the Academy, by recommending the method of torsion to the more general practice of French surgeons.

EXTIRPATION OF THE SHOULDER-JOINT FOR OSTEO-SARCOMA.

M. Roux performed this operation lately at La Charité: it occupied an hour and a quarter, notwithstanding all the cleverness and address for which the operator is so celebrated. The patient sunk from exhaustion in two hours after leaving the theatre. The diseased mass weighed about fifteen pounds.

* American Jour. of Med. Sciences, May 1834.

ACARUS, OR ITCH ANIMAL.

M. Beaude, on the 1st of this month, presented to the Academy of Sciences six living acari, which he had that morning extracted from the skin of some itch patients in the Hôpital St. Louis. The insects were found imbedded in the epidermis, at the end of a little furrow leading off from the vesicle. They have never yet been discovered *in* a vesicle.—*Gaz. des Hôp.*

WEEKLY ACCOUNT OF BURIALS,

From Bills of Mortality, *Sept.* 9, 1834.

Age and Debility	54	Inflammation	29
Apoplexy	5	Bowels & Stomach	21
Asthma	7	Brain	3
Cancer	4	Lungs and Pleura	5
Childbirth	5	Insanity	1
Cholera	61	Jaundice	1
Consumption	87	Liver, diseased	3
Constipation of the Bowels	3	Measles	10
Convulsions	43	Mortification	3
Croup	5	Paralysis	3
Dentition or Teething	11	Scrofula	1
Dropsy	16	Small-Pox	9
Dropsy on the Brain	14	Spasms	4
Epilepsy	1	Stone and Gravel	1
Fever	9	Thrush	3
Fever, Scarlet	15	Tumor	1
Fever, Typhus	4	Venereal	1
Gout	5	Worms	1
Heart, diseased	6	Unknown Causes	2
Hooping-Cough	12	Stillborn	12

Increase of Burials, as compared with the preceding week . . . 131

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

Sept. 1834.	Thermometer.	Barometer.
Thursday . 4	from 52 to 66	30·03 to 30·05
Friday . . 5	47 70	29·99 29·97
Saturday . 6	53 69	30·00 30·07
Sunday . . 7	49 67	29·99 29·92
Monday . . 8	48 65	29·70 29·56
Tuesday . . 9	44 64	29·37 29·41
Wednesday 10	49 63	29·66 29·72

Wind variable, S.W. prevailing.

Except the 7th, cloudy; rain frequent during the three last days.

Rain fallen, ·45 of an inch.

Charles Henry Adams.

NOTICE.

An "Old Subscriber" will find the information which he wants in volume xii. p. 630. With regard to the other subject, the arrangements are made, and the publication will commence with our next volume.

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LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture L.

COMPLEX LABOURS.

SYNCOPE NOT PRODUCED BY HÆMORRHAGE OR LACERATION OF THE GENITAL ORGANS.

Both during and after labour, women become occasionally the subjects of syncope, unconnected either with hæmorrhage or laceration of any of the organs more immediately concerned in parturition. In women of a delicate habit, nervous and hysterical, slight faintings under the first stage of labour are by no means uncommon; and in the higher ranks of life, therefore, such complications are most frequently observed. They are also not unusually met with in the abodes of poverty, where a want of proper ventilation and sufficient nourishment combine, perhaps with an habitual use of ardent spirits, to destroy the vigour of the system, and incapacitate it from bearing up against the exertion attendant upon labour. Such cases require but little consideration; the vital powers must be kept up to a certain point by the stimulus either of warmth, fresh air, easily digestible nutriment, or by the judicious use of wine, spirits, æther, or ammonia.

If organic disease exist in any of the viscera, particularly those of the thorax, sudden death may take place, consequent on the violent struggles attendant on the expulsive pains; an aneurism, or an abscess, may burst; or the heart may be choked, or its action otherwise impeded. I was once requested to be present at the inspection of the body of a woman, about forty years old, who had died suddenly in labour of her first child. She had been for seven years subject to great difficulty of breathing, with cough, which had latterly increased, and the sputum had been occasionally streaked with blood. Some hours after the membranes had ruptured, while standing by the bed-side, during an uterine contraction, she seized hold of her attendant's arm; and without uttering an expression, she fell on the floor dead. On opening the body, we found in the two cavities of the pleuræ nearly three pints of serum; the lungs, independently of their compressed state, were healthy; the pericardium also contained a considerable quantity of fluid.

On another occasion I was requested by an old pupil to assist him in investigating the cause of death in a patient, aged twenty-eight, who suddenly expired immediately after having given birth to her fourth child. She had been for three or four years subject to violent palpitations, and much difficulty of breathing, on the least exertion, even walking slowly up stairs; she had constant cough, and occasionally expectorated small quantities of blood. My friend was not called until the os uteri was entirely dilated; the labour was unusually easy; the child was born an hour after he entered the room; and the same pain which expelled the breech, also threw off the placenta. She appeared not to have suffered much from fatigue, and inquired concerning the sex of the child. While, however, her attendant was tying the funis, he observed that she was attacked with a slight convulsion; and before he could get round to the side of the bed, near which her head lay, she had ceased to breathe. The uterus was firmly contracted, and contained a very small quantity of coagula; the viscera of the abdomen were remarkably healthy; the lungs were healthy in structure, but gorged with blood; the heart was small, and very

flaccid; the mitral valve was much thickened, and the communication between the left auricle and ventricle would only just admit the tip of my little finger, as far as the first joint. There were about five ounces of serum in the pericardium. These cases would teach us to watch a patient narrowly under labour, in whom there had previously existed any symptoms of organic disease either in the heart, lungs, or, indeed, any other organ connected with the respiratory or circulating systems.

But a more simple cause of syncope after the child's birth, independently of hæmorrhage, consists in the collapse consequent on the rapid abstraction of that pressure from the abdominal viscera and large vessels of the trunk, to which they had been so long accustomed. When speaking of artificial delivery under placental presentations, I referred to this sudden change in the relative situation of the contents of that cavity, as adding, in no small degree, to the danger of the case; and I have known faintness and death occur quickly after the process of labour had been naturally completed, when there was no hæmorrhage to account for the fatal result, and when dissection neither discovered any organic disease, nor threw the least light on the immediate cause of death.

Such attacks of syncope most frequently follow rapid labours; and women of a relaxed fibre, whose minds possess a gloomy turn,—especially those who have entertained deeply-rooted apprehensions with regard to their recovery,—are most usually the subjects of this dangerous affection. My father states, that it is observed more frequently when the child is still-born, and refers it partly to despondency, the consequence of such an aggravated disappointment. The liberal admission of fresh air, placing the head and shoulders rather below the level of the other parts of the body, the exhibition of repeated small doses of stimuli, the application of warmth to the extremities, abdominal friction, and especially the adaptation of a properly-contrived broad bandage, girt tightly round the person, seem to offer the most effectual means of restoring the tone of the circulating system.

PROLAPSUS OF THE FUNIS UMBILICALIS

by the side of the head or breech, not unfrequently occurs under labour. The loop, however, cannot descend until after the membranes have ruptured; and usually it passes down the moment the liquor amnii is evacuated. The longer the cord is, the more likely is this accident to happen; and should it have gravitated to the os uteri, and collected there in a fold, it is scarcely possible to prevent the coil being carried down into the vagina by the rush of the escaping fluid.

Such an occurrence brings with it not the least danger to the mother;—the labour goes on as well as if it had not happened; for since the space occupied by the fallen funis is most inconsiderable, it cannot impede the regular advance of the process. But the child must always be placed in greater or less jeopardy: the peril is generally extreme, and entirely dependent on pressure. Since the life of the fœtus is sustained by the circulation through the cord, any interruption to the free passage of the blood must produce hazard; and if it be suspended for any length of time continuously, death will ensue, as surely as if breathing were prevented after birth; and it therefore becomes a matter of great moment that we should adopt some plan for its preservation. Not that it is absolutely necessary the fœtus should perish because the funis prolapses; but the chances are much against its being born living unless means are taken to protect it.

Diagnosis.—There will be no difficulty in detecting the cord hanging in the vagina. By its softness, smoothness, and roundness, and particularly by its pulsation,—should the circulation be still carried on,—it may be distinguished both from any part of the child, as well as from any of the maternal structures.

Treatment.—If the pulsation has entirely ceased for some time,—especially if the cord be external, and have become cold, and flaccid,—there can remain no doubt of the child's death; and the subsidence of the funis is of no importance. But if the arteries be still beating, whether in a natural manner or more feebly, it is right that we should attempt to guard them from the pressure which they must more or less experience before the birth is perfected.

With this intention, four methods have been proposed:—carrying the prolapsed cord to that part of the pelvis where there is most room, and where it will be most out of the way of injury;—turning the child, and delivering by the feet;—returning the funis, and keeping it above the presenting part until the fœtus is partly in the world;—and delivering by the forceps as early as is practicable.

It will seldom be possible to preserve the funis from pressure by carrying it to one or other side of the pelvis; for the fœtal head is so adapted to the capacity of the pelvic cavity, as to leave but little space unoccupied. This mode of proceeding, then, is very unsatisfactory, because but of partial benefit; and in few cases can it be trusted to. Turning the child, and de-

livering by the feet, has received the sanction of most obstetrical authorities of the present day, provided the presence of the funis at the os uteri be discovered before the membranes break, or it prolapse while the head remains entirely above the brim. Thus Denman counsels us, "if the child be living, and the presenting part remain high up in the pelvis,—especially if the pains have been slow and feeble,—it will generally be better to pass the hand into the uterus, to turn and deliver by the feet, using, at the same time, the precaution of carrying up the descended funis, that it may be out of the way of compression." But he afterwards utters a sentiment which would render the instruction just quoted almost a dead letter, in these words: "No attempts to save the child are on any account to be made, but such as can be practised without *the chance* of injuring the mother."

Burns says, "as soon as the os uteri will admit the introduction of the hand, the child should be turned;" but if the presentation be advanced before we are called, he recommends "removing the cord to that part of the pelvis where it is least apt to be compressed;" or what is still better, pushing it above the head, because "this is less violent, and safer, than attempts to turn in an advanced stage of labour." Dewees tells us, if the cord prolapses, "turning may be had recourse to—1st, When the uterus is sufficiently dilated or dilatable for the operation; 2nd, When the head is still enclosed in the uterus; 3rd, When there is no deformity of pelvis to defeat the object of the operation." Gooch, after premising that we are not justified in adopting any measure which will endanger the life of the mother, adds, "If we detect a presentation of the funis when the os uteri is nearly dilated, the membranes entire, and the parts in a relaxed state, no one would here hesitate to turn and deliver, as it may be done with ease and safety." Campbell thinks, "that of all the methods proposed for managing these cases when the passages are prepared, or when the labour is not too far advanced, turning is decidedly the most proper; but this practice is not unexceptionable." Hamilton supposes we are bound to turn if we detect a presentation of the funis before the membranes rupture; but that we are not warranted in doing it afterwards. My friend, Dr. Thatcher, also recommends turning, if we detect the cord early.

I have thought it right to cite the opinions of these eminent practitioners at length, because they are at variance with my own, and that you may be in possession of their sentiments as well as mine. No argument that I have ever heard has inclined me to adopt their practice as a general principle; and I perfectly agree with Baudelocque, that "although the accident is dangerous, the precept of delivering instantly, by turning the child, if adhered to indiscriminately, is not less so." He advises, that nothing should be done until we ascertain what course nature is likely to take, and the degree of compression the umbilical cord is suffering; for he thinks that the natural expulsion is often more rapid than the extraction could be. Dr. Conquest, also, seems adverse to the operation of turning under these circumstances; nor does Dr. Blundell sanction the practice.

The objections which I take to this mode of proceeding depend partly on the hazard in which the mother must be involved, under every case of turning, however favourable the attendant circumstances, and however skilfully the operation is performed; and partly on the danger which the child must suffer from compression of the umbilical cord itself during the passage of its shoulders and head through the pelvis; and this danger will be extreme if the pelvis be of small dimensions, or the soft parts preternaturally rigid.

The practice I would recommend for your adoption, provided the subsidence of the funis be discovered before the membranes break, is to keep the patient perfectly quiet in one posture,—to prevent her moving off the bed,—to caution her strongly against exerting her voluntary efforts,—not to leave the chamber on any account, and to be most careful in preserving the bag of membranes perfect. The moment it has ruptured, to introduce two or three fingers of the left hand, or the whole hand, if necessary, into the vagina,—to carry the loop up above the presenting part of the head,—and to retain it there until the next pain comes on, which will most likely propel the head somewhat downwards, while the funis remains above. Should it, however, again descend, another attempt may be made; the fingers need not be withdrawn until two or three pains have been suffered, and it is highly probable that the head will then have passed down so low as to preclude the likelihood of the cord again prolapsing. We may sometimes, however, be disappointed by the loop again appearing as soon as we remove our fingers: if that should be the case after the cord has been returned, a small piece of soft sponge may be introduced, to act as a stay on which it may rest. This mode of proceeding I have once or twice found efficacious; and Gooch used to relate a case in which he also succeeded in saving the child by the same means.

Even in Mauriceau's time this cause

of danger to the fœtus was not overlooked, and he recommended that the descending loop should be wrapped in a piece of linen, and carried within the uterus; a practice still, I believe, followed on the continent. He insists on the necessity of the funis being kept warm; and if this plan does not answer he says we must turn the child. Dr. Colin Mackenzie thought he had improved on this plan, by substituting for the linen a leathern purse, with a mouth that closed by a running string. Neither of these methods possesses any superiority over the piece of sponge; and it is not impossible that the vessels of the funis might be so compressed by their envelope as to suspend the flow of blood through them. The late Sir Richard Croft, knowing the difficulty of preserving the coil of cord above the head, recommended that it should be carried by the hand into the uterus, and suspended on a limb, which would effectually prevent its future descent. He published two cases in the London Medical Journal for 1786, in which he practised this method successfully, and informed Dr. Denman that he had also met with others equally fortunate. His suggestion, however, has not been generally followed; and I cannot myself recommend it, because of the difficulty of accomplishing the object, and because there must always be some risk to the mother in the introduction of the hand within the uterus. This is an operation, indeed, not to be adopted without grave occasion, and a tolerable certainty of being able to accomplish the end for which it was undertaken.

Should the membranes have broken some time before you first see the patient, you will find the same means avail you, if the head be still above the pelvic brim. But if it has descended within the scope either of the long or the short forceps, and the pulsation in the umbilical arteries is quick, weak, and intermittent, (particularly if it should be suspended during each uterine contraction,) while the progress of the labour is slow, one or other of these instruments may be employed to facilitate the birth. In their application, however, we must be extremely careful that the funis is not pinched between the head and the blade, else we shall run into the very danger we seek to avoid, and our interference will be highly injurious, instead of useful. The extraction must be as rapid as is consistent with the mother's safety.

It is always desirable, when the funis descends, to inform the patient's friends of the great probability existing that the child may be born still, and to require that the common means for its resuscitation should be in readiness on its expulsion; and if she herself is inquisitive about the extraordinary attention we think it necessary to pay, we may candidly confess to her that the navel-string has fallen down; and add, the accident does not in the least endanger her safety, but that our solicitude is for the preservation of her babe.

If the funis prolapse by the side of the breech, and the vessels be suffering compression, traction may be made to terminate the labour more speedily; and if it pass down while the child lies transversely, turning must be had recourse to; the operation being undertaken not because the cord descends, but because of the unfortunate situation of the infant.

DESCENT OF THE HAND BY THE SIDE OF THE HEAD OR BREECH

is another complication of labour, by no means so serious as the case we have last considered, but which occasionally is productive of much embarrassment. One hand only may prolapse, or both may at the same time descend. It is owing to the original position of the fœtus in utero. I have already informed you that the most usual situation of the arms is their being crossed upon the chest; but that sometimes one, and occasionally both, are placed against an ear; and when this is the case on the evacuation of the liquor amnii, the descent takes place.

Though not dangerous to the life either of the mother or her offspring, this accident is in a degree unfortunate for both;—for the mother, because the hand occupies a certain quantity of space, and may therefore proportionally retard the labour;—for the fœtus, because the pressure on the cartilaginous structure of the wrist may so injure the limb as to be of serious eventual consequence; and this especially if both prolapse. I have never myself, however, seen any great injury result.

It is not difficult to detect the hand at the brim of the pelvis, even before the membranes break. There is no part of the body with which it is likely to be confounded, except the foot; and the marks I have before enumerated will, if borne in mind, be sufficient to distinguish the one limb from the other.

Treatment.—It will be in your recollection that I directed, whenever the hand was detected at the os uteri, that an accurate examination should be instituted to determine, as soon as possible, whether the shoulder was above; or whether the head, or some other part was presenting; because our treatment entirely depends on the information we then acquire. Thus, if the shoulder present, or the fœtus lie otherwise transversely, turning must be had recourse to, which operation is not neces-

sary if either the head or the breech offer themselves to the finger. Should the case prove such as we are now considering, our duty is to keep the prolapsed limb above the presenting part, that as little impediment as possible may exist to the easy expulsion of the fœtus. With this view, on the rupture of the membranes, the fœtal hand may be embraced between the two first fingers of our left hand, and returned without force or violence, within the os uteri: it may there be kept until two or three pains have propelled the head sufficiently low to preclude the probability of a fresh descent. If we withdraw our fingers immediately we have passed it up, the next pain will again protrude it, and we may find it requisite to return it many times. Should we be foiled in keeping it out of the way in this manner, a piece of sponge may be used, as directed when the funis prolapses; and if it gives us continued trouble, rather than irritate the vagina or os uteri, we had better allow it to remain down, and take the chance of its being slightly swollen. It is not necessary to deliver instrumentally merely because the hand is in the vagina; but if the pelvis be narrow in its diameter, and especially if both arms are protruded, so much room may be occupied by them as materially to interfere with the easy passage of the head, and such symptoms of exhaustion may be induced as will require the application of the forceps, or even the use of destructive instruments, to terminate the labour.

I am pretty well persuaded that many of those cases which we sometimes hear of, where the fœtal hand presents in the vagina, and it is supposed that the shoulder has been raised, and the head brought to the pelvic brim, have been mistaken, and that the child did not originally lie transversely, but that the presentation was the hand by the side of the head. I have myself more than once heard the ease with which this evolution could be effected mentioned, and the superiority of this mode of *turning* over that commonly practised, (which indeed I have recommended), strongly insisted on; and I have always suspected some error in the diagnosis: for I know by experience how difficult it is to push up the shoulder, and bring the head to the os uteri, when the membranes have been some time ruptured.

By the position of this phantom, you may immediately perceive how easily a simple case, such as I am now describing, might be converted into one of the most difficult in obstetric surgery; for if the hand be brought fully down, so as to appear externally, under the supposition, for instance, that it is a foot, the head will be canted over one ilium, the shoulder and chest will be impacted in the brim of the pelvis, and a transverse presentation will be formed, which will require the introduction of the hand, and the version of the fœtus, before the labour can be completed: and this should be an extra warning to us perfectly to assure ourselves of the position of the child before we interfere by traction at a limb.

The development of those irregular formations termed

MONSTERS,

offers many curious objects for physiological speculation, and some of interest even to the practical surgeon: in both of these points of view, therefore, the subject deserves from us a little consideration.

Varieties.—The word in itself, not perhaps the most appropriate that could be chosen, has very improperly been applied to the subjects of disease, such as appears occasionally in after life. Thus the hydrocephalic fœtus is by some considered as a *monster*. If used at all, however, it should strictly be confined to those instances in which some great deviation from normal structure is observed, either as the result of original natural formation, confusion of the organs of two separate children, or irregular or diseased action of a specific kind, such as can only exist in, and influence the organization of, the fœtus in utero. It will be most convenient to follow the arrangement adopted by Buffon, and consider these productions as divisible into four varieties:—1st, those in which there is a deficiency of parts; 2d, those which are redundant in organs; 3d, where the parts are misshapen; and 4th, where, although the organs may be naturally formed, they are misplaced.

Deficiency of parts.—Of all these irregularities, instances in which there exists a deficiency of parts are most commonly met with; and this deficiency may exist in many organs. Among those that can be brought under the examination of the eye, the mouth and lips are perhaps most frequently the seat of this anormal development. Sometimes there is a simple fissure in the upper-lip, forming the single hare-lip; at other times there is a double fissure, and a want of a greater or less portion of the palate; sometimes, again, the palate is faulty, while the lips are perfect: specimens of which are before you. Not unfrequently, also, there is some imperfection in the genitals. The anterior part of the bladder and parietes of the abdomen, just above the pubes, have been found wanting; so also has a portion of the

muscles and integuments round the navel. In the former case, the mucous lining of the bladder is continuous at its circumference with the skin, and forms a soft, red, sensible protuberance in the pubic region; the ossa pubis do not meet, and the recti muscles are separated to some extent. Such an extensive malformation could not exist without disturbing the arrangement of the genital organs. In the latter, the intestines in the neighbourhood of the umbilicus appear to have no covering but the peritoneum, and the chorion and amnion continued from the placenta. Often a large portion of the bowels is received into the cord itself; and cases are on record in which the whole contents, both of the abdomen and chest, were without the protection of their usual parietes. The septum between the ventricles of the heart, as in this preparation [presenting it], and occasionally the diaphragm, have been deficient, or imperfect. Sometimes one or both arms, at others the legs, are scarcely formed at all; and when this is the case, nature seems to make up for the deficiency by granting an extra growth to other parts: thus, in the fœtus on the table, the head and trunk are nearly twice the natural size, while the arms and legs are not more than three inches long. A want of the spinous processes of three or four contiguous vertebræ, is not a very uncommon specimen of monstrosity. This constitues *spina bifida*. There is usually a soft fluctuating tumor in the situation of the malformed bones, caused by water contained within the sheath of the spinal marrow. A midwife under my superintendence delivered a woman, last year, of twins, each labouring under spina bifida situated low in the lumbar vertebræ.

But the most interesting and singular variety of deficient organization, is exemplified in what is denominated the *acephalous* monster. In this, as you see by the specimens on the table, there is a total want of the bones at the side and upper part of the cranium, as well as of the brain and the membranes ordinarily covering it. The *basis cranii* is ill-shaped, and covered by a membrane continuous with the integuments. There is no forehead, but the skull runs backwards from the superciliary ridge. Sometimes, under the membrane at the base of the skull, there is a quantity of soft pulpy matter; but more frequently the spinal marrow commences, as it were, abruptly. The preparations of the acephaloid fœtus (which have been multiplied *ad infinitum*, and specimens of which may be found in every museum), prove that the case is by no means very rare; and they shew also that the brain is not essential to our being while in utero: for many of these children have arrived at the full intra-uterine size—nay, some are actually larger than an ordinary fœtus; as if nature had intended to compensate for the loss of the brain by allowing an exuberant growth to the body. In these instances the nerves are well formed, and even those of the senses which arise from, or if you please terminate in, the cerebral mass itself—such as the optic—are not wanting. Acephaloid children have been known to live some hours, and even days. I myself saw one alive thirty-six hours after its birth, which cried (though feebly), sucked, and seemed to perform all the animal functions much more perfectly than we should suppose. The spinal marrow has been found wanting in some cases, when the brain was deficient.

Redundancy of parts.—Organs are not unfrequently redundant: thus occasionally there are supernumerary thumbs, fingers, or toes; such an irregularity being sometimes confined to one limb, sometimes affecting all. It is evidently both erroneous and unjust to call a child a monster, merely because it possesses a toe or a finger more than the natural number; for the very word conveys a horrible, or at least an unpleasant, impression. It is worth remark, that this deviation from natural formation sometimes runs in families: Meckel has observed this, and two instances have come under my own eye. In the year 1831, two children were brought to my house, twin boys, of a fortnight old, one of them with a supernumerary finger and toe on each hand and foot, the other with only one extra finger on the right hand; the toes had apparently well-formed joints, by which they were connected to the metatarsal bones; the fingers merely hung by a pellicle of skin. I saw the mother afterwards, and found she had a supernumerary finger and toe on each hand and foot, with perfect joints, and capable of motion. She told me she had borne twenty-one children—that all the girls but one were born with extra fingers and toes, but only one of the boys besides the twins were affected in the same manner. She also said her mother and one of her sisters were the subjects of the same kind of irregularity. The other case was somewhat similar.

Sometimes a larger and more important member than a finger or toe is supernumerary. Thus Sir E. Home has described, in the 80th volume of the Philosophical Transactions, an Indian child which had two heads, united together at their crowns, —the upper one being inverted. The subject died of the bite of a snake when it was above four years old. It was found that the two skulls were nearly of the same size — equally complete in ossification. "The frontal and parietal bones, in-

stead of being continued over the top of the head, meet each other, and are united by a circular suture. The two skulls are almost equally perfect at their union; but the superior skull, as it recedes from the other, becomes imperfect, and many of its parts are deficient. The number of the teeth is the same in both. There is no septum of bone between the crania, so that the two brains must have been contained in one bony case." The dura mater of each, however, was continued across, so as to divide the cerebral masses from each other, and their membranes were perforated by a number of large vessels by which the upper brain was nourished. The skull was deposited in the Hunterian Museum, and is now in the possession of the Royal College of Surgeons.

I shew you the skeleton of a double sheep's head, which appears to have arrived at maturity, united laterally, and possessing three orbits—one on each side, and another in the centre. In the lower animals, indeed, monstrosities occur more often than in the human subject, and the domesticated are more obnoxious to them than those in the wild state. There are specimens before you, of pigs, kittens, puppies, chickens, and ducks, with double bodies and extra members; and one of a calf with eight legs.

Two children have been joined together by the back, the sides, and by the sternum and abdomen. In these two preparations, each of about six months, there appears a confused junction of two fœtuses. In one we have two bodies, four arms, four legs, two sets of organs of generation, two ani, and only one head. The other has twice the complement of limbs, while there is only one body and one head. Here is another child, larger even than an ordinary fœtus, which, to one body and the proper number of members, has two heads, each of the same size, attached side by side by a distinct neck, to the upper part of the trunk; and the other is a perfect double child, also at full time, the two bodies being attached together by an inseparable union from the upper edge of the sternum to the pubes. Nor are these specimens by any means singular; but many similar are preserved.

Instances, indeed, are not wanting, of individuals variously joined together by nature, surviving their birth, and even living to maturity. The far-famed Hungarian sisters, who were born in Szony, on October 26, 1701, and exhibited in most countries of Europe, form an instance in point. These girls were united at their loins and sacra; but instead of standing back to back, their faces and bodies were placed half sideways, in regard to each other. They had but one anus and one vulva; their viscera were all double, except that the two vaginæ united at their extremity, and the two recta had a similar arrangement. They were not equally strong, nor of equal plumpness, and were separately affected by hunger and the calls of nature; one was more sickly than the other, and often affected with convulsions, while the other was well. One often slept while the other was awake. They lived till they were nearly twenty-two years old, and menstruated at different times. They died almost at the same instant.

We have lately, in Britain, had an opportunity of seeing an example somewhat analogous to the sisters of Hungary, in the persons of the Siamese twins, who were born in May 1811, and exhibited here in the year 1829. These boys were connected by a band of about two inches in length, and three and a half deep, situated at the lower part of the sternum, involving the ensiform cartilages, and possessing at its lower face an umbilicus. The length of the band allowed them to turn a little sideways toward each other. Their nervous systems seemed to act more in unison than in the case of the sisters; for they both slept at the same time, and one could not be awaked without rousing the other. Hunger affected both simultaneously; they both preferred the same kind of food, and were both satisfied with nearly the same quantity, and at the same time. But the vascular systems were distinct, or had but slight communication; for asparagus eaten by the one did not impregnate with its peculiar odour the urine of the other; and not the least pulsation could be distinguished in the band. Three or four other cases of double fœtuses, who lived for different periods, are on record.

Instances also are recorded of the union of a perfect with a but partially developed body, of which this model [presenting it] of a Chinese, sixteen years old, named A-Ke, with the loins, nates, and lower extremities, of a small parasitical brother escaping from the abdomen between the umbilicus and the sternum, may be adduced as an example. This prodigy, I believe, was shewn in England some years ago. Another case very similar to the last is related by Ambrose Paré, whose testimony, although he deals in many most marvellous stories, is in this instance, at least, not to be treated lightly. The man exhibited himself in Paris in 1530, was forty years old, and had growing out of his abdomen a smaller body, perfect in all its parts, but wanting the head and shoulders. Paré has given a drawing of this, as well as many other monstrous productions, some highly probable, but most of them absurd, and perfectly incredible; and his representation

is just as though the little parasite was endeavouring to burrow its way into the man's belly, and had succeeded in insinuating its head and shoulders;—and Palfyn gives the history of a man having a small body attached in the same way; but in this instance there were arms external, and the head only was wanting. Winslow relates that he saw a girl of twelve years old, well-formed, and of the common size, with the abdomen and lower extremities of another body hanging from the left side of the epigastric region; and in the 79th volume of the Philosophical Transactions there is the account of a well-made Gentoo boy, who had the pelvis and lower limbs of a little brother suspended from the pubes.

Rueffe, Paré, and Palfyn, all speak of a man, alive in 1519, from whose abdomen a small though well-formed head appeared to grow; and Winslow saw, in 1698, an Italian, who had another head, much less than his own, connected to the chest below the cartilage of the third rib. The man felt any impression made on this *extra* head.

Bartholin, who saw the person, and Zacchias, relate the case of an individual named Lazarus Colloredo, æt. 28, of common stature, and well formed, who had a deformed twin brother, John, hanging by the chest from the lower part of the sternum. His head was larger than that of Lazarus; he had two arms, with three fingers on each hand, but only one lower extremity. Respiration was hardly perceptible, but there was evident pulsation in the thorax; he was nourished by the food taken in by Lazarus.

Again, an imperfect body has been found entirely inclosed within another. In the Gentleman's Magazine for December 1748, mention is made of a child born with a large bag extending from the perineum to the toes, which in a few days burst, and a mass of florid flesh protruded, in which were distinctly perceptible a hand and foot, with perfect fingers and toes; but no organs could be traced, or any rudiments of sex. Richerand mentions a lad named Bissieu, who died at thirteen years' old, and who, from his earliest infancy, had a tumor on the left side of the lower part of his abdomen, which was very painful. He was seized with fever and increase of pain in the prominent part, and voided by stool purulent and fœtid matter, and a ball of hair; after which he soon sunk. The tumor was found to consist of a cyst, having a recent communication with the transverse colon, and containing the rudiments of a fœtus. There were discovered a brain, spinal marrow, very large nerves, muscles, and the skeleton of the head, vertebral column, pelvis, and imperfect limbs, with a short umbilical cord attached to the mesocolon. No organs of digestion or respiration, urinary, or generative, could be found. The case was drawn up at length by M. Dupuytren; and drawings were made by MM. Cuvier and Jadelot; and a detailed account was published in the "Bulletin de l'Ecole de Medecine," "Gazette de Santè," 1804, and some other works of the period. A somewhat similar case was published by Mr. George Young, in the first volume of the Medico-Chirurgical Transactions; it was of a child whom he saw frequently during life, in consequence of a tumor in the abdomen, which gradually increased till his death; he survived nine months. A cyst was found occupying a large portion of the abdomen, which contained four pints, fourteen ounces, of greenish limpid fluid, and an imperfectly formed fœtus adhering to it by a conical process arising from the umbilicus. The surface was covered with that sebaceous matter so usually found on the skin of infants at birth; and the skin itself was rosy, and of a healthy look. The extremities were distinct, but short and thick; the fingers and toes were furnished with nails; there was a well-formed penis, and cleft scrotum. There was no brain, nor spinal marrow, nor diaphragm; neither heart, nor liver, nor urinary organs, nor any internal organs of generation. Scarcely any muscular fabric was discovered in the whole mass. The alimentary canal was the most perfectly formed of the internal organs; a part of the intestines, indeed, was in all respects naturally constructed. Mr. Highmore, a surgeon of Sherbourne, in Dorsetshire, opened the body of a boy named Thomas Lane, between 15 and 16 years old, in June 1814, in which he found the rudiments of a human fœtus. The two last mentioned specimens are preserved in the Museum of the College of Surgeons.

Parts misshapen, though properly situated, are by no means uncommon; sometimes this unnatural formation is the result of defective, sometimes redundant organization; thus the different features of the face may be malformed; the scrotum is sometimes cleft; the urethra and rectum imperforate. Club feet are usually classed among this variety of monstrosity; but it appears to me that they usually owe their origin to accidental causes rather than natural formation. The distortion mostly arises from the limb being cramped in utero, owing, perhaps, to the awkward position in which the child lies, or to there being but a small quantity of liquor amnii.

Misplacement of perfectly formed parts is the least common of all kinds of monsters. I do not know that, even among all the ex-

travagant stories in the older works, there is any account of a well-formed arm rising from the pelvis, or a leg from the scapula; and we should certainly not give credence to it, were we to meet with such a tale. But the viscera have been transposed; and such a case may be considered a monstrosity of this description. The most perfect on record, perhaps, is given by Dr. Baillie, in the first volume of the Transactions of a Society for the Improvement of Medical and Surgical Knowledge.

I ought, perhaps, gentlemen, to crave your pardon for having dwelt so long on a subject from which no practical good appears likely to result; but its interest has led me into the digression. Besides, by studying nature in her imperfections and irregularities, we are more likely to arrive at some knowledge of her laws, than if we regard her only in her healthy condition. By learning what parts she can dispense with, we ascertain those organs essential to existence; and by tracing the deviations from her common course, we may perhaps be hereafter led to a more correct acquaintance with her *methodus agendi*.

Origin. — It is not my intention now to enter deeply into the disquisition as to the origin of monstrous formations; if time permit it, I may hereafter, when speaking of *nævus maternus*, again return to the subject, particularly as regards the probability of impressions made upon the mother's mind influencing the organization of the fœtus in utero. But I may cursorily state, with regard to deficient and redundant monsters, that some suppose the germ, before impregnation, is improperly formed; others, that it is an undue admixture of prolific particles at the moment of fecundation; others, that monstrosity has taken place after conception, owing to some irregular vascular excitement, or deficiency of nourishment;—thus the vessels of the redundant part being more numerous, and more active than they ought to be, produce an excess of growth, whilst those of the deficient part are just in a contrary condition. Where a connexion of two children, nearly or quite perfect, exists, I cannot help thinking that union takes place not only after fecundation, but after the fœtuses have grown to a certain size. Take the Siamese twins, or the preparation on the table, as examples. I presume that they were originally true twin conceptions, but that the membranes which ought to have enveloped each body, so as to form an inseparable barrier between them, were imperfect, and that, in consequence, the bodies were allowed to come into close contact with each other; that there is such a strong formative power existing in the vascular system of the fœtus, that when the two cuticular surfaces came together, vessels shot from one to the other, and the parts became permanently united by adhesion, in the same way that two fingers would coalesce, provided the skin was removed and they were kept in apposition. If we can believe it possible for an aperture to exist in the membranes, we can easily understand how a junction of the two bodies might occur.

Richerand tells us, "by placing in a confined vessel the fecundated ova of a tench, or any other fish, the numerous young ones which are formed, not having space sufficient for their growth, adhere to each other, and fishes truly monstrous are produced." And in the vegetable kingdom it is not very unusual for two fruits, in contact and cramped in their growth, to unite indissolubly. Of this we have examples in the lemon, and nuts on the table.

Treatment.—As far as our practice in such anomalous cases is concerned, I have but a few words to offer. It is very possible we might any of us be deceived in mistaking the presentation of the head of an acephalous child for some other part, or we might be quite at a loss to make out what it was. Under such circumstances, as accurate an examination as possible of all the body within reach should be instituted, and probably one or other of the features might be felt, which would lead us to a correct diagnosis. If the fœtus be deficient in the size of any part, or in its members, without a correspondent enlargement of bulk in other organs, no interference can be required, provided the pains be strong, and the pelvis sufficiently roomy; but if it be double, or excessive in development, the common principles before laid down must guide us. The varieties of monstrous formations in excess are so many and diversified, that it is utterly impossible to lay down rules to meet all exigencies. The conduct of the case, therefore, must be left entirely to your own judgment; and the welfare of your patient will depend on the correctness of the views you have formed of natural and instrumental delivery, and on the dexterity you may have acquired by practice. I need not remind you that the infant, if born living, however horrible its form, or however nearly assimilated to the brute creation in appearance, must be preserved with all necessary care. Instances have been known of the wilful destruction of such irregular productions, under the belief that the law authorises their being put out of the world; and parties have been arraigned for murder, who had ignorantly, and to a certain extent innocently, acted on this erroneous impression.

EXTRACTS

FROM A

JOURNAL OF NATURAL HISTORY;

Kept during a Voyage from England to New South Wales, and on the return to England, by way of Batavia, Pedir Coast, Singapore, &c.

By George Bennett, Esq.

[Continued from p. 854.]

June 15th; latitude 16 deg. 20 min. south, longitude 33 deg. 34 min. west. Captured in the towing net, in the evening, a large specimen of the genus Smerdis, of a beautiful vitreous appearance. Length one and six-tenths of an inch; breadth of shield seven-tenths of an inch. When taken out of the glass of sea water in which I had first placed it, it moved by a few leaps or springs; it survived for thirty minutes after being out of the water.

The instant it was taken out of the water, it would close itself by bending the tail towards the inner part of the shield. The shield is partly loose, only being attached to the animal by one-half of its length. The spines on each side of the lower part of the shield had a pinkish tinge upon them. At midnight I captured three specimens of Hyalæa tridentata, and specimens of the S. armata of Leach; also two small species of squilla. The length of the specimen of smerdis was eight-tenths of an inch, and survived out of the water fifteen minutes. The eyes of the smerdis were of a fine golden green colour. On re-examination of the specimen of smerdis on the following morning, the spines of the head and shield were found to have a delicate pinkish tinge, which had not yet faded, although the animal was dry. The lower edges of the shield had collapsed inwardly, giving the animal an appearance as if it had been pressed. The small species of smerdis had not the pink tinge on the spines. In the large specimen there are four spines on the tail; two short anteriorly, and two long ones posteriorly.

June 17th; lat. 17 deg. 42 min. south; longitude, 32 min. 55 deg. west. —A few flying-fish seen this morning; the flight of one appeared to be six feet above the surface of the water. Captured by the towing-net phyllosoma, smerdis (vulgaris), biphora, squilla, &c.

June 18th; latitude 19 deg. 10 min. south; longitude 32 deg. 59 min. west.—At 10 P.M. captured in the towing-net several specimens of smerdis (armata?) alima hyalina and phyllosoma (commune). Attached to one of the smerdis was a filament of about fourteen inches in length, attached to the thorax of the animal: when in the water, this gelatinous filament is much contracted. There were others of the same genus that had this filament attached to the thorax. The length of A. hyalina was two inches; shell, in breadth, nearly 3-10ths of an inch.

June 23d; latitude 29 deg. 28 min. south; longitude 26 deg. 41 min. west. —An albatross (D. exulans) seen this day; the first during the voyage. Towing-net overboard, but only a few specimens of smerdis (armata?) captured.

June 24th; latitude 30 deg. 14 min. south; longitude 25 deg. 30 min. west. —Light airs, nearly calm. This morning our attention was attracted by a huge whale, which moved steadily along, occasionally rising, when its huge black body would be seen emerging from the fathomless deep. On rising, it blew, sending out a small quantity of water, not to any height, but in a bushy form. It was probably a physeter, and may have been from forty to fifty feet in length. The light airs and smooth sea at the time gave us an opportunity but rarely met with of seeing all the movements of this "colossus of the deep;" and it was an interesting sight.

It is asserted that the back of the whale is so very sensitive, that if it rises under and merely touches the boat, it sinks again immediately; so that no danger is dreaded from an unwounded or unirritated animal rising underneath and upsetting a boat.

When whales are seen, it is usually expected that there are abundance of molluscous animals about. The haze proceeding from a column of water thrown up by a whale, has been seen at a distance of seven miles, and remained by the watch for upwards of a minute. The spermaceti whale spouts the column of water low, and in a bushy mist; the black whale throws it up like a *jet d'eau*.

A few pintados or Cape petrels, "Cape pigeons," as they are popularly called,

were about the ship for the first time during the voyage.

June 25th; latitude 30 deg. 01 min. south; longitude 24 deg. 18 min. west. —At 3 P.M. the towing-net captured a solitary molluscous animal, which differed from all others that I had captured or seen before. In length it was sixteen inches, and was merely a filament of that length, to which at irregular distances were attached several small horn-shaped bodies, varying in length from 2-8ths to 3-8ths of an inch in length, and of a beautiful and delicate pink colour, which soon faded after the animal was taken. The portion of the animal from which the filament proceeded was perfectly globular, about the size of a hemp seed, and had a resemblance to an eye. When placed in a glass of sea-water, it did not display any vitality. This curious animal is probably the Stephanomia amphitritis of Peron.

Several luminous salpæ, which gave out a quantity of phosphoric matter on being rubbed on the hand. There was no luminosity of the ocean this night, except a light flash at very long intervals, probably occasioned by the *salpæ*.

June 28th; latitude 30 deg. 38 min. south; longitude 21 deg. 10 min. west. —Several whales about the ship. The spouted water rose in a misty form, which remained, borne before the wind, visible for ten seconds before it was dissipated.

July 2nd; latitude 30 deg. 15 min. south; longitude 17 deg. 9 min. west. —Cape petrels numerous. Albatrosses have been rare. One of the Cape petrels was captured this morning by a small hooked line from the stern. It is the *damier*, or *petrel du Cap*, of the French; *peintada*, of the Portuguese; *Procellaria Capensis*, of Linnæus; *pardela*, of the Spaniards. Head dusky black; breast white; back, neck, upper part of the tail and wings, beautifully spotted with black. Length of the specimen, 14 inches; expansion of wings, 2 feet, 8 inches. Bill black; length, 1 inch and 8-10ths. Length of the leg to end of middle toe, 4 inches and 1-10th. Breadth of foot, 2 inches and 4-10ths. Leg black; foot black, spotted with white, particularly near the edges of the toes. Down underneath the feathers, blackish; skin white; tail feathers black at the tips; tail rounded.

When brought on board, it discharged from the mouth a quantity of masses of food, mingled with oil. After twice discharging the contents of the stomach, it ceased; but after death a quantity of oil would dribble from the beak, if the head was held down.

This petrel is said to be found from 24 deg. to 60 deg. south latitude. Wings crossed, when at rest, extending to about an inch beyond the tail. Tongue 1 inch and 1-10th long; posterior part finely dentated; form obtuse; œsophagus large, longitudinal folds running down the interior: the commencement has two short processes, fringed at the edges, and directed downwards. Liver, two lobes nearly equal in size, right probably the largest; a small narrow fissure at the inner part of each lobe, very distinct. The intestines had a beautifully injected appearance; first stomach curved from left to right: a line of demarcation separates the œsophagus from the first stomach, and a valvular apparatus is placed at the opening from it into the gizzard, or second stomach: bile fine grass-green. The second stomach contained a quantity of the beaks of cuttle-fish, intermingled with much oily and fatty substance. Kidneys of a dark chocolate colour, three lobed; anterior the largest. Pancreas placed in the fold of the first convolution of the intestines; long, whitish colour; divided into two unequal lobes, both sending an excretory duct, which opens into the intestines at about the second curvature. Gall-bladder is placed in a little depression on the right lobe, but unattached to it. The gland over the orbit is situated partly in a semi-lunar narrow depression over the orbit, and partly on a dense membrane continuous from the margin of this depression, extending over the eye, attached by two orbital processes, completing the superior part of the orbit. Why, we may inquire, is part of this cavity membranous and partly bony? A very dense fascia covers the glands on each side, at the same time being continued over other portions of the head, and appears to bind the glands firmly in their situation.

On the body of the animal, among the feathers, were numerous parasites of two genera; one appearing to belong to *ricinus*, and the other probably a *gyropus*. The second were most nu-

merous, of very minute size; the first was larger, being rather more than 1-10th of an inch in length.

July 30th; latitude 40 deg. 35 min. south; longitude 75 deg. 14 min. east. —The Cape petrels, before so abundant, were now rare. I only saw one or two flying about the ship this day. These birds, when flying, have one longitudinal black mark on the upper parts of the wings, and the margins of the wings are black all round.

August 14th.—Several of D. fuliginosa about the ship: lately they have appeared of smaller size and lighter hue than those before seen; perhaps young birds.

The albatross seen with a white body and black tips to the wings, is considered by Lesson a young specimen of the large white or wandering albatross; latitude 40 deg. 43 min. south; longitude 130 deg. 50 min. east.

August 19th; latitude 40 deg. 30 min. south; longitude 138 deg. 3 min. east; King's Island distant 368 miles.—We have this day been almost deserted by our feathered visitors: for the last few days but few have followed the ship: only one albatross, (of browner plumage, but same size as spadicea) has been seen, and a solitary whale bird this day.

August 20th; latitude 40 deg. 34 min. south; longitude 141 deg. 19 min. east. —Several albatrosses about the ship (New Holland species); a solitary Cape petrel, and a few whale birds.

On making King's Island, on the 21st August, the species of tern, with a hurried flight, called "king birds," were seen; but these birds, (which are not seen out of sight of land, except blown off), as well as the albatrosses, as we near the land, do not fly after the ship as before, but skim the surface of the water to some distance to windward and leeward of the ship.

No Cape petrels were seen this day; but on arriving to the eastward of Bass's Straits we again saw them.

I left Sydney on the 14th of March, 1833. The New Holland species of albatross was usually about the ship since leaving Sydney.

March 27th; latitude 44 deg. 23 min. south, longitude 145 deg. 48 min. east. —Sooty albatrosses very numerous about the ship; their range does not appear to extend further to the eastward than 144 or 146 degrees. Several of those about the ship this day had the plumage of the back as if sprinkled with snow, which appearances were probably the effects of age.

April 10th; latitude 40 deg. 56 min. south, longitude 130 deg. 30 min. east. —Albatrosses still about the ship. I remark that in calm weather, or when the wind is light, these birds alight on the water to take their food; but during strong breezes they seize the floating object remaining on the wing.

We lost the albatrosses April 21st, in latitude 34 deg. 58 min. south, longitude 117 deg. 5 min. east.

I have often remarked, that when near land (except among desert rocky islands), the albatrosses or petrels that may be about seldom or never follow the ship, as they are accustomed to do when at sea; they disregard the presence of the vessel, and are apparently more intent in seeking their food on the surface of the water, as they fly very low.

We did not see a single specimen of the Cape petrel (P. capensis.) We saw numbers when we passed this way in August, both to the east and west of Bass's Straits. The reason most probable to be given for it is, that they retire during this season of the year to the rocky islets, for the purpose of breeding.

On the 2d of May, in latitude 23 deg. 11 min. south, and longitude 109 deg. east, off the coast of New Holland, a number of specimens of Zoea pelagica were captured. The body was transparent, eyes of a beautiful ultra-marine blue, with three spots of a similar colour nearly joined, situated at the base of the dorsal spine. Dorsal spine and spiniform rostrum clubbed; the clubbed extremities were of a pinkish colour.

	Inches.
Length, including the long spiniform rostrum and dorsal spine	1 1/8
Length of the dorsal spine.....	3/8
Length of the spiniform rostrum	5/8
Lateral spine	1/8
Length of the body	1/8

These crustacea were captured in great numbers on this day; and when placed in a glass of water, moved about by aid of the tail, with great rapidity of motion.

May 7th.—A number of "boobies" flying about; some occasionally near but most of them at some distance from the ship: there appears to be two species,

one having the whole of the body white, except the tips of the wings, which are black, and the under surfaces of the wings are speckled with black; the other species is not quite so large, and has the plumage of a sooty colour: they go down upon the water to feed, and skim close to the surface for the most of the time. They appear to be the species P. sula and fiber of Linnæus—the latter the brown booby. In the evening none were seen; I suppose they had retired to roost on Christmas Island, which at 6 P.M was about 40 miles distant. Not one came near or alighting about the ship, as is usual with these birds, and for which they have acquired their popular appellation.

May 8th; latitude 9 deg. 36 min. south; longitude 105 deg. 9 min. east.—Captured, in the evening, specimens of the genera salpa, smerdis, phyllosoma, alima, porpita, physalia, and several small fish; one of the phyllosoma was of large size, and differs from the published species, in having the first pair of legs very slender, and destitute of the usual appendages: with respect to the antennæ, it resembles the P. laticorne of Leach; the joints are of a bright red colour; it measured—

	Inches.
Length of the body	$2\frac{1}{8}$
Breadth of the body	$1\frac{2}{8}$
Length of the external antennæ	$\frac{1}{2}$

With this specimen, two legs also, with red joints, were captured, appearing to belong to a much larger species; the longest was $3\frac{1}{2}$ inches long, the other 3 inches. I have preserved them in a dried state. Among others captured were a small red species of cancer, several small physalia, specimens of alima hyalina and phyllosoma clavicorne of Leach, and two specimens of hyalœa, of a fine pink colour.

I have never yet succeeded in capturing the phyllosoma, or leaf insects, by daylight: we must therefore regard them as night insects.

May 9th.—Towing net in the evening, 9 P.M.: numerous large specimens of alima hyalina, several of which had a very long filament attached to them (similar to what I had before seen on the voyage out, attached to some other crustaceous animals); also specimens of phyllosoma clavicornis and commune of Leach; some specimens of luminous salpæ. A small fish, one inch and a half in length, of a bluish-green colour on the back, abdomen silvery, and a species of syngnathus, or pipe-fish, $3\frac{6}{8}$ inches in length.

Latitude 8 deg. 6 min. S.; longitude 105 deg. 7 min. E., a "booby" was caught; which may be the species "maculatus," but differs from it, as well as from other species, in having light orange-coloured feet and legs. Its dimensions were as follows—

	Ft.	Inches.
Length, from extremity of back to the end of the tail	2	3
Length of the beak	0	$3\frac{1}{2}$
Expansion of wings when open,	5	$4\frac{5}{8}$
Length of the first joint of the wing	0	$6\frac{7}{8}$
Length of the second joint of the wing	0	7
Length of the third, or pen-feather joint, (fourth joint)	1	3
Breadth between the wings	0	$2\frac{6}{8}$
Length of the tail	0	$10\frac{1}{2}$
Length of the leg, to the termination of the middle toe	0	5
Expansion of the web	0	3

The first pen-feather is the longest. The upper part of the head and neck is of a dirty white or pale yellowish colour. Back, upper part of the tail, and wing coverts, of a speckled cineritious colour. Breast, abdomen, and urupigum, white. A bare space round the eyes, of a light-blue colour. Middle tail-feather the longest, and projects two inches beyond the others. Irides pale-brown; bill blackish; face reddish; feet of a light orange colour, as also the legs. Under parts of the wings white, speckled with black. The specimen was a male. In its stomach was a quantity of very small beaks of a species of sepia.

May 10th, latitude 7 deg. 6 min. S.; longitude 105 deg. 20 min. E.; several specimens of alima hyalina were captured, and also one of those ribbon-shaped molluscous animals, seemingly allied to the leptocephalus of Shaw, measuring $5\frac{1}{2}$ inches in length. It had a slight vermiform motion when first placed in the water, and I thought the gills expanded, as in fish. It died in a few seconds after its removal from the water.

The minute black spots down the centre and back of the leptocephalus (?) is more distinct in the specimen I captured last night (10th) than I have seen it in those before taken.

There is evidently more luminosity of the ocean seen at one season of the year

than at another, and very probably depending on the migration of the animals which produce the beautiful oceanic phosphorescence. Passing down the straits of Malacca, in July and August, there were only a few scintillations upon the surface of the water; not the extreme degree of brilliancy witnessed in the same localities at other seasons.

Naturalists are not yet satisfied with the cause even of the luminosity in the glow-worms, fire-fires, or luminous centipedes; and when the discovery of the cause of the luminous appearance in those insects remains hidden from us, why should we be surprised at our not being as yet able to ascertain the cause of so many of the marine animals being endowed with a similar property for some "all-wise purpose?"

I have seen the luminous centipede, in many of the tropical islands of the Southern Pacific Ocean, giving out their beautiful emerald green light of colour, at times more or less vivid, having also a peculiar odour. When this phosphoric matter is rubbed off from the insect upon the fingers, it smells not unlike muriatic acid. In this, as well as in all the luminous animals, it can be readily rubbed off upon the hand.

I have been informed that about a ship, when on the Malabar coast, a luminous appearance was seen around the vessel, giving out vivid flashes of phosphorescence, and extending for a short distance around, whilst the whole of the other parts of the ocean around was wrapped in darkness. This singular appearance was observed in three separate instances during the night; but, excepting these periods, no other luminosity was observed upon the ocean.

[To be continued.]

ON THE

MECHANISM OF THE SOUNDS OF THE HEART;

Being the Analysis of the Second Part of a Memoir read before the Academy of Sciences of Paris, 11th August 1834.

By M. Magendie,
Member of the Institute of France, &c.

In the first part of his memoir* the author attempts to prove that the sounds of the heart are neither owing to the action of the blood on the heart or great vessels, nor to any movement of the cardiac valves, but that they arise from two distinct shocks which the organ communicates to the walls of the chest; one of them being produced by the apex at the moment of the contraction of the ventricles, the other by the anterior surface of the heart the moment the ventricles are dilated. Now, if the mechanism of these sounds be really thus simple, may we not hope to arrive at a plausible explanation of the numerous modifications of them, caused by disease, and differing in frequency, intensity, and other circumstances,—modifications which begin to be every day more and more appreciated, as by them the physician is able to distinguish the various organic lesions of the heart? This is the question that M. Magendie undertakes to answer in the present part of his memoir.

The chief varieties in the sounds of the heart, produced by disease, have reference to their frequency, their regularity, or their intensity. As to the frequency, this depends purely on the acceleration or retardation of the sounds of the heart: the same is nearly the case with respect to the intensity; and every moral or organic cause which augments the force of the shocks of the heart, striking against the thoracic parietes, will increase the intensity of the sounds. So considerable sometimes is this increase, that the sounds of the heart may be heard at some distance,—in an adjacent room, for example; and they will communicate to the bed and alcove, in which the patient may be lying, a very sensible vibration.

Instances of this nature are observed in those persons whose hearts strike with force against spacious and sonorous chests, occasioning sounds the intensity of which indicates the violence of the shocks which produce them, and physical qualities of the body that receives the shock. When, on the other hand, the pulsations of the heart are very feeble or almost imperceptible, as is often observed in the blue stage of cholera, the sounds of the organ can no longer be heard. These sounds, however, are sometimes inaudible, even when the heart proceeds regularly in its functions, and continues to maintain the circulation unimpaired. This is a fact which, on most of the hy-

* Med. Gaz. present vol. p. 440.

potheses that have been offered, appears to be inexplicable. Surely, if these sounds were produced by any mechanism within the heart, it could scarcely happen that while the organ continued to act, and that too with great energy, they should become imperceptible. The only way of accounting for it, would be to allow that the surrounding parts have become unfit for the transmission of sonorous vibrations,—which seems not very admissible. In the new theory, on the contrary, the circumstance offers no difficulty. The causes which operate in the prevention of the heart's sounds are various, but all reducible ultimately to the suppression of the shock against the walls of the thorax.

Thus, in hypertrophy of the heart, when the disorder is in its first stage, there is for a time a gradual increase in the intensity of the sounds, which evidently results from the continually increasing shocks in consequence of the enlargement of the volume, and the augmentation of the contractile energy of the organ. But the time comes when the force of the heart's contraction continuing to increase, and becoming excessive, the cardiac sounds are no longer heard; yet the ventricles contract and dilate, the blood enters them and is expelled, the circulation is complete and very active: if the hand be applied to the chest, violent impulses are felt; the chest itself is visibly shaken. But the volume of the heart is now so great, that there is no space left between it and the walls of the thorax. Hence it is that there is no shock, in the proper sense of the word; there cannot, therefore, be a sound. But if, by well-directed treatment, we succeed in diminishing the volume of the organ, a space again occurs between the heart and the parietes of the chest; and consequently the shock is renewed and the sound is heard.

Another cause of a very different description, which operates in keeping the heart applied to the chest, and thus extinguishing its sounds, is the presence of a fluid effused in the right side of the chest, which occasions a pressure against the organs contained in the cavity of the left. M. Magendie has had an opportunity lately, at the Hôtel Dieu, of recognizing a case of this kind; it was that of a woman who, labouring under considerable hydro-thorax, only presented the first sound, that of the apex, the second having completely disappeared. The details of the *post-mortem* examination explain why one of the sounds was suppressed, while the other was maintained. Experiments also on living animals support M. Magendie's views. Thus, if the heart be held close to the chest, by means of a finger introduced into the sternal cavity, the sounds are no longer heard.

In the first part of the memoir, it was shewn that if by any means the contact of the heart with the walls of the chest be prevented, the sounds of the organs are not produced. Now certain conditions of the parts contained in the pectoral cavity may give occasion, in the ordinary state of health of the individual, to the same result as is obtained in the experiments on animals when a mechanical obstacle is interposed. Thus, let a portion of the left lung be found habitually situated between the apex of the heart and that spot which is commonly struck by it, and the cushion thus formed will dull or destroy the sound which would otherwise be produced. This cause, however, but seldom interferes, for the lung is very rarely placed between the parts from the shock of which the sound results: but other substances of a morbid origin may be placed in that situation. In a young woman, for example, who died at four-and-twenty, of a congenital affection of the heart, the first sound alone was audible. On examination after death, it appeared that the suppression of the other was owing to the presence of albuminous layers, arising from an old pericarditis, and which enveloped almost all the heart except its apex.

Accumulations of fluid in the membrane which surrounds the heart do not in general obstruct the sounds. This is because, in fact, there is nothing to impede those sounds: there is nothing to present an obstacle to the movements of the organ immersed in fluid, and moving without difficulty, as long as the accumulation is not excessive. When, however, it becomes so, the fluid throws back the base of the organ, keeps it at a considerable distance from the sternum, and prevents both the shock and the noise.

This remarkable fact was noticed by Laennec, who, however, did not venture to give a reason for it, as it was, indeed, inexplicable on his hypothesis. The hypothesis of Laënnec would have led to

conjectures more nearly approaching the truth, if it were sought to explain the suppression or modification of the sounds arising from other causes. Thus, in a case observed by M. Magendie, the sound of the apex persisted; but instead of the other sound, there was heard a rubbing noise *(de frottement)*, which was evidently produced the moment the blood entered the ventricles. After death, it was found, as the observer suspected, that the anormal *bruit* depended on a contraction of the right auriculo-ventricular aperture. In consequence of this, the blood, in penetrating the cavity, proceeded slowly, and caused the peculiar sound alluded to—but no shock.

The author thinks this rather a rare modification; for he has never met with it more than once. There is, however, another, which is more frequent, and forms, as it were, a kind of counterpart. Here the sound of the body of the heart is normal, but that of the apex is either altogether inaudible or replaced by a well-marked rubbing sound. In this modification the blood readily enters the ventricles; hence the clear sound: but it does not pass without difficulty into the aorta, in consequence of a contraction of that vessel, or some irregularity about the sigmoid valves; hence the passage of the blood being no longer instantaneous, there is no longer a smart check caused by the cross of the aorta—a check which, in the normal state of the organ, carries the apex forward and impels it against the chest. A case was observed at the Hôtel Dieu, by M. Magendie, which seemed to depend on conditions somewhat more complicated. There was found, 1st, a complete imperceptibility of the second sound; 2dly, an extreme feebleness, and sometimes absence, of the first—in the place of which was heard a distinct noise of rubbing, the moment the blood entered the aorta. Now the second of these facts is explicable enough on the preceding hypothesis; but what was the cause of the first phenomenon? There was no hydrothorax in the case, for percussion produced every where a clear sound; nor was there any indication of a hydropericardium; and, accordingly, but two suppositions remain — either that there has been old pericarditis, or there is a dilatation of the organ.

Such, says the author, are the clinical facts of any importance which have been presented to me since I turned my attention to the mechanism of the sounds of the heart. They are certainly not very numerous, compared with what I might have observed in a longer period of time, for the deviations in the physical changes of the heart are multiplied; yet, whatever may be their number, they all, as it may be seen, coincide so completely with my theory, that, whether taken together or apart, they strongly tend to support it. I have by no means yet concluded (adds the author); I have still more to do, but time must supply me with the means. I have more especially to examine the accidental sounds which are occasionally developed in the heart and great vessels, in consequence of certain disorders; and I have to reduce them to the general laws of the production of sound. There are in my possession at present, a considerable number of facts illustrative of this new and curious subject, on which I have made several experiments; and I shall have the honour, on a future occasion, of presenting the results to the Academy in a special memoir*.

STRUCTURE OF THE NERVOUS SYSTEM.

To the Editor of the Medical Gazette.

Sir,

In your abstract of Dr. Macartney's paper on the structure and functions of the nervous system, it is stated, "The membrane mentioned as pervading the entire substance of the brain, and supporting its delicate organization in every part, has heretofore escaped the observation of anatomists; and yet, when the fact is declared, we at once perceive that such a membrane must exist."

In my book entitled Observations on some points relating to the Anatomy, Physiology, and Pathology, of the Nervous System, published in 1822, the following experiments are related, page 83:—

"I divided a portion of the medulla spinalis of the ox longitudinally, and immersed it in the liquor potassæ of the London Pharmacopœia. After some days the medullary matter appeared to be dissolved, when, by adding water to it repeatedly, it was washed away, and suspended in the water so as to form

* Journal Hebdomadaire.

a fluid like milk. What remained, had the appearance of membrane. I suspended this in water, and found it to be that beautiful and cellular structure which I have just mentioned (in the preceding page.) I afterwards put other portions of the spinal marrow into liquor potassæ for twenty-four and forty-eight hours; I then washed them with water, so as to dislodge part of the medulla, and immediately after put them into rectified spirit of wine, which hardened the medullary matter left in the membranes; I then suspended them in water, and found the same beautiful and cellular structure empty, and continued from the portions containing the medullary matter. After I had done this in the ox, I could make out the same structure in the human subject. The cineritious substance is far more delicate than the medullary. The same structure exists in the brain, but it is very difficult to make out so distinctly."—I remain, sir,

Your most obedient servant,

JOSEPH SWAN.

6, Tavistock-Square,
Sept. 13, 1834.

ON THE COMPARATIVE MERITS

OF

DR. BELLINGERI'S AND SIR C. BELL'S WRITINGS

AND OPINIONS ON THE FUNCTIONS OF THE FIFTH AND SEVENTH PAIRS OF CEREBRAL NERVES.

BY G. NEGRI, M.D.

[Continued from p. 848.]

Bellingeri's Opinions on the Functions of the Ganglionic Portion of the Fifth.—Part III.

At the conclusion of the preceding article, we left Bellingeri when he was just going to take into consideration what share the ganglionic portion of the fifth has over the sense of tact of the face, and over those of smell and taste: to this interesting part of our subject I shall at once proceed.

Sensus communis tactus undequaque per faciem, et late per totam externam corporis superficiem diffusus. Obscurus est in capillata capitis regione, exquisitissimus in tunica oculi adnata, membrana pituitaria proprio donatur tactu, est et in palato, dentes carere videntur, sed tactus peculiarem habent speciem, labia delicatulo admodum fruuntur tactu, lingua, et exquisito non solum tactu, sed et gustu pollet, genæ, et frons suum possident non mediocrem tactum, habent tempora, et externa auris, levem quidem in statu salutis, acutissimus tamen aurium sensus est sub morbo. Sed memoratis in partibus tactus a quinto, an a septimo perficitur?—(P. 159.)

Cui ut respondeam, inprimis animadverto eum clar. Magendie, distinctionem esse ponendam in sensu tactus, communem scilicet omnibus animantibus, et per omnem externam hominis superficiem extensum, quo corporum contactum, et temperiei potiores modificationes dignoscimus;.....tactum communem latine indicabimus, quem non ad animales proprie dictos sensus referimus, sed ad sensum communem, vel organicum a Sprengel dictum. Est alia tactus species, qua corporum minimas modificationes asperitatis, levigationis, consistentiæ, ponderis, voluminis, temperiei dignoscimus; hæc cæteris animantibus fere ex integro denegata, quibusdam tantum in partibus homini concessa;.....tactum humanum, vel animalem latino nomine designabimus. Insuper tactum in *physiologicum*, et *pathologicum* distinguimus; primus nobis corporum modificationes tangibiles denotat, et voluntatem in exercitio parit, secundus ingratam excitat sensationem, doloremque producit; per morbum autem omnes dolent humani corporis partes.—(P. 160.)

Communem igitur tactum a quinto dirigi ultro concedam; sensus enim iste non ad animales stricte vocatos refertur, namque et absque cerebri perficitur conscientia, quemadmodum in dormiente homine, in infante, in delirio hysterico quandoque, in insectis etiam, vermibusque, qui cerebrum carent. [Ut melius intelligantur, quæ ante dicta, et inferius dicenda sunt, distinctionem ponendam esse mihi videtur inter vitam sensiferam, atque animalem. Sensifera vita et per solos nervos perficitur absque cerebro, ut patet in animalibus acephalis, atque in monstro a Fauvel descripto, quod encephalo, et spinali medulla deficiente, nulla tamen, licet obscura, dedit sensuum indicia. Animalis vero vita proprie dicta, quæ intimam ponit externarum rerum perceptionem, absque cerebri

concursu perfici nequit.] Hæc autem tactus species omni nervo demandata, et æque ab iis, qui secretionibus, ac qui motibus inserviunt perficitur; ad hanc speciem referimus albugineæ oculi, et pituitariæ nasi tactum, quæ licet ab externeis corporibus acutissime sentiant, nullam tamen cerebro gratam, et tantum dolorificam transmittunt sensationem; quapropter non cum animali, sed potius cum organica vita relationem servat. Si quis vero contendat verum animalem esse tactum in oculi externo, animadvertam, non solas ibi esse quinti paris propagines, sed et plura e tertio pari filamenta in oculi membranis insumi; ita etiam in interna narium inferiorum parte non sola quinti, sed et septimi filamenta accedunt.

Peculiaris vero tactus, quem humanum, vel animalem distinctionis ergo vocavimus, existit et in facie; sed an a quinto dependet? Animadverto in primis, plus minusve acutum existere in fronte, genis, auribus, labiis, mento et lingua: sed ibi non solæ sunt quinti paris propagines, septimi productiones ubique natura deduxit, et ubi exquisitior est tactus, ibi majores, multiplicatæque septimi sunt divisiones. Quot non instituit natura anastomoses inter filamenta septimi paris, et ramum subcutaneum malarum quinti; quantæ non sunt auricularis quinti cum septimo, et cum cervicalibus conjunctiones; desunt ne fortasse septimi cum quinto communicationes in fronte, et naso? Quot non conspicimus repetitas anastomoses inter ramos infraorbitalis, et mentalis maxillaris inferioris cum faciali? Et inde mirari jam desinamus, si labia tam delicato prædita sint sensu. Lingua non a quinto tantum, sed et ab hypoglosso, atque a ramo nervi facialis, etiam in tota sua substantia, recipit filamenta. Quapropter, si ubi sola sunt quinti paris filamenta, dubius, aut imperfectus est *animalis* tactus, ubi et septimi accedunt propagines exquisitus tactus occurrit, a septimo *animalem tactum* perfici, dubium adhuc erit asserere?—P. 162.

§ XXVIII. Modo quid præstent in naribus nasales quinti rami remanet investigandum. Nos edocet anatomes, omnibus in animalibus vertebratis nares dupliei nervo ditari, cetaceis quibusdam exceptis, in quibus deficit primum par, ut de delphino cum Cuvier tradidit Jacopi. Spectabiles nervos primum elargitur par, et neque in omnibus naribus, sed tantum ad superiores, et usque ad medias nares omnibus pariter in animalibus distribuitur, estque nervus iste, qui excipiendis odoribus, et ad cerebrum deducendis inprimis inservit; communi igitur physiologicorum, anatomicorumque consensu olfactorius nervus vocatus. Alii, copiosioresque narium nervi a quinto diversis in punctis suboriuntur, et late per omnem pituitariam membranam undequaque distribuuntur. Exoriuntur porro a ramo nasali, a ganglio spheno-palatino, ipsiusque ramis palatino, et vidiano. Jamvero hæ quinti productiones tantum *aliquatenus olfactui conducunt*, ut sunt verba exactissimi Willis, qui in nervorum physiologia nulli secundus*. 162. (Cereb. Anatom. cap. xvii. p. 329.) Verum puto, distinctionem esse ponendam inter olfactum animalem, et naturalem, vel ab instinctu. Animalis igitur olfactus, quo sensu corporum odores dignoscimus, percipimus, comparamus, a primo pari regitur; hinc, quum grate aliquid olfacere cupimus, vel odorem quemcumque distinguere volumus, vehementer ore clauso, perque nares inspiramus, ut odori halitus sursum ascendant, et usque ad productiones primi paris adveniant.....163. Contra naturalis olfactus, vel ab instinctu, et a quinto perficitur ..... Per hanc autem olfactus speciem homo, brutaque præsertim animalia, noxia a convenientibus distinguunt alimenta, vitæ infestas persentiunt emanationes, et bruta præsertim veneris allicinntur voluptatibus.....Multa etiam a quinto ad nares in homine deduxit filamenta, et miro prorsus ordine disposuit natura. Cur autem surculos a nasali ortos voluit, cur inricato tramite per ossa rursus ad cerebrum alios traduxit, ad nares deinde egressuros, cur alios a ganglio spheno-palatino, alios a ramo vidiano, alios a palatinis deduxit? Miranda prorsus naturæ dispositio. Fortasse, quæ a nasali ophthalmiei natura derivavit filamenta, nullis voluit implexa gangliis, reducia ad cerebrum revocavit, ut et per hosce nervos aliqua odorum ad cerebrum traduceretur sensatio; vel potius, ut sympathiam inter nares, et oculos constituerent: e ganglio enascentes fecit alios, ut præcipue pituitariæ membranæ vitæ organicæ, et muci secretioni congrue cum aliis or-

* See Mr. Swan's article, Medical Gazette, July 19, 1834.

ganis inservirent: a palatinis alios suborios voluit, et nasopalatinum instituit, ut inter olfactum, gustum, et deglutitionis organa intimus intercederet consensus: a vidiano alios progenuit, ut odores in cor, ventriculum, intestina, et genitalia organa recto fere agerent tramite, et mutua esset nasalium, et intercostalis multiplex demonstrata sympathia.

166. Anamalis olfactus nervum natura in penetralibus abscondit, eumque minus frequenter ipso utamur, neque tam extenditur, ut naturalis olfactus organum, et tantummodo diductæ, et amplæ alæ nasi animali olfactui inserviunt colligendis odoribus, sicuti externa auris sonoras colligit vibrationes. Olfactus naturalis animalibus præstat utilitatem non tantum in alimentorum delectu, sed et in respiratione qualitates aëris examinat, qui continuum est vitæ pabulum; inde ore non tantum, sed et naribus aërem attrahimus; nares quapropter ventriculi, et pulmonum custodes, atque tutela.—P. 166.

§ XXIX. Ad gustum tandem deveniamus disputatissimum sensum: quæstio præcipue, an lingualis quinti, an hypoglossus gustui inserviat. Utrinque magni nominis pugnant auctores. Prima nervorum istorum physiologia rudimenta a Galeno tradita jam aperte constituunt, nervum hypoglossum, quem sub nomine septimæ conjugationis designavit Galenus, motorium esse; tertiam vero ipsius conjugationem, quæ cum quinto respondet, gustui omnino præesse *. Willis, Vieusseus, Haller, Meckel omnino Galeni amplexati, rationibus tuiti sunt opinionem. Scarpa inter cæteros agmen ducit, qui, argumentis ex anatome, physiologia, et pathologia desumtis, Galeni asserta confirmare omni animo studuit.....Nervum magnum hypoglossum ex *filamentis partim sentientibus, partim motoriis constare*, tradidit Albinus. (Vid. Tissot, Traité des Nerf. t. i. p. 157.)—P. 167.

§ XXX. In tanta rerum ambage quid statuendam? Lucem ex anatome imprimis, et physiologia mutuabimus. Et primo quidem animadverto, in linguam multiplices sese insumere nervos, ex quatuor nervorum paribus desumtos; scilicet media fere ex parte nervus glosso-pharyngeus, spectabili sui parte nervus magnus hypoglossus, vel undecimum par; fere ex integro tandem lingualis quinti ramus distribuitur, postquam sibi insertam accepit chordam tympani, e nervo faciali, vel septimo pari advenientem.—P. 168.

§. XXXI. Porro nervus glosso-pharyngeus licet, ut cum Portal concedamus (Anatom. Medical. tom. iv. p. 521), propria nonnulla filamenta usque ad linguæ papillas cum aliis nervis distribuat attamen nullommodo gustui ipsum inservire, demonstrant copiosa ipsius stamina, quæ in linguæ basim pone foramen cæcum in papillis calyciformibus insumuntur, ubi nullum absolvi gustum in confesso est apud omnes: insuper vidimus paullo ante, *gangliosum esse* nervum, et linguæ et pharyngis *motibus involuntariis* præesse, demonstrare adgressi sumus. Non igitur nervus gustatorius.—P. 168.

§. XXXII. Inter hypoglossum vero atque lingualem magis ipsamet anatomes dubios nos esse voluit; . . . . vidimus superius, hypoglossum sese inserere in linguæ, et laryngis musculis voluntati subjacentibus, nervus igitur *motorius*, et quidem *animalis est, vel voluntarius*. Sed an et gustui animali famulatur? Comparata præsertim anatomes id denegat. Revera in piscibus, testantibus Dumeril, et Cuvier, deest nervus hypoglossus, deest et loquela.—P. 169.

§. XXXIII. 170. Superest igitur ramus lingualis, quem ex insigni surculo a quinto pari, atque ab ita dicta chorda tympani nervi facialis constare in anatomicis diximus. Multo magis ardua res est disquirere, an lingualis gustui præsit per filamenta a quinto procedentia, an per filamenta, quæ a nervo faciali suppeditantur per chordam tympani: sed attentis cæterorum ramorum quinti, et septimi paris structura, insertionibus, et usibus, lux fortasse affulgere poterit.

Profecto lingualis nervus intertextam præsefert filamentorum structuram, ganglion maxillare efformat, prospicit glandulis maxillaribus, sublingualibus, tonsillis, dat pharyngi ramum, distribuitur tandem omnibus linguæ partibus, muscularibus, membranaceis, glandulosis; in hisce omnibus partibus quam maximas adimplet functiones, quæ ad solam vitam organicam spectant, quas quidem functiones pendere a linguali, quatenus est quinti propago, facile assequemur, si animadvertemus, in cæteris suis divisionibus quintum par vitæ tantum organicæ famulari, et ganglia efformare, et peculiarem præseferre de-

* De Loc. Affect. lib. iv, cap. 2, et De Us. part. Lib. ix. et xi.

scriptam in anatomicis structuram. Sed et cum in lingua animalis sit sensus exquisitissimus, scilicet gustus, ipsum a distincto nervo pendêre putarem. Hic autem nervus, meo quidem judicio, est ita dicta chorda tympani. Revera usquedum probare adgressi sumus, iis in partibus, ubi distribuitur quintum par, et vita animalis adest, non ab ipso, sed a superaccedentibus nervis eam pendêre, et præcipue nervum facialem sensum, et motum animalem tribuere partibus, in quibus inseritur. Igitur et sensus gustus animalis, et ex parte voluntarius linguæ motus a septimi paris propagine, chorda scilicet tympani dependet. Hæc est, quæ lingualem comitatur, et unicum fasciculum cum ipso constituit.—P.170.

Suadet et ipsamet reliquorum organorum sensuum structura, hanc esse lingualis quinti, et lingualis facialis physiologicam explicationem; profecto in visu, auditu, et olfactu, quod ad vitam organicam spectat, præcipue a quinto dependet, quod vero animalis vitæ est, ab accidentibus nervis perficitur; igitur et animalis gustus a ramo facialis, qui cum linguali connectitur, in lingua absolvitur.—P. 171.

[Ingenue fatemur tamen, collatis pathologicis nostris observationibus quinti, et septimi paris, quæ inferius aducenda, admodum opinionem nostram de usu lingualis quinti paris, et lingualis nervi facialis infirmari posse; læso namque quinto, læsus et gustus; morbose affecto septimo, gustus superstes, et integer: verumtamen a quinti affectione lædebatur et vita organica linguæ, quod vitium præsens fuisse, patet in naribus; nil mirum inde si gustus etiam imminutus: in pathologica observatione nervi facialis paralysis tantum erat quoad motum, superstite sensu tactus in facie, et gustus in lingua; quam frequenter vero occurrit paralysis quoad motum integro sensu in parte paralytica; insuper in hoc casu tactus illæsus erat, illæsus et gustus.]—P. 171.

§. XXXIV. Interim dum hæc assero, non omnino ausim denegare, lingualem quinti aliquatenus gustui famulari. Sed quod de olfactu, et tactui dixi, ita et gustum in naturalem, vel ab instinctu, atque animalem distinguo. Naturalis autem gustus ad sensum organicum accedit, ut merito animadvertit Sprengel. (Op. cit. tom. ii. p. 465.) Naturali gustu infans lac sugit, bruta vescuntur, nocua ab innoxiis distinguunt alimenta, vermes, et zoophita ipsa proprio non carent gustu. Porro naturalem hunc gustum a quinto regi dijudicamus; revera ipsomet in homine, licet obscurus sit, imperfectus, et a quibusdam Physiologis denegatus, aliquis tamen gustus est, ubi solæ fere sunt quinti paris propagines, uti in palato, velo pendulo, gingivis, buccis, et labiorum interno. Tradidit inde nobis Lecat, quod lingua naturaliter deficiente, vel per morbum, diversi nihilominus aliquando distinguebantur gustus. (Traité des Sens I. p. 224.) Quæ quidem observatio dum ostendit, et a ramis quinti aliqua ex parte gustum absolvi, demonstrat etiam, perfectum animalem gustum a faciali perfici; ubi enim ipsius desunt filamenta, vel pauca sunt, confusus, et obscurus sensus est gustus.....171. — Præterea in naturali gustu quodammodo affici quintum par, comprobant nonnulla physiologica phœnomena; inde enim intelligimus, cur quinto in gustu affecto, quemadmodum a jucundis alimentis fauces ad deglutionem, ventriculus ipsis accipiendis invitatur, ita a nauseosis, et ingratis constringuntur musculi faucium, et pharyngis, ventriculus ad vomitum sollicitatur; cur etiam ab adore, vel animi pathematibus gustus, et deglutitionis organa per quinti propagines diversimode afficiuntur."—P. 173.

" Naturalis insuper gustus ad vitam organicam refertur, per ipsum enim animantia propriæ prospiciunt conservationi; conveniens erat igitur ordini naturæ, ut quodammodo hic sensus a quinto pari penderet, quod ubique organicæ vitæ famulari hucusque demonstrare adgressi sumus. Quam vero gustus ad vitam organicam spectet, præcipue a cerebelli influxu, juxta Willisii sententiam, dependet."—(Cer. Anatom. cap. xvii. p. 328.) "Quod quodantenus comprobatur observatione Malacarne in maniaco, cui gustus penitus deerat, et cerebellum morbose erat constructum, constabat enim tantum ex tercentum, et viginti quatuor laminis."—(Nevro-encefalotomia, p. 7.)—P. 173.

" Quum igitur firmiter teneamus, organicam linguæ vitam a quinto pendêre, patet, morbose ipso affecto, animalem gustum vel depravari, uti in morbis, vel destrui, aut lædi, sicuti in pathologica quinti adducta observatione vidimus. In coryza quoque, licet membrana pituitaria acutissime ab attactu sentiat, nullus tamen olfactus; vitiatis nervis ciliariis jam cœcitas, vel visus depravatio subsequitur; surditas quan-

doque, aut falsus auditus a vitio musculorum internæ auris.—P. 174.

"XXXV. Verumtamen in lingua duo distincti animales sunt sensus, gustus nimirum, et exquisitissimus tactus. An ab ipsismet nerveis filamentis ambo dependent? Morbi contrarium docere videntur; perit scilicet omnino gustus quandoque, integro superstite tactu. Natura in linguæ apice duos constituit papillarum ordines, fungiformes nimirum, et conicas, quæ, sicuti animadvertit Sprengel, maximam habent eum digitorum papillis convenientiam. An igitur fungiformes præsertim gustui, an conicæ tactui præprimis dicatæ? An animalis gustus a faciali, linguæ vero exquisitus tactus ab hypoglosso? Non longe absum ut credam; *natura enim tangentes nervos in reliquis corporis patribus cum motoriis consociavit.*"—P. 174.

"XXXVI. Quidquid de hoc sit, certum est, in lingua multiplices existere nervos a quatuor nervorum paribus desumptos; et duo quidem vitæ præsertim organicæ dicati videntur, glosso-pharyngeus nimirum, atque lingualis quinti, reliqui duo vitæ ipsius animali præsertim prospiciunt, ramus scilicet lingualis facialis, et hypoglossus. Glosso-pharyngeus autem motibus involuntariis præcipue inservit, parum sensui organico; lingualis vero quinti sensui præcipue organico paucos involuntarios absolvit motus, ramus lingualis facialis gustui ex dicti præsertim, hypoglossus maxime motui voluntario dicatus. Sunt igitur in lingua nervi organicæ vitæ distincti a nervis vitæ animalis; non tam vero aperte nervi sentientes utriusque vitæ a motoriis distinguuntur.—175.

"XXXVII. Ex hucusque dictis quoad sensus consequitur, distinctionem esse ponendam inter tactum, olfactum, et gustum organicum, atque animalem. Sensus isti, quatenus ab instinctu dependent, ad vitam organicam referuntur; hinc nulli animantium generi denegati, et perfecti ab ipso fere ortu tributi; per hosce enim sensus, plus minusve evolutos, animalia propriæ prospiciunt, et tuentur existentiam; et sensus isti etiam per nervos solæ organicæ vitæ famulantes perficiuntur, ut patet in vermibus, et in monstro a Fauvel, et Mery descripto; animalis vero tactus, olfactus, et gustus a distinctis exercetur nervis, qui suam transmittunt cerebro sensationem. Consequens porro erat naturæ instituto, ut naturales, vel organici sensus, et a nervis vitæ organicæ perficerentur; animales contra distincta nervorum classis absolveret. Igitur in nostro casu quintum par sensibus naturalibus præest, vel organicis; animales vero sensus, qui iisdem in organis, ubi distribuitur quintum, insunt, a superadditis nervis dependent. *Statuam igitur, quintum par nervum esse sentientem, et motorium vitæ organicæ in capite. Quæ de quinto pari generatim diximus, de ipsius portione majori tantum intelligenda.*"

I must leave a few remarks which I have to make on this subject for my next article.

I am, sir,
Your obedient servant,
G. Negri, M.D.

September 18, 1834.

SKETCHES OF THE PRESENT STATE AND FEELINGS OF THE MEDICAL PROFESSION IN THE PROVINCES;

And more especially in Liverpool and its Vicinity.

By James Collins, M.D.

I do not know whether our provincial hospitals have engaged as much of the attention of the committee on the state of medicine as they deserve. Perhaps they did not naturally fall within their province, as these institutions are generally supported by private subscriptions, and therefore not under the control of parliament. But as there are many curious facts connected with their management and support, and illustrative of the temper and character of the times; and as the question of receiving their certificates of attendance has been mooted and acceded to by the College of Surgeons, and will probably become a part of the law of the land, it may not be uninteresting, nor out of character with the design of these sketches, to convey some information respecting them in this neighbourhood; for, without this knowledge, it is impossible to have correct notions of the state and practice of medicine here, and how far it may be right or wrong to place them,

in respect to their certificates, on terms of equality with the London hospitals or those of other capitals; for every one must allow that if they are not as well managed, their patients are as frequently seen by their medical attendants;—in short, equal to the latter in the facilities of instruction they afford, they ought not to be so in the scale of our medical legislation.

We have here, properly speaking, three hospitals: the Northern Hospital, the Fever Hospital, and the Infirmary. The first is a new institution, not more than six months old, established in opposition to the last, through the exertions of one or two merchants, who felt annoyed that they could not push into the infirmary, as soon as they wished, a favourite and sanctimonious surgeon, the medical attendant of their own families. It has accommodations for about thirty patients, but is capable of much more extension, if its funds were more ample. There are six medical men, three surgeons and three physicians, attached to it. The laws of the institution require the patients to be seen three times a week, in contra-distinction to those of the infirmary, that exact the same but twice a week. It was originally intended, in order to catch support, and to create an invidious contrast, that the patients of the new hospital should be seen every day by their medical attendants; but this was relinquished as too bold and daring an innovation on the established usages of this place, and as likely to rouse, rather prematurely, the lion from his slumbers; for this hospital has been viewed from its very commencement in a very equivocal and suspicious light by the friends and admirers of the infirmary; and as all the high patronage and subscriptions go with the latter, it required no small tact and mannerism to avoid being shipwrecked by too glaring an opposition. It is now doubtful whether this hospital can go on, in consequence of the late change in the poor laws. Under the old system there was always a ready and ample fund in the poor rates to have recourse to, when subscriptions failed, and when any thing like a plausible reason could be alleged to sanction it. It was the common practice here for men who would not subscribe a shilling out of their own pockets, but who had influence in the select vestry or at head-quarters, to be most generous in appropriating large sums, for the projects and schemes of their friends, out of this fund. The hospital in question, even before it was christened, got, through their interference, a donation from it of two or three hundred pounds. This was to be a regular annual gift, much more likely to increase than diminish, were it not for the late change in the administration of the poor laws. In fact, any thing that could in any way be connected with a plausible tale for the benefit, as it were, of the poor, was sure, when interest was made, of support from this quarter. No one who did not reside in Liverpool could or would believe the facilities that existed in this respect for propping up ricketty schemes and institutions that required money for support. The dispensaries receive five hundred guineas a year from the poor rates, and several thousands are annually paid out of them for the support of the church: but it is now presumed that a stop will be put to these frequent and gross misapplications, and that it is more than probable, since the central board will henceforth have the control, that the Northern Hospital and other institutions will be shaken, if not tumbled, by the change: at all events, it is too small to give certificates of hospital attendance, or to come within the meaning of what are termed recognized hospitals, be its fate what it may.

The Fever Hospital is connected with the workhouse, the same medical men attending both, and consisting of two physicians, one surgeon, and a house-surgeon, who has a salary of three hundred guineas a year, with bed and board. This is the only good situation in the profession here, and is owing to the liberality with which we expend the poor rates; for the merchants will not give the surgeons who go out in their vessels, even to the unhealthy coast of Africa, more than fifty or sixty pounds a year, and generally much less; indeed, many of them not more than thirty or forty.

Until very lately, the senior physician or surgeon of the dispensaries was promoted, as a matter of course, to the Fever Hospital, and thence to the Infirmary, when vacancies occurred. This

was the state of progression always observed here from the minor to the higher charities, no matter how dull, illiterate, and in other respects unfit, the individuals were. Seniority was the order of the day, and the ladder by which incapacity frequently mounted. Formerly the select vestry had the appointment of the medical men, but the rate-payers have latterly got it; and since then frequent attempts have been made to break through, though in vain, this routine of promotion. One of these was made some time ago by an unfortunate Catholic, an Irishman. Nothing could exceed the party and political feelings that manifested themselves: all the bloody scenes of Irish history were raked up against him. O'Connel, the pope, and the priests, were made to pass as by magic before the electors; and he soon felt the terrors they inspired more than counterbalanced any personal or local claims he otherwise would have had. This was the cry that was raised, and insured the election of the present honorary surgeon. The contest was running against him; his friends got alarmed, and soon raised the war-whoop that the church was in danger; that a Popish surgeon was about to be appointed to the Fever Hospital and Workhouse. The scales were soon turned; old maids in terror and dismay were seen canvassing and sounding the alarm in the streets; the clergy and the laity soon joined in the chorus, and such a dust was kicked up about the horrors of Popery, that it was in vain to oppose them. Thus the present surgeon succeeded, and enjoyed a laugh at the credulity of the public. Even the dissenters themselves have little or no chance when a churchman appears in the arena against them. The latter generally brings the influence of the clergy and corporation to his aid; and hence the true spring and secret of his success; and this is the reason why all the medical men, almost without a single exception, are of their party.

The place of physician, or surgeon, to the Workhouse and Fever Hospital, has latterly been no sinecure, as cholera patients are now received there. This disease has been raging among us, more or less, during the last three months, but it has latterly assumed a more virulent and frequent appearance, and is now exceedingly bad. There are no returns of the daily cases made; no Board of Health; no Cholera Hospital; no means, in fact, to form a correct, or even approximate, notion of the mortality. The greatest pains are taken by the merchants and the local authorities to hush all up, and even to deny, against the evidence of their senses, its existence to any great extent, in order not to injure the town by scaring strangers from it, or risking the interruption of its commerce or relations abroad; and so effectually has this been done, that not a single local or other paper has noticed, directly or indirectly, the recurrence of this disease here, though the malady at present is frightful, and the churchyards and undertakers in constant requisition. Many of the poorer patients are carried to the fever wards, and passed there, and in the streets, as fever patients. The mortality is nearly one-half.

But there was a time when it was a matter of great importance to be attached, as a medical man, to the Fever Hospital and Workhouse, when there was a knife and fork, and a bottle of wine, and the other necessaries of good cheer, laid as regularly for him and the pupils, and their friends, as he made his visits. In these days the patients were particularly attended to — frequently seen twice or three times a day. Once a week there was a regular and sumptuous dinner for the medical men and the select vestry, and their friends. Often the morning broke in before they separated. Some of them almost literally lived in the Workhouse, such were the charms it afforded. But things are changed; there are no longer lunches, dinners, or suppers, served up for them as formerly, out of the poor rates. Modern reform has swept away all these things, and changed the whole aspect of affairs, and has left them nothing but the cold and dangerous monotony of their visits to the sick. Some of the older medical men can scarcely believe the evidence of their senses at the change, and now and then heave a sigh for the good old times that are never to return, and often amuse their junior friends with the narrative of their experience in this respect.

The principal hospital of all is the Infirmary, the only one here that comes within the privileges assigned by the College of Surgeons to hospitals of a

certain size, and therefore most deserving of particular notice. It has accommodations for three hundred patients. There are six medical men attached to it—three physicians, and three surgeons. Until very lately the Lunatic Asylum was also under their charge; it adjoins the Infirmary, and almost forms one and the same building with it, and stands in the centre and populous part of the town, one of the most unhappy and inappropriate selections for its site that could be made, when we consider how injurious the noise, and bustle, and want of sufficient space for gardens and grounds, are to the recovery of the insane. But it was an object that these institutions should be near each other, as the medical men of the Infirmary attended both; and therefore the interests of the patients was sacrificed to theirs; for it has always been their policy to prevent other medical men from being appointed to it, and to limit as much as possible hospital practice to themselves. Hence they often undertook to do what they really could not, and upon all occasions threw cold water, if they did not smother, every attempt to increase the number of our hospitals. They supposed the more monopoly they had in this respect, the more professional reputation and gain accrued to themselves. Even the other day, when it was proposed to introduce a few beds into the dispensaries, they mustered and successfully opposed it, though there were funds, spare room, and every other requisite, and necessity for them in abundance. In fact it was this opposition that gave birth to the new hospital, by the disgust and discontent it created. But they no longer have charge of the Lunatic Asylum; for, in consequence of an investigation into some alleged abuses there, they were removed, and made consulting physicians and surgeons; or, in other words, allowed a decent and honourable retreat under this cover. Their removal caused at the time a great sensation; friends and foes opened their batteries for and against; the current was decidedly setting in against them, when the corporation stepped in to their support, and conferred the freedom of the borough on them as an equivalent for their dismissal from the Asylum, and as an act of grace and respect; for however low or disreputable your acts may be, this favour on the part of the corporation, when conferred, no matter in what time, or under what circumstances, was supposed by them and their partisans sufficient, in Scripture phrase, to blot out your iniquities, and to make you walk in the newness of life; in fact, to whitewash you in a moral and physical sense. The transactions I allude to took place some years since; and some idea of their nature, and of the management, in these times, of our public institutions, may be gathered from the statement of Mr. Moss, the banker, a man of large property, high character, and great influence here. At a meeting of the trustees, in March 1826, he stated, "that at the close of the year 1825, he called, in the capacity of President of the Infirmary and Asylum, on the Governor of the Asylum, for an account of all the patients confined there, and the particulars of their respective cases, and that such an account was given him, as far as it was possible to make it out; but that in the list of the patients, he was sorry to say that there were several who had been confined twenty or twenty-five years, and of whom no particulars could be stated; for there was no account of their arrival, how they got in, who sent them there, or admitted them; nor were there any certificates of their insanity among the records." He also stated, "that he was much surprised on inspecting the certificates on which some of the patients were admitted. As a specimen, he brought the one to the Governor on which Lieutenant T. was put in; it was a shabby piece of paper, without seal, or address, or abode, of the medical man whose signature it bore; though, on looking over the rules of the institution, he found that no person ought to be admitted there who did not bring an explicit certificate with him, and that it was a great abuse to admit any without one." (See the Report of the Proceedings in the Commercial Chronicle for January, 1826.) Let me transcribe the words of Mr. Hollinghead, when speaking of these transactions at the same meeting. He stated "that he received a letter from Mr. Peel, in his capacity as mayor of Liverpool, requesting inquiry to be made into the allegations of Lieut. T.'s memorial; and that in consequence of Mr. Moss, the banker, the chairman of the committee, deeming the most rigid investi-

gations necessary, he said, that whenever the committee thought fit to attend at the Town Hall, he would swear the witnesses and take their examinations: but he never heard any thing from them until he received the resolutions of the 31st of March; which struck him with perfect astonishment, and which he transmitted to Sir Robert Peel without comment, leaving him to draw his own conclusions; and that if he had any thing to accuse himself with, it was for shewing too much delicacy towards that Committee who could forget what they owed themselves and the public, so far as to cloak up, as they did in their report, the abuses of the asylum, and say all was right when all was wrong. Mr. Ruston, one of the commissioners appointed to inquire into the corporations, stated, "that some of the medical men did not see their patients in the asylum for six weeks; that a patient of the name of Norris, being asked by the visitors whether he had not been in the habit of administering medicine and dressing the wounds of the patients, said he had, and asked if that was wrong; and he added, that there was evidence of this patient being seen to bring, from the upper wards, a cupful of matter which he had taken from the back of another patient, and of whom he observed, 'Curse that fellow, he will be dead in a day or two, and then I shall be rid of him.' There was proof of his exhibiting a bone which he had extracted from the toe of a boy, on whom he had operated. If insane, why was he permitted to administer medicine and operate on patients?—if sane, why was he not discharged?" These are curious facts, and deserve to be recorded, in illustration of the state of our provincial hospitals a few years since.

Previous to 1820, there existed neither visitors nor books in our Infirmary, or Asylum, to record neglect or indifference there; and consequently there was no means of ascertaining the abuse that previously existed: but of it we may form some idea from the admission of the medical men themselves, that in little more than twelve months (from February 1822 to April 1823) nine instances of medical neglect were registered against them. They were frequently accused of not seeing their patients there more than once in eight or ten days. To this they always replied, that they had done their duty as their predecessors had done—that the practice was to see the patients on Saturday, or once a week, and those whose cases required particular attention, on Tuesday. The laws, however, say that they shall be seen on both days; and when this circumstance was pointed out to them, "they stated, they were not aware of it before, but that since it was pointed out to them, in 1822, they have, almost without a single exception, been attended." Thus they give the extraordinary fact, that though some of them have been attached to the Infirmary, some twenty, others thirty years, it was not until 1822 that they knew it was necessary, by the laws of the institution, to see the patients twice a week. Mr. Ruston, the commissioner, stated, in a letter published in the Liverpool Mercury of April 1824, some extraordinary illustrations of the way our Infirmary used to be managed in those days. He says there, "I tell you, gentlemen, as Guardians of the Liverpool Infirmary, I am willing, when called upon, before a committee of the Trustees, to produce evidence of the following statement:—A person, by accident, was severely injured; his leg was broken, he was taken to the Infirmary, and a medical pupil, instead of a pupil appointed by the surgeon under whose care he was placed, set his leg. He remained in pain, and without examination from any of the pupil's superiors, for nine days, and on the ninth day his leg was again broken by order of the apothecary, it having been improperly set. This patient remained in the Infirmary five weeks, and during that time, he positively asserts, he did not see his surgeon more than four times; and he further states, that at the end of five weeks he left the infirmary a cripple—and a cripple he will remain as long as he lives." In fact, no one who had not personal and local experience of our provincial hospitals, could believe how long we remained, in the march of civilization, behind our neighbours: even to-day, no medical man can pay a visit to them here, without the special license of the physicians and surgeons attached to them. In consequence of the discussions that commenced in public and private, respecting the proceedings in our hospitals, and the best means of reform, the medical men of the Infirmary stated to Mr.

Moss, the banker, that, if they would cease, they would sign a paper agreeing that, at the close of every ten years, the medical men should retire from the Infirmary, and others be appointed. The discussions accordingly ceased, and this document was deposited in the hands of Mr. Moss. They afterwards refused to comply with the terms of their own agreement, and did not hesitate, in the face of a public meeting convened to ratify it, to break their own solemn contract; some of them alleging, in excuse, that they had an opportunity, since they agreed to it, of seeing Sir Astley Cooper in Liverpool, and that he thought five and twenty or thirty years ought to be the limit of an hospital appointment; and therefore, not from any feeling or interest of their own, but for the sake of the profession, they could not agree to their original deed. But I must give the words of Mr. Moss himself on this subject. He stated, at the meeting of the Trustees, March the 18th, 1826, (see Liverpool Commercial Chronicle,) "that he could no longer refrain from saying that he felt himself to have been very ill used by the medical men in this affair; he could not help feeling indignant at having been made use of by them as a sort of cat's-paw in the business. They had induced him to go to Mr. Ruston with the assurance of their assent to certain propositions, on condition of his allowing the discussion of past proceedings to drop, and abstaining from further comment upon them. Mr. Ruston acquiesced in the terms which he had been made the medium of proposing to him, and the arrangement was fully adopted by the medical men, at their meeting at Dr. ——. The agreement thus entered upon had been strictly observed by Mr. Ruston, &c. but he found that the medical men had been endeavouring to thwart those arrangements to which they had been parties; and he now saw them coming forward in that public meeting, to oppose those very propositions to which they had previously acceded. This was conduct from which, as regards the Trustees, he could not withhold the expression of his strongest disapprobation, and as regards himself, of his extreme disgust."

Liverpool, August 22, 1834.

MEDICAL GAZETTE.

Saturday, September 20, 1834.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

COLLEGE OF PHYSICIANS.

FELLOWS AND LICENTIATES.

We endeavoured in our last number to point out the nature of the connexion between the London College of Physicians and the English Universities; and we shewed that it was one naturally springing out of the circumstance, that some of the early members of the former were directly and intimately connected with the latter—as founders of colleges, or of lectureships, or as themselves holding professorships. Nevertheless, such connexion was unquestionably one of convenience and expediency, not of necessity, and seems to have been gradually drawn more close in proportion as competitors from other seats of learning rendered it more necessary for the graduates of Oxford and Cambridge to take those measures for their own protection, which their position with respect to the College of Physicians placed within their power. One of the most remarkable of those measures was the institution of an entirely new order, or rather of two new orders—those, namely, of Candidates and Inceptor-Candidates—for the express purpose of preserving the English Graduates and embryo Fellows pure and uncontaminated by even a temporary sojourn among the Licentiates. The impolicy of continuing this preposterous arrangement had latterly become apparent to many; and in the report of the Committee appointed last year to revise the by-laws, its discontinuance was recommended, though the measure was subsequently lost at a meeting of the whole College, and thus

the matter was suffered to remain *in statu quo*.

It cannot be concealed that the existing laws of the College of Physicians, as respects the admission of Fellows and Licentiates, have long been regarded by the latter as a serious evil, and, as we have seen, many even of the former have of late been induced to think that some change was required. It was shewn by the Parliamentary returns recently made, that from 1771 to the present time not fewer than 147 graduates of Oxford and Cambridge, and not more than 19 from other universities, had been admitted to the fellowship. Now it is not possible, we apprehend, for the most out-and-out exclusive to hold that no more than this paltry number could be found who were worthy of being received. The learned president acknowledged that "probably" there were more so situated, but endeavoured to account for their exclusion by a reference to the acrimony of the disputes between the Fellows and Licentiates, and by the violence with which the latter urged their claims. He further urged that no "injury" was done to the excluded. Now we shall not stop to argue this point; nor to shew, as might easily be done, that where there is selection and exclusion of various parties equally meritorious, there must necessarily be as a consequence both benefit and injury; but we shall pass on to the principle admitted by Sir Henry Halford, namely, that if the merits of the aspirants could be satisfactorily ascertained, equal justice would be done them with respect to their admission to the fellowship.

"You prefer the restriction of admission into the College to the graduates of Oxford and Cambridge, because you can better inform yourselves of their literary and scientific education, and of their moral habits?—*Sir Henry Halford:* Yes.

"Provided you could inform yourselves of persons, not graduates of the English Universities, being equally well qualified by good moral conduct, and by literary and scientific acquirements, ought you not, consistently with your own arguments, to give to them the same facilities of admission into the College which you give to the graduates of Oxford and Cambridge?—*Sir Henry Halford:* If I could have the same satisfactory proofs, I would admit them in the same manner."

Such, however, does not seem to have been the opinion of the Fellows generally; at all events no manifestation of such liberality was manifested until just before the opening of the parliamentary committee; for when, the preceding year, Dr. Wilson moved "that it is expedient to reconsider the by-laws relating to the qualifications of candidates, and of inceptor candidates," the resolution was negatived by 19 to 14. It met with the same fate a second time, the numbers being 23 and 15; but was carried almost unanimously on its third and last time of asking. A committee was then appointed, and reported in a paper *de Sociis*, which was subsequently printed. In this paper some very important changes were proposed, of which we gave a sketch at the time (see vol. xiii. p. 599); but though supported by Dr. Warren, Dr. Chambers, and other influential men, it was nevertheless ultimately thrown out—Dr. Seymour, among others, having strongly opposed it, more, however, from objections he had to some of the details, and from reprobating the notion of legislating under the intimidation of the then approaching Parliamentary Committee, than from denying altogether the necessity of reform; indeed, the latter gentleman, in his evidence, suggested several important and liberal changes. To us it appears extremely doubtful, for reasons formerly stated (see vol. xiii. page 555), whether the proposed concessions would have proved

satisfactory to the licentiates; while it is quite certain that no one would have given the College credit for an act of grace which came so late, and with the scrutiny which has since taken place impending over them. A considerable number of those more immediately interested would certainly have concurred in the sentiments expressed by Dr. Wilson, in that part of his evidence which follows the portion quoted last week.

"Are the changes which you think advisable in the present constitution of the College principally embodied in the paper *De Sociis*, that has been referred to; and if not, state what farther changes you think would be advisable?—*Dr. Wilson:* I think the general principle of this paper good, that of rendering all physicians practising under the license of the College eligible in a certain time. As I stated the other day, I should be glad, in my private opinion, to admit all physicians, with the restrictions I have mentioned today—[these restrictions applied to physicians practising as accoucheurs, and to members of the Council of the College of Surgeons]—all physicians practising under the College license; to render them eligible after one and the same certain and sufficient period of probation, which is not the case in this paper. I think the difference of time here is too great between the Oxford and Cambridge physicians, and the physicians of other universities.

"What do you mean by a period of probation?—*Dr. W.:* A period answering to that which is now passed by the candidates under the name of "candidates:" I would have them *in fact* candidates. I think the period of probation here for certain of the licentiate physicians is too long, and that I have stated in the College. The longest period here is seven years. I believe that in five years, or perhaps in a shorter time, we could judge sufficiently of the medical attainments and general character of a physician practising with our license under our own eyes; we should have sufficient time to form our opinion of his fitness for the fellowship.

"The principal object of the probation being to ascertain the respectability of the party?—*Dr. W.:* In my opinion. Again, I have an objection to the admission of the Doctors of Dublin University on more favourable terms than the physicians of other universities who have been admitted to examination for their license. Again, I object altogether (and those objections I have stated at length at the College) to the way in which the candidates and licentiate physicians, on completing the period of their probation, are to be selected and elected into the body of the Fellows. I would have, with the exception I have already mentioned, every licentiate physician (and I moved a resolution to that effect in the College), after a long and sufficient probation, say five years, or any time that might be considered sufficient, brought forward in rotation, proposed for admission to the fellowship, and balloted for as a matter of course. I would wish on many accounts to have the entire body of the physicians in this town really represented in the College, and to avoid all heartburnings and jealousies, and possible canvassing among friends, for the purposes of election. I could wish as much as possible to do away with all analogy between the College of Physicians and the clubs in the neighbourhood. [And again] Perhaps I may state another reason for my objection to this plan of selection and ballot, that I would wish to avoid by all possible means the chance of attaching stigma, by exclusion, to any competent physician who was not selected and elected. Under the plan here proposed, it seems to me that a licentiate might be passed over by the College, from mere circumstances of manner; from his not putting himself sufficiently forward; from not meeting with the Fellows of the College in the neighbouring clubs, or in general society. I would wish to avoid the possibility of a man of retiring habits (supposing him to be a physician of good attainments and character, and competent to execute the offices of the College) being injured by rejection even for one year, or, as it might be, for a series of years, with no fault on his part, &c."

But as the proposals in the paper alluded to, *de Sociis*, were rejected, and are not likely to be brought forwards now that the matter is in the hands of

Parliament, it is unnecessary to dwell upon them longer: we must, however, be allowed to allude shortly to an argument which we have frequently heard adduced in defence of the present system—namely, that the licentiates are eligible to the fellowship at the end of ten years;—*eligible* they may be, but as not one has ever been *elected*, the nugatory nature of the provision is sufficiently obvious. Indeed, we never heard of any one who was even likely to have been benefitted by this regulation (if benefit it may be called) except Dr. Pearson; who, poor gentleman, died at the age of 80, in the transition stage. But why, if the Fellowship is thought worth having, and every Licentiate, at the end of ten years, be *eligible* (we like the word) —eligible for the honour at the end of ten years—why has not even one instance occurred for the College to point to? The reason we apprehend to be simply because the Licentiates think, with most other men, that even gold may be bought too dear. Nor is this opinion with respect to the present question, to be found only beyond the walls of the College: hear Dr. Elliotson:—

"What do you conceive to be the reason why, under the seven years' by-law, no licentiate has, from the time of its passing to the present time, ever been admitted?—*Dr E.:* I should think it enough to frighten most men, to stand the chance of being examined by a whole College; not by a few individuals, but by an assembly. A licentiate would have to submit to examination from every fellow in the room, before the whole body, and he would stand the chance of being examined by men who were much his juniors—perhaps who had been his pupils. It is too terrific, I think.

"Does not a physician, after being in practice for many years, forget many of those subjects which a student, at the time of his quitting the University, is required to know?—No doubt of it.

"That he retains the substance, but casts away the scaffolding?—No doubt of it.

Another point to which we would refer, is the construction which has been put upon the words "British Universities." It may, indeed, be argued, that though the "scaffolding" just alluded to be better at other places, the "substance" is more compact and firm at Oxford and Cambridge, and consequently, that the foundation is more solid. Now we are very far from undervaluing the importance of preliminary education, and we think that no graduate in physic has any just claim to the full honours of the Doctorate, who has not previously taken his degree in arts. We do not, however, find that the College of Physicians, at any time, made this or any analogous specification the avowed grounds of selection, or the want of it the declared cause of rejection. But they took the charter, and arbitrarily rendered the words "British Universities," into *Oxford and Cambridge;* at a time, too, when Edinburgh, for medicine at least, held a higher rank than any other seminary in Europe. We do not thing that the sentiments of Dr. Sims, on this point, are at all too strongly expressed.

"Are you aware of the statute of the College of Physicians in or about 1751, in which, while pretending to give an explanation of the words "British University," they restricted it to the Universities of England?—*Dr. S.:* Yes, I am; it was cited in Dr. Stanger's case.

"At the period of passing that statute, for how long a time had Edinburgh existed as a medical school of celebrity? —*Dr. S.:* I think degrees were granted in Edinburgh about the year 1720-1725; it was about that time that students from all parts began to assemble there to obtain medical instruction.

"What at that time was the state of Edinburgh, as a medical school, in respect of the professors who were attached to that school?—*Dr. S.:* At that period, most of the medical professors were men of the first eminence in Europe.

"Was it not considered as, at that time, a school of so much celebrity as to compete with that of Leyden?—

Dr. S.: Yes, at that time it did compete with Leyden.

"Did many English students in medicine at that time repair to Edinburgh for the purpose of studying their profession?—*Dr. S.:* A great many.

"If, at that time, the College of Physicians, instead of endeavouring, by their by-laws, to restrict their fellowship to the graduates of Oxford and Cambridge, had erected a standard of qualification in literary, scientific, and medical attainments, would not that have been much more for the advantage of the medical profession, and have given an useful impulse to this study, as well in the Scotch as in the English Universities?—*Dr. S.:* I think it would have been a decided advantage to medical graduates generally.

"Is not the giving exclusive privileges to particular Universities as to the admissibility of their students to particular professions, liable to create supineness in those Universities, as to the study of those particular subjects in which they have exclusive privileges?—*Dr. S.:* I think it is.

"Would not the giving the privilege to well qualified students in the Scotch Universities have been likely, on ordinary principles of competition, to stimulate the English Universities as well?—*Dr. S.:* I think, in all probability, the English Universities, under those circumstances, would have made it their business to form schools of medicine as well as schools of arts.

But it was not necessary for them to erect schools of physic, because, forsooth, they were protected by the exclusive by-laws of the London College. For our parts we must say, that we think the whole system of *privilege* is a bad one—leading to idleness and arrogance; but we trust it has had its day, and that the time is coming when the place where a man has been educated will be received only as a presumption, not admitted as a test, of his qualifications. Certain it is, that the machinery has not worked well, either for the public or for the College itself; and that it must either be reformed, and that very thoroughly, or speedily fall to the ground.

MEETING OF THE BRITISH ASSOCIATION AT EDINBURGH.

At the first meeting of the Association at York, in 1831, it appears that the members did not exceed 700 in number; the year after, at Cambridge, they amounted to 1400; and it is supposed that above 1000 more have been added to the list at the Edinburgh "gathering." On Monday, the 8th instant, the first general meeting was held in the Assembly-rooms, George-street, when the Presidency devolved from Professor Sedgwick, who so ably filled that office for the last year, to Sir Thomas Brisbane. The different sections met on the following day in the class-rooms of the University: the first section being that of mathematics and physics; the second, of chemistry and mineralogy; the third, geology and geography; the fourth, natural history; the fifth, anatomy and medicine; and the sixth, statistics.

The medical section was placed under the able direction of Dr. Abercrombie, assisted by Sir Charles Bell, Professor Clark, Dr. Roget, and Dr. W. Thomson. A paper by Dr. Alison was read on Tuesday: the subject, the connexion between muscular irritability and the nervous influence. Other papers were read on the following days, but without exciting much discussion. On Thursday and Friday, Sir Charles Bell was engaged in delivering, in the form of a lecture, his views of the nervous system. The report of the proceedings of this section was read on Friday, by Dr. Abercrombie, who, after mentioning that several papers had been received, took occasion to express the very great pleasure which he and his brethren had expereriened from the present meeting in Edinburgh, and their anticipation of the happy results to which such periodical congresses of learned associates must lead. He concluded with some remarks on the connexion of physical and moral science, and the tendency of such pursuits to advance the high interests of man, as a moral being.

In the chemical section there were several discussions on the atomic theory, the analysis of the gaseous components of mineral waters, on chemical notation, and crystallography. Dr. Christison acted as secretary, and read the report on Friday.

In the section of natural history,

many most interesting subjects were discussed; and among the geologists, Dr. Traill gave an interesting account of some fossil fishes which he brought with him from Orkney. M. Agassis had discovered among them five new species. The latter gentleman read a paper on certain fossils found in the quarries near Burdiehouse, which he conceived at first to be reptiles, but afterwards discovered to be fishes partaking of the character of reptiles.

Some curious facts were mentioned in a statistical paper by Captain Maconochie, on the population and state of crime in France, showing that education is not always a sufficient guarantee against the commission of crime, but a mighty instrument for good or evil, according as it is directed. In one hundred crimes against the person, eighty-six were committed by men, and only fourteen by women; four-fifths of the latter were cases of infanticide. A considerable increase in the number of second convictions had taken place; but, according to the captain, "those persons sentenced to the gallows seldom rendered themselves liable to a second punishment,"—an observation which excited no small amusement among the hearers.

Besides the daily meetings of the sections, there were *conversazioni* every evening, in which "Scotland's daughters" took a part. These *soirées* were indeed most numerously attended; and at each, some lecture on a popularly-interesting topic of science was delivered. Thus Professor Robinson, on Tuesday evening, gave a brilliant account of comets; Dr. Lardner, on Wednesday, displayed the wonders of Babbage's calculating engine; and Dr. Buckland, on Thursday, delivered an interesting and amusing lecture on rare and extinct species of fossil reptiles.

Among the distinguished foreigners who attended the Edinburgh meeting were M. Arago, the astronomer royal of France; Professor Moll, of Utrecht; Professor Ulman, of Berlin; Baron Ende; Dr. Vlastos, &c. &c.

On Saturday the proceedings were wound up with sundry speeches on general matters, by Professors Sedgwick and Buckland, M. Arago, Lord Brougham, and others; and it was announced that the next annual meeting will be held in Dublin, in August 1835, under the presidency of Provost Lloyd.

CASE OF TIC DOULOUREUX,

Treated Locally with Success.

Some years ago, when I was trying the efficacy of the local influence of mercury in arresting chronic inflammation of the various structures in the body, with a view to illustrate the treatment of diseased joints on this principle, I met with several very obstinate and protracted cases of tic douloureux, which had resisted every remedy that could be devised, and I determined to subject them to that mode of treatment which I had found so successful in what I believed to be a similar condition of other textures.

The plan I adopted was to keep constantly applied to the part, on a piece of flannel, an ointment composed of one drachm of tartarized antimony and an ounce of mercurial ointment, renewing it as frequently as it could be borne, the object being to produce such a degree of irritation on the skin as would insure the mercurial influence on the part.

This method was attended with very considerable success, but I have not preserved any record of the cases in which it was employed at this time: it was in consequence of the benefit which a patient derived from this remedy, that I was consulted on the following case.

James Burrell, 62 years of age, residing at High Beech, in Essex, consulted me on the 9th of February, 1829, and the first attempt to describe his complaint produced a violent paroxysm, attended with so great contortion of the face as at once to indicate the nature of the malady.

As he was unable to articulate, I obtained from his daughter, who accompanied him, the following particulars of his case.

She informed me that, when able to speak, he described the pain as commencing just in front of the ear, and darting thence up towards the forehead, which he compared to a burning iron thrust into the part in this direction. The paroxysms were momentary, recurring suddenly every few minutes day and night, without any previous warning, and subsiding as instantaneously, without leaving any vestige behind them, the intervals being perfectly free from pain; they varied in severity at different times, and latterly there had been some tenderness on pressure just in front of the ear. The paroxysms were at first brought on by the act of deglutition; afterwards by any attempt at mastication or articulation; and this with such certainty and severity, that for the previous week he had never uttered a single syllable, but had been compelled to

write every thing on a slate; and independently of their being thus produced, they constantly occurred spontaneously. I was informed that the paroxysms were accustomed to recur with the same frequency and severity for about two months; they would then gradually lessen in frequency as well as in severity and duration, until they altogether disappeared, when he would be free from them for about two or three months, and they would then again recur with the same violence as before, without any assignable cause.

He had been afflicted in the same manner, with similar intervals, for thirteen years, and believed that the disease was originally brought on by mental anxiety: he was subject to rheumatism occasionally, and was very moderate in his diet, although rather of a full habit.

Mr. Brown, of Waltham Abbey, informed me, that during the progress of the complaint, he had prescribed the carbonate of iron, quinine, and colchicum; that bleeding, purgatives, and the general antiphlogistic treatment, had been carried to the fullest extent, aided by cupping, and the application of leeches to the part affected; that blisters had been employed, and the tartar emetic ointment freely rubbed on to the part, to the extent of producing as much irritation as could be borne; but none of these modes of treatment seemed to produce any effect on the disease, which pursued its own course, uninfluenced by remedies.

I directed some opening pills to be taken every second night, and the mercurial ointment, with tartarized antimony, to be applied to the face on a piece of flannel. This application produced great irritation on the skin; but after its employment for a fortnight, the paroxysms were decidedly relieved, and in a month he was perfectly well. He lived till July 1833, without having any return of the disease, and then died of some other complaint *.

MADRID MEDICAL GAZETTE.

The *Gaceta Medica de Madrid, periodico semanal*, No. 1, made its appearance on Saturday, June 7th, 1834. It has gone on regularly, and we trust prosperously, since. We noticed the commencement of the journal not long ago, when we made some remarks on the present state of medical matters in Spain; and we add the present note merely to mark the date of the first number.

* Cases of Tic Douloureux, and other Forms of Neuralgia. By John Scott, Surgeon to the London Hospital, &c. *(Just published.)*

FRENCH COOKERY.

The law of France forbids the exposing for sale the gelatinous flesh of young calves not at least six weeks old. M. Huzard, of Paris, argues strongly against the regulation. He allows that this gluey sort of young veal is good for nothing if it be boiled; but says he, "roast it, or make a fricasee of it; put plenty of pepper and salt to it; and with the addition of a little wine, a *petit-verre*, and a cigar, it is as good food as any in the world!" — *Annales d'Hygiène.*

WEEKLY ACCOUNT of BURIALS,

From Bills of Mortality, *Sept.* 16, 1834.

Abscess	1	Heart, diseased	2
Age and Debility	44	Hooping-Cough	8
Apoplexy	10	Inflammation	30
Asthma	7	Bowels & Stomach	19
Cancer	2	Brain	3
Childbirth	2	Lungs and Pleura	8
Cholera	46	Insanity	2
Consumption	80	Jaundice	4
Convulsions	42	Liver, diseased	2
Croup	4	Measles	5
Dentition or Teething	8	Mortification	2
Diarrhœa	1	Paralysis	4
Dropsy	12	Scrofula	1
Dropsy on the Brain	11	Small-Pox	5
Dropsy on the Chest	3	Stone and Gravel	1
Fever	12	Thrush	3
Fever, Scarlet	14	Unknown Causes	2
Fever, Typhus	2		
Gout	2	Stillborn	18
Hæmorrhage	1		

Decrease of Burials, as compared with the preceding week . . . 59

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

Sept. 1834.	Thermometer.		Barometer.	
Thursday . 11	from 51	to 59	29·71	to 29·82
Friday . . 12	50	65	29·99	30·13
Saturday . 13	41	66	30·17	30·24
Sunday . . 14	40	67	30·28	30·29
Monday . . 15	39	67	30·25	30·21
Tuesday . . 16	40	68	30·14	30·04
Wednesday 17	39	71	29·91	29·89

Wind variable, S.W. and N.E. prevailing.

Frequent rain during the 11th; the 12th generally cloudy, with frequent intervals of sunshine; since the 12th, except the morning of the 16th and 17th, generally clear.

Rain fallen, ·2 of an inch.

Charles Henry Adams.

NOTICE.

We fear the cases of Cholera sent last season from Walsall have been destroyed. From the length of time which had elapsed, we did not suppose that the writer wished them to be returned.

W. Wilson, Printer, 57, Skinner-Street, London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, SEPTEMBER 27, 1834.

LECTURES

ON THE

THEORY AND PRACTICE OF MIDWIFERY;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture LI.

COMPLEX LABOURS.

PLURAL BIRTHS.

Average frequency. — Woman, although usually uniparient, like other uniparient animals, sometimes produces more than one offspring at a birth; and when the gestation is plural, twins are by far the most frequent. The average of twin cases varies considerably in different parts of the world; and we find also no little difference in the tables kept by separate individuals in the same country. Thus Denman shews that in the Middlesex Hospital in this metropolis one occurred in about every 93 labours; in the London practice of midwifery the estimate is stated as one in 48; Conquest considers it one in 90; Gooch, one in about 70; Blundell states, that from the statistical accounts transmitted to government in the year 1801, it appeared that in these islands one in 65 was a twin case. Bland, in London, and Boer, at Vienna, met with one in 80; in the Maternity at Paris, one was met with in 88; in the Maison d'Accouchemens, one in 91; Mad. Boivin met with one only in every 132; Dewees averages the frequency in North America as one in 75; Dr. Arnell's average is also one in 75; Dr. Moore's, one in 76. From the table I hold in my hand, of 67,439 women delivered in the Dublin Lying-in Hospital, there were 1115 cases of twins, being about one in every 60 labours; 20 of triplets, and one of quadruplets. From tables which I have myself kept, I find, out of 13,900 cases, twins occurred about once in every 80. Triplets are generally supposed to be met with once in about 3 or 4000 labours; and the returns from Dublin would lead us to believe that estimate tolerably correct; but I am inclined to think the frequency of these cases generally much overrated. Quadruplets are so rare as to defy any thing like an accurate calculation.

It is popularly supposed that climate, and the state of civilization to which the country has advanced, exert an influence on the multiplication of the human species; and that certain external circumstances are favourable or otherwise to the frequent production of twins; but this is by no means proved; although we know that some animals, the sow, for instance, farrow more young at a birth, and also more frequently, when domesticated, than when in a state of nature. Dewees says, that if the various tables can be relied on, it is certain "there are conditions and circumstances which give rise to more double births" in America than Europe.

It is also a frequent belief that preternatural fecundity is to a certain extent hereditary, and Dewees states that "some facts within his own knowledge would seem to countenance this supposition; but they are not sufficiently numerous or strong to confirm it." He looks upon it, however, as in some instances constitutional, and adduces the case of a woman whom he knew, that five times produced twins, and never had a single child; and another who thrice brought forth twins, though not consecutively.

It has been also observed, that some seasons appear more prolific than others, as well in the human race as other productions of nature; but whether this is quite accidental, or dependent on some fixed laws, is not easily determined. Denman, indeed, says, "it can scarcely be

doubted that there is some relation in those years between the animal and vegetable creation."

The prolific powers of the human female, however, are not even limited to the production of four children at a birth. I have already mentioned five fœtuses, preserved in one bottle in the Museum of the College of Surgeons, born at one birth, under the care of the late Dr. Hull, of Manchester; they had advanced to five months intra-uterine age.

Since I last spoke to you on this subject, I have paid some attention to the histories on record of extraordinary fecundity in the human race; and, putting out of the question the extravagant stories related by Ambrose Parè, Shenckius, Petrus Borelli, and others, I find there are four other well-attested cases of five children at a birth on record: one of these occurred in London, another at Wells, in Somersetshire, (both of which are noted in the Gentleman's Magazine;) the third in Upper Saxony; and the last near Prague, in Bohemia. These cases are cited by Dr. Garthshore, in a curious paper on plurality of children, read before the Royal Society at the time when he presented them with Dr. Hull's interesting instance. The London Practice of Midwifery, and some other works on the science, state that Dr. Osborn met with six distinct ova thrown off at one abortion, but on what authority I have not been able to discover. Parè indeed tells us, that in his day the wife of the lord of Maldemeure, in the parish of Sceaux, near Chambellay, produced six children at a birth, after which she died; and that the then present lord of Maldemeure was the only surviving one. His history of this extraordinary occurrence is so circumstantial as to impress us with the belief that he was himself fully convinced of the fact. It would be going too far, perhaps, to say that such an event was impossible; but we must take into account that Parè, though an honest man, and an excellent surgeon for his time, was a very credulous philosopher.

But the most miraculous instance of supposed fecundity in a human female is that of the Countess Henneberg, recorded on a marble tablet in the church of Lonsdunen, near Leyden. The monument bears the following announcement:—

> "En tibi monstrosum nimis et memorabile factum,
> Quale nec a mundi conditione datum."

After which lines follows a prose account of the miracle; to wit, "That Margaret, the wife of Hennam, Earl of Heneberg, and daughter of Florence, the fourth Earl of Holland and Zealand," (then we are favoured with her pedigree for many generations,) "being about 40 years old, upon Easter-day 1276, at 9 A.M. was brought to bed of 365 children, all of which were baptized in two brazen basins by Guido, the suffragan of Utrecht. The males, how many soever there were of them, were christened John, all the daughters Elizabeth; who altogether, with their mother, died on the same day, and with their mother lie buried in this church of Lonsdunen." This supernatural infliction is accounted for on the principle of retributive justice; for we are informed that the countess, being solicited for alms by a poor woman who was carrying twins, shook her off in contempt, declaring that she could not have them by one father; "whereupon the poor woman prayed to God to send her as many children as there were days in the whole year; which came to pass as is briefly recorded in this table for perpetual recollection, testified as well by ancient manuscripts as by many printed chronicles."

Now, you will be inclined to laugh at this story, and the credulity of the people who raised the memorial; and yet no doubt the broad fact is true, that the poor lady died after having *given birth* to a number of watery cysts, which were looked upon as ova. A very slight stretch of the imagination might transform them into children in embryo, and in the rigid adherence to the Catholic forms of worship, it might have been thought necessary that the baptismal ceremony should be performed: in fact, they were nothing more than a mass of hydatids, (the same indeed as all other cases of abortions, in which it was presumed many ova were expelled), as is clear from the declaration, that "they were baptized in two brazen basins." The noble lady probably sunk from flooding.

Twins may possibly proceed both from one ovarium, or the rudiments of one fœtus may be furnished by each gland. When the conception, however, is more than duplex, it is clear that one ovary must supply two; for no instance has yet been met with where these organs have been in excess. It is commonly supposed that twins are the result of one connexion; and instances have come within my own knowledge where this must have been the case; but it is not equally plain that this is an universal rule. I shall hereafter offer to your notice a few remarks on superfœtation, and endeavour to prove that those cases which were, till comparatively late times, looked upon as instances of a second conception, occurring some months after a first impregnation,—while the uterus still retained the fruit of the former intercourse,—were falsely esteemed instances of superfœtation. But it is by no means impossible that a second impregnation may

take place soon after a former one has occurred. It is not difficult, indeed, to imagine that such an event may happen at any time previously to the uterus becoming lined with the secretion afterwards converted into the deciduous membrane; or until its mouth is plugged with that viscid mucus which divides its cavity from that of the vagina, and which would entirely prevent the immission of the seminal fluid *in coitu*. Cases, indeed, are recorded that bear upon the point. The celebrated one related by Buffon, for example. A white woman at Charlestown, in South Carolina, was delivered, in 1714, of two children, one black, and the other white;—this difference in colour led to an inquiry, and she confessed that, on a particular day, immediately after her husband had left his bed, a negro entered her room, and, threatening to murder her if she did not consent, forced her to submit to his will. Dr. Moseley has recorded another instance somewhat similar, in his Tropical Diseases; it occurred within his own knowledge, on Shortwood Estate, Jamaica. A negro woman brought forth at a birth two children of the same size, one of which was a negro, and the other a mulatto. On being questioned, she admitted that a white man belonging to the estate came into her hut one morning before she was up, and she suffered his embraces, almost immediately after her black husband had left her. Dr. Dewees has related that a servant in Montgomery county was delivered of a black and white child at one birth, which were often seen by the Doctor. He states also, that on the report of her pregnancy, both a black and white man disappeared from the neighbourhood; and Elliotson has put on record, that Mr. Blackaller, of Weybridge, sent him the following account:—A white woman of very loose character left her husband, and some time afterwards returned pregnant to the parish, and was delivered in the workhouse of twins; "one of which," says Mr. Blackaller, "was born of a darker colour than I have usually observed the infants of the negroes in the West Indies to be; the hair quite black, with the woolly appearance usual to them, with flat nose, and thick lips; the other had all the appearances common to white children." That these respective twins were not the offspring of one parent is very evident; and a second impregnation, therefore, must have taken place; but we have proof in two, at least, that the connexions followed each other quickly, before any changes could have been commenced in the uterus. With the knowledge, then, of such accidental occurrences in our possession, we are warranted in believing that, in the case of a woman living with her husband, twins might possibly be the result of two separate connexions, if only a short period intervened between them.

Each individual child which the uterus contains, according to the law of nature, is distinctly enveloped in its own membranes, so that its body is not in contact with that of its brother, possesses its own quantity of liquor amnii, has a separate funis and separate placenta,—the circulations not inosculating. Generally the placentæ are attached at a part of their edges; and often, on regarding the maternal face, they appear but one mass; at other times they are situated distantly from each other, at different points of the uterus; again, occasionally, though very rarely, the vessels of the one child anastomose with those of the other. It has been remarked, that both children have lain in one bag of membranes; and cases are recorded, such as the one in the Memoirs of the Royal Academy, where the placenta was in all respects single, and the funis arose singly, and divided into two branches when about to terminate in the umbilicus of each fœtus.

Symptoms of twin gestation.—There are no symptoms during pregnancy which positively indicate to us that the womb contains more than one fœtus. Some have been noted and dwelt upon as diagnostic marks, but they are all more or less fallacious. Such are, the uterus being of a larger size than usual; but this may depend on an increased quantity of liquor amnii;—the woman feeling two distinct movements at different parts of the uterus; but the sensations of a pregnant patient on this point, as expressed by her, are scarcely ever to be relied upon;—an irregularity in the shape of the womb; its being broader than common, or measuring more laterally than in the longitudinal direction; but this again may be the consequence of a transverse position of the fœtus, or an irregularity in the development of the uterine fibres themselves. If, indeed, it should happen that the organ was divided into nearly equal portions, by a sulcus running longitudinally downwards from the fundus to the cervix, we might suspect a twin gestation with some confidence. Dr. Kennedy, of Dublin, whose name I have before mentioned, and other gentlemen who have given their attention to auscultation, as a means of distinguishing pregnancy, inform us, that they can always detect twins in utero by the double pulsation of the fœtal hearts. I have not myself acquired such perfection in the use of the stethoscope as to warrant my giving an opinion founded on the indications which that instrument supplies. And, fortunately, such knowledge is not required in practice; for, al-

though we might be assured of the gestation being plural before labour commenced, our treatment would not be in the least influenced by our discovery.

Position in utero.—The two children may each be placed in utero in all the varieties of position which one may occupy. It is generally believed that the most frequent presentation is the head of one and breech of the other; but I have before remarked, that from my own tables, I should conclude it was more usual for both the heads to offer themselves downwards. Campbell also states, in his late valuable work, that from a register of his cases, "he finds both the fœtuses have almost always presented the vertex."

Progress of labour and treatment.—Twin labour generally proceeds exactly in the same manner as though there was but one child. The pains increase in frequency and strength, the membranes protrude through the os uteri, and in process of time burst; but the uterine contractions are often more feeble than when the womb contains but one; and they do not seem so effective, since they can only act upon the body which is presenting through the medium, as it were, of the ovum lying above. No interference, however, is necessary, solely on that account; and, provided nothing untoward happens, the labour must be allowed to proceed uninterruptedly, until the first child is expelled; when, for the first time,—although we might have had our suspicions before,—we become positively certain of the existence of a second.

I have already advised you, in all cases, to place the hand on the abdomen as soon as the funis is divided, to ascertain the state of the uterus and placenta, and to learn whether there be a second child; if so, the womb will be found still large, its fundus rising to the umbilicus, or above it, and occupying a space apparently almost as great as before the birth of the first. We may detect, also, that degree of elasticity and subdued fluctuation,—if the membranes be still whole,—which I have before mentioned are so characteristic of the uterus at full time. This simple examination will generally be sufficient to inform us of the fact; should any doubt, however, remain, the finger must be passed up to the os uteri, and the membranes of the second fœtus will be felt presenting, as during the first stage of natural labour.

It is possible we may be deceived both in the external and internal examination. The uterus may contain, besides the placenta, a large quantity of coagula, which may so distend its cavity, that the organ may occupy the principal part of the abdomen; but it will be softer than when another child remains, and on pressure being applied, blood will most likely escape through the vagina. Again, a collection of blood behind the fœtal membranes may be mistaken for the unbroken cyst of a second child. The case will be rendered clear on lacerating them; for coagula and fluid blood will escape instead of liquor amnii.

As soon as we have satisfied ourselves that the case is plural, it is our duty to determine the presentation as speedily as possible; and if it be transverse, to turn, as in ordinary cases, according to the rules already sufficiently detailed. But if the head or the breech be presenting, the membranes may be ruptured immediately, that an opportunity may be given for the depending part to pass at once into the pelvis. There cannot be the same necessity for preserving the bag of membranes of a second child entire that I have so often insisted on in single births, because the passages have been sufficiently prepared by the exit of the first to allow the easy transit of the second, if the children are nearly of the same size; and this proceeding frequently excites the uterus to increased energy, and facilitates the termination of the case.

It will occasionally happen, indeed, that the two children are expelled so rapidly, one after the other, as scarcely to give time for an internal examination to be instituted between their births; and it has happened to me more than once to find a second child in the world before the one already born could be separated.

If the uterine contractions be tolerably powerful, the birth of a second twin is very seldom protracted, unless it be misplaced in utero, monstrous in formation, or much larger in size than the one first expelled. It is not unlikely, however, that twins may exist with a deformed pelvis, and both may require to be extracted either by the forceps or craniotomy instruments; and Denman justly remarks, "if we were compelled to make the first labour artificial, it might be necessary or expedient to deliver the patient of her second on the same principle, unless the natural efforts should be efficaciously made soon after the birth of her first child." Women, then, seldom suffer much during the birth of a second twin. As the principal pains, under ordinary labour, are those of dilatation, and the sufferings generally in proportion to the resistance experienced, we should naturally expect that the second child would be born without either any great effort or much additional painful sensation. But when a woman, after having given birth to one child, learns that there is another still in utero, she mostly becomes not only apprehensive of her safety, but also fearful that

she has to undergo a repetition of the agonies she has just endured; and the impression on her mind may possibly interfere with the continuance of uterine action. For this reason it is better neither to inform her abruptly of the nature of the case, nor to make any mystery about it, but calmly to tell her that she will soon be blest with a second; and this may be coupled with a congratulation on the fortunate progress of the labour so far; and an assurance that she will have but little more pain to bear, and that the case presents no features calling for anxiety.

In the conduct of a common twin case, it is of the greatest consequence that we should not attempt to extract the placenta of the first until after the birth of the second, and that we should not make any traction at the cut funis which is hanging out of the vagina; for if we separate the placenta from its uterine attachment, flooding will certainly supervene, and the loss of blood may be so great as to require the immediate evacuation of the uterus; the only likely means by which it can be restrained.

Upon the expulsion of the second child, the uterus must be again examined, both externally and per vaginam, to ascertain that there is not a third, the birth of which (should there be another), is to be conducted exactly on the same principles; so also with regard to a fourth and fifth likewise; for we may any of us possibly meet with one of these prodigious instances of fecundity. It is seldom, however, when there are more than two children in utero, that gestation proceeds to its full term.

In every case of plural gestation, there is considerably greater danger from hæmorrhage than when the birth is single; partly from the increased size which the uterus has acquired, and its indisposition to contract thoroughly; partly from the larger number of vascular orifices exposed on the separation of the placenta; and partly from the greater chance of adhesion having taken place at some part of the more extended surface in apposition to the uterus. Our principal attention should therefore be directed to preventing or subduing flooding. With this view, the uterus should be stimulated to throw off the placentæ, by the grasping pressure of the hand, and the utmost care must be taken that both these masses pass from the cavity at, or nearly at, the same time. If one be brought away while the other remains behind, hæmorrhage must occur, for reasons before frequently noticed. Compression, then, must be used more diligently than in common labours; and on examining internally, to ascertain whether the placentæ be separated and lying loose in the vagina, one of the funes must first be twisted round two or three fingers of the left hand, and brought to its bearing, while the index of the right is carried up to the brim of the pelvis; and afterwards the other must be treated in the same way. No attempt must, on any account, be made to remove them through the agency of the cords, until the beds of both can be most distinctly felt, and their masses principally surrounded by the fingers, introduced as in a common examination; but when they have descended sufficiently low to be entirely encompassed,—each funis having been put slightly on the stretch,—traction may be made by both together, and the organs removed from the vagina, with the cautions before sufficiently, I trust, insisted on. Should flooding supervene after the birth of both children, or the time previously specified elapse, the hand must be introduced, and the placentæ removed; should adhesion exist, the separation must be conducted on the common principles—care being taken not to remove either until both are fully in our grasp. The uterus must be stimulated to continued contraction by pressure, the application of cold and astringents if necessary, and the case must be treated as one of ordinary hæmorrhage. On the withdrawal of the placentæ, it is always desirable that the maternal face should be inspected, to assure ourselves no part remains within the uterus.

I need scarcely warn you that if flooding, convulsions, or other dangerous symptoms, shew themselves between the birth of the two children, the ordinary methods must be used to combat them, and the speedy delivery of the second must be undertaken. But there is a point admitting of some dispute, and deserving of very grave consideration—namely, the length of time that it would be desirable to wait after the birth of the first child, before means are taken to extract the second; no dangerous symptoms in the meantime appearing. Some practitioners decry artificial assistance, merely in consequence of lapse of time, and found their arguments on the very excellent obstetrical maxim that nature should never be interfered with, or thwarted in her intentions, so long as she can be safely trusted. The consequence of this doctrine is, that often many hours, sometimes many days, have been allowed to pass, after the birth of one child, before the labour was terminated. This is a practice that I cannot sanction, because of the danger (particularly from flooding) to which the woman must be more or less subjected during the interval; and because of the anxiety, excitement, and alarm, that must necessarily harass her mind until she is relieved: nor is it by any means improbable that such depressing feelings may materially interfere with her

ultimate recovery. I therefore perfectly concur with Denman, in thinking that if uterine action is not re-established, some limit should be placed to our passive treatment, and that the time which "it may be expedient to wait shall neither be so short as to run the risk of injuring the patient by hurry or rashness, nor so long as to increase the danger, should any exist, nor the difficulty of delivering the patient, if we should be at length obliged to use art for this purpose." And I think the period specified by the same estimable physician—four hours, perhaps—the least objectionable. I have already advised that the membranes should be ruptured soon after the birth of the first child, and the possibility of being compelled to deliver artificially does not militate against this practice; for if the uterus acts vigorously, the fœtus will most likely pass naturally; and if the pains are feeble, or altogether deficient, there can exist little or no impediment to the introduction of the hand and the performance of turning: and this is the operation, indeed, which we shall find most usually called for when it becomes necessary to terminate the labour by art. Some practitioners advise the exhibition of the ergot in preference to having recourse to artificial delivery, in cases where inertia of the uterus retards the expulsion of the second child. I should not object to making a trial of this drug, but would not rely implicitly on its efficacy, to the exclusion of more certain means. These recommendations, however, must only be understood to apply to twin labours, at the full period of gestation. If one fœtus be thrown off prematurely, and another be retained in utero, it would be unwise either to rupture the membranes or extract manually—unless, indeed, the immediate preservation of the mother required the emptying of the cavity; because it is not improbable that gestation might be carried on for the perfection of the one remaining, and it would be our duty to save it, even at the sacrifice of inflicting much personal inconvenience on the mother, or at some small risk to her.

Cases have been known, indeed, where one fœtus and placenta have both been expelled prematurely, and the other retained and carried until the completion of the period of gestation; and this without the patient suffering any dangerous loss of blood.

It is seldom that the membranes of both ova break before the first child is expelled, but such cases are occasionally met with, and instances are recorded in which parts of two separate children descended into the pelvis together. Thus Dr. Ferguson, of Dublin, not long since, related a case in which the head of one child and the feet of another presented in the pelvis at the same time. The midwife in attendance, before he arrived, had pulled down the feet, and jammed the breech and head together. The pains being very powerful, the labour was terminated naturally; the child whose head presented being expelled first, the other afterwards. In the 12th vol. of the Medico-Chirurgical Transactions, is a case, related by Mr. Allan, of Leicester-Square, in which the heads of two children occupied the pelvis together, (the body of one being in the world,) and were both expelled simultaneously by uterine contraction; and he refers to two others of the same kind—one in the Edinburgh Medical and Surgical Journal, for January 1822, and the other in the Journal de Médecine for November 1771. I was on one occasion sent for to the assistance of a midwife who had been pulling at two feet, which I found external to the vulva. Although they were a right and left, I immediately detected, by the direction of the toes, that they belonged to different bodies; by gently pushing up one, and careful traction at the other leg, I extricated each breech from the brim of the pelvis, and both children were born living.

With twin cases, gentlemen, my observations on labour are brought to a close; but there remain still to be considered the maladies attendant on the puerperal and the pregnant states, the organic affections of the uterine system, and the diseases of infancy; which I have generally considered the second part of our course. The next subject, therefore, will be Puerperal Diseases.

EXTRACTS

FROM A

JOURNAL OF NATURAL HISTORY;

Kept during a Voyage from England to New South Wales, and on the return to England, by way of Batavia, Pedir Coast, Singapore, &c.

By George Bennett, Esq.

On the 2nd of June we sailed from Batavia for the Pedir coast, on the north-eastern part of the island of Sumatra.

June 11th; latitude 6 deg. 39 min. south; longitude 99 deg. 9 min. east; thermometer 82½ deg. to 83½ deg.—In the evening I captured with the towing-net several species of salpa, and also

numerous specimens of a transparent mollusca animal of a triangular form, the extremities of the angles being spinous.

On the 1st of July, in Pedir Roads, during a calm, there were large quantities of a small species of salpa (S. confederata?) united together in chains of several yards in length, and a large number were taken out of the water with the dipping-net; separated, they measured in length 2-8ths of an inch.

I captured also a beautiful specimen of porpita, of a light purple colour: the tentaculæ radiated off from its disc in an elegant manner, and were also of a purplish colour; but so delicate were those appendages, that it was almost impossible to take the animal from the net, without the whole, or nearly the whole, of the tentaculæ being left behind. Soon after the animal had been removed from the water, the fine purplish tint faded, changing gradually to white; and, if placed in spirits immediately on being removed from the water, it changed to an orange red colour, the centre of the disc becoming of a pearly white.

The leptocephalus, which had been caught some time since, I remarked this day, on looking at it in the bottle of spirits, that it was changed in appearance, having now an inflated form; it looked like a piece of dried inflated skin. From this a question may probably arise, whether the animal has not a power of inflating itself?

We sailed from the Pedir coast on the 23d of July, and on the 25th, about 10 A.M. it was calm, when a rushing noise, and an appearance of overfalls on the water, was seen in the N.E. direction, which soon came up to us, and rocked the ship, as well as the water about, the ship appearing as if agitated by a strong breeze, although not a breath of wind filled the sails; it passed away after a short duration; but occasionally, during the morning, similar overfalls from the current or other causes were experienced.

As we sailed along the Sumatran coast, after leaving Pedir to the eastward, the same appearance of landscape presented itself as we had passed on approaching Pedir from the westward, consisting of a low coast and lofty verdant hills and mountains; one was conspicuous from its peculiar form, from which it had been designated by the name of the "Friar's Hood."

We had very fine weather to the 30th, during our passage down the Straits of Malacca; but the passage was rendered tedious by light and variable winds and calms. Thermometer 83 deg. to 86½.

July 30th., Tuesday.—Fine weather; wind light and variable from the eastward. This morning a small fish, which was seen swimming about the rudder, was captured by the towing-net; it measured four inches in length; of a dark green colour, spotted with white over the body; tail white, with several longitudinal brown lines—the white dots were so disposed as to give the appearance of broken white longitudinal lines upon the sides of the fish; fins black, interspersed with white, except the pectoral fins, which were transparent; eyes of a golden colour.

This afternoon two species of *Medusa* were captured by the dipping-net alongside the ship; the first was about four inches in diameter, and a very handsome species: the upper portion of its body was purple, intermingled with brown, and covered with irregular small pearl-white spots; the tentaculæ were brown, and grasped several of the "finny tribe," which appeared to serve it for daily food. This beautiful specimen, after being in spirits for two days, lost all its former beauty, nothing but a mass of jelly, without form, remaining. The second species was perfectly transparent, of a rounded form, and with projections round the upper part of the edges (which are readily to be distinguished from the other parts of the animal, when in the water, by their pearly whiteness), like the impressions of the ciliary processes upon the crystalline lens of the eye, and the consistence of it when out of the water. Several *Porpita*, with long extended rays, were also taken. A small species of *Cancer* was also found with the first *Medusa*, and a very young *Sepia* was also captured.

Logs of wood, covered with barnacles, &c. and attended by several kinds of fish (and upon them frequently a bird was seen watching its prey, or resting itself), passed the ship last evening and this morning.

August 2nd.—Shoals of fish, skip, jacks, yellow-tails, and others, have been splashing about the water in large shoals frequently during the day.

At 10 A.M. of the 4th of August land was in sight, having soundings at 35 fathoms; the land is the Hanging Hill, and adjacent coast. We beat in towards the high range of land of Pulo Dinding and islands or islets about it; and about 6 P.M. tacked in 25 fathoms, with a bottom of green mud and fine sand.

August 5th.—Early this morning the Sambilangs Islands, and Pulo Jarra, were in sight; the latter a small, high, wooded islet. In the evening we passed in the passage between Sambilang Islands and Pulo Jarra.

The lightning was very incessant and vivid in the straits; at one time there was so little intermission of the flashes, that I thought some one was carrying a lighted lamp about the deck; it frequently also exploded in very heavy peals of thunder; the forms, likewise, the electric fluid assumed were very peculiar and varying, often resembling the discharge of rockets across the heavens above. Thermometer from 76 deg. to 84 deg.

August 18th.—Passed Tree Island, which is a long sandy flat, with low bushes upon one side of it, and further inland one single tree, more elevated than the shrubs around, and a short distance from it a group of trees, of about the same elevation. This sandbank, of course, from the continued growth and increase of shrubs, trees, &c., necessarily must change in appearance in the course of time.

August 19th.—We anchored in Singapore Roads about 4 P.M. We only remained at Singapore until the morning of the 22nd of August, when we sailed for China. We had fine weather, with moderate breezes from S.W. to S.S.W. to the 29th August. At noon of that day we were about 35 or 40 miles distant from Pulo Sapata; and in the afternoon I observed a solitary swallow twittering and flying about the bows of the ship, and at sunset flew about the ship into the cuddy and cabins, seeking a roosting place for the night; at last it was not visible, having probably taken refuge for the night in one of the boats, or some other convenient roosting place. It was nearly about the same time of the year that I met before with this interesting migratory bird, when on a passage beating down the China Sea against the S.W. Monsoon, in October 1830. We did not, however, see our little visitor again the following day; so that it probably gained the land on the subsequent morning.

The weather continued beautifully fine, with moderate and light breezes, varying from S.W. to S. to the 1st of September, when we had soundings upon the Macclesfield Bank, about 4, A.M. in 17 fathoms with the bottom, judging by what was attached to the armed part of the lead, of small pieces of red coral. At 8, A.M. soundings were again obtained in 40 fathoms, with a bottom of broken fragments of red and white coral, madrepores, &c. &c.

According to Horsburgh, this bank was discovered by an English ship, whose name was given to this extensive shoal, in 1701. At noon, no soundings were obtained with 90 fathoms of line. We have passed over the western edge of the bank.

For the last two days a current has been experienced, setting to the E.N.E. 32 miles in the 24 hours each day.

September 2nd.—Lat. 17 deg. 20 min. N.; long. 114 deg. 4 min. E.—A solitary swallow was flying about the ship this morning. This afternoon there was a large number of the *Gannet* tribe about, and at some distance from the ship, and were all very busily engaged in fishing. Our proximity to the Paracels group of islands may sufficiently account for their appearance.

On the 13th of November we sailed from China, and arrived at Singapore on the 26th of the same month.

On the 5th of January 1831, on a former voyage, a blue shark (*Squalus glaucus*, Linn.) was caught, which measured ten feet in length; the teeth were longer, but not so broad, as in the white species, but the edges were serrated. During a dissection of the head, I met with two tumors, one about the size of a hen's egg, the other smaller; they were imbedded far back in the cavity of each nostril, and so strong was their attachment to the adjacent parts, that it was with difficulty I succeeded in removing them. On laying open the smallest of these tumors, there appeared a quantity of what at first resembled bits of chopped hay, but when minutely examined, were discovered to be animalculæ (*Epizoæ*), which, on being placed into a glass of water, displayed symptoms of vitality. On laying open the other tumor, it was also full of

Epizoœ, but differing in appearance from the preceding, being smaller, thinner, of a white colour, resembling *vermicelli* more than any thing else to which it could be compared.

The tumors were of soft consistence, and covered by an investing membrane, of rather thin texture. When placed into a glass of sea-water, these animals could be plainly distinguished moving about.

The first kind were about an inch in length, having, when in the water, a pellucid appearance, with a white streak passing down the centre longitudinally; the caudal extremity terminated in a small point; at the head, on each side, were several short tentaculæ, which ap peared almost in constant movement, and the white streak (probably the intestinal canal) commenced from the lowest of these tentaculæ. This canal, on being pressed, emitted a whitish fluid.

In the second tumor they were also about an inch in length, but differed in their appearance, as before described.

The smell of these tumors was also peculiar, differing from other portions of the fish, being of a faint, fœtid odour (*Odor sui generis.*)

The animalculæ, on an examination, were found to be *Epizoœ*, and probably new species.

The tumors, &c. were presented and deposited in the Museum of the Royal College of Surgeons in London.

A much-desired animal is that which inhabits the shell termed *Carinaria*; they had been seen by a surgeon in the navy, who describes the animal as being perfectly transparent, much elongated from the shell, and, from the transparency, difficult to be distinguished when in the water; the carinated shell was situated below the animal when in the water.

This was seen on the 14th of October 1832. Fine weather; nearly calm; and in lat. 34 deg. 26 min. S.; long. 2 deg. 58 min. E. Three spemimens were seen.

The animal seemed to possess the power of locomotion, by the aid of a kind of fin or fins, and the extremity or tail (if it may be so termed), had some resemblance to that of a fish; no portion of the animal was seen above the surface of the water.

The first specimen was seen (being nearly "dead calm" at the time) as the boat passed it; and although the boat was brought round as speedily as possible, the animal had disappeared—supposed to have sunk; it reappeared, however, soon after, at a distance further off, but by the time the boat could come up with it, the animal again disappeared.

The two other specimens were seen in the water close to the ship; but the reason assigned for no attempt having been made to capture them was, having no net by which it could be done; and thus two rare specimens, and among the desiderata of science, were lost.

At certain seasons of the year, at the Mauritius and elsewhere, some kinds of fish are regarded and are known to be poisonous; whilst at other seasons the same species are quite wholesome. Thus, it is said at the Mauritius, the fish are of a poisonous nature "*when the coral is in bloom:*" if so, are zoophytes of the coral poisonous?—do the fish feed upon them? If so, they must possess in themselves deleterious properties, and, alalthough harmless to the fish, must be of a nature poisonous to human beings that feed upon them, or rather the fish which used them as food at that season; when, at other seasons of the year, the same species are perfectly wholesome.

The "red fish," mentioned by Captain Cook, and which was caught at the New Hebrides Group, proved poisonous to those of his crew who partook of it; yet in the Bay of Erromanga (one of the islands of the same group) one of them was caught on board the ship Sophia; but, from its being recollected that Captain Cook had mentioned it as poisonous, it was not eaten by us; but some of the natives of the island being on board at the time, it was given to them; they took it, and said it was "kasipakasi," (good), "tamataka" (eat.) From this it is not improbable, that, although at one season of the year it may have been unwholesome, yet at another it may be perfectly wholesome.

We did not know sufficient of their language to ascertain whether they were or were not aware of the circumstance; but, at all events, they eat of this fish without experiencing any ill effects from it. We caught the fish in the month of August.

The mackarel of Sydney, at one season of the year, is considered wholesome, at another, to be unwholesome, if eaten when "out of season."

The expedition which was sent from India against the Mauritius, in 1810, made their rendezvous at the Island of Rodrigues; and at that place so many of the troops became sick and lost their lives from eating fish of a deleterious quality, which were caught there at that season, that it has been stated they were obliged to return to India for reinforcements.

It is often the custom, when a dolphin is caught on board a ship, to put a dollar in its mouth, when boiling; and if the silver became black, the fish was declared poisonous. But is not the salt water in itself sufficient to turn the silver black?

On the 15th December I left Singapore.

On the passage down the Straits of Dryan, Banca, and Sunda, nothing occurred worthy of remark. About the Keeling, or Cocos Islands, a number of boobies, as well as the elegant pearly white Tropic birds (phaeton œthereus) were seen about the ship; and, although the former were often seen some days after, the latter continued with us (only seen, however, during fine weather) until about the longitude of the Mauritius.

On approaching the coast of Southern Africa, the large sooty petrel was seen flying about at some distance from the ship, as well as occasionally some of the smaller species of albatross, with white body and black wings, probably spadicea; and we lost them in south latitude 28 deg., and east longitude 8 deg., on commencement of February.

On the 24th of January, 1834, when in latitude 26 deg. 26 min. south, and longitude 46 deg. 48 min. east, and a range of temperature of from 74 deg. to 79 deg., several bonito (Scomber) were caught; they were not of that species of the scomber genus, commonly called bonito by the sailors, but were that species classed by Lacepede under the new generic name of caranx, and accorded in all its characters with C. Rotleri, except in having eight spurious fins below, and twelve above. The character of this species is as follows:—"Sides, head, and back, blue; ventral and pectoral fins red; the others violet and yellowish; ten spurious fins above and below; lateral line marked by broad acuminated scales; fourteen inches long. Inhabits Indian seas. Bloch, pl. 346."—*Stark's Elements of Natural History*, vol. i. p. 474.

The genus Caranx is thus defined:—"Carina of the lateral line formed by a row of imbricated ridged scales; a small fin before the anal, supported by two spines; pectorals long and pointed; teeth generally crowded, but in a very narrow band, sometimes scarcely perceptible."

The length of the specimens caught were from a foot to fourteen and fifteen inches.

I preserved the heart of one of the fish in spirits: it retained its irritability for rather more than half an hour after it had been detached from the body of the animal. The stomachs of the specimens contained a variety of food, such as half-digested fish of small size, and of various species, and also a number of small sepiæ.

The liver of one of the fish contained several parasites, of a round form and whitish colour, imbedded in its substance.

On the evening of the following day, January 25th, being then in latitude 26 deg. 58 min. south, and longitude 44 deg. 52 min. east, with a range of the thermometer from 74 deg. to 76 deg., several of the same species of fish were captured. In their stomachs were found some half-digested fish, and a few perfect, but small specimens of a species of balistes, of a light bluish or violet colour; as well as also a small species of diodon, and several hyalæa.

On the 27th of January, in latitude 28 deg. 56 min. south, and longitude 39 deg. 56 min. east, and the range of the thermometer from 75 deg. to 80 deg., I captured with the towing-net in the evening a specimen of physalia, alima hyalina, smerdis, &c.

On the 1st of February, during drizzling rain, and when the ship was distant at the time sixty or seventy miles from the southern coast of Africa, a small and pretty reddish-brown hawk, with wings and tail barred with black, was seen flying about the ship, occasionally alighting upon the studding-sail booms or long-boat. As the wind was from the sea, and had been so for several previous days, it could not have been blown off, but probably came off in pursuit of prey, and was incapable

from fatigue of returning. In the afternoon we did not see it; and as the weather was squally, with heavy rain, it had been very probably blown to leeward, and unable, in consequence, to regain the ship.

On the 5th of February, in latitude 35 deg. 3 min. south, and longitude 21 deg. 34 min. east, with a range of thermometer from 75 deg. to 78 deg., being at first calm, and afterwards light breezes from the E.S.E., during the day, I used both the dipping and towing-net, but not with the expected success. I captured a fine specimen of velella, which does not appear to differ from seaphidea: the longer tentaculæ, situated around the outer margin of the disc, were very perfect in this specimen, and were of a bluish-white colour, tipped with orange at their terminations. A small shrimp-like crustaceous animal, with a long spine projecting from the head, was also caught.

Besides a number of velella and physalia, which were floating about the ship this morning, being on the bank of L'Agullas, there were also some salpa, of very large size; but they swam so deep as to prevent their capture by the dipping or towing-net.

On the 6th of February, in latitude 34 deg. 46 min. south, and longitude 20 deg. 40 min. east, the range of the thermometer being from 74½ deg. to 76 deg. The weather was fine, with calm and light breezes, and we were on the L'Agullas bank, when I again tried the towing-net, and with more success than before.

About 2, P.M., found in it a number of the physalis pelagica, commonly known by the name of the Portuguese men-of-war, and by others confounded with the nautilus. All the specimens in the net were of small size, but delicately and prettily tinted with carmine upon the bladder portion of the animal, and pending from beneath were numerous long purple tentaculæ, which, having been broken off, and left about the net, annoyed me with their stinging properties during the time I was occupied in exploring the contents of the net. Several velella were also in the net, not differing, however, from V. scaphidea. There was a young bonito of about two inches in length, but I did not consider it worth preserving.

A very small and delicate shell, of a beautiful violet colour, which was mingled with the confused mass in the net, attracted my attention: it was a very small specimen of Janthina communis, but was without an inhabitant. This genus is never met with but at the surface of the water; and the animal is stated to float suspended at the surface by a vesicular appendage, which adheres to its foot.

This was a busy day for my investigation of objects of natural history; for several bonito were also caught from the bowsprit of the ship with hooks and lines. The species called bonito were readily recognized by the deep-purple back, and longitudinal black stripes upon the sides. On being opened, I found imbedded in the internal muscles lining the sides, a number of small round white epizoæ; in some of the fish being embedded only under the fascia, whilst in others they penetrated into the substance of the muscles in great numbers. Although, in some of the fish, these parasitical creatures would be seen in large quantities, in others they were comparatively few; but it was very rare to find them entirely absent. Some could be seen attached by their mouth, which seemed to be surrounded by several suckers or tentaculæ. I have preserved some specimens of these epizoæ, imbedded in the muscles of the bonito, in spirits: their colour being white, they can usually be distinctly seen through the muscles when the fish is opened.

I examined the stomachs of all the fish caught, and found them empty; but on continuing my investigation through the intestinal canal, I found the latter contained a quantity of dark-grey sand, among which several specimens of *hyalæa* (*tridentata* (?), and other apparently different species of the same genus, I picked out and preserved. The *hyalæa* were principally of a reddish-brown colour. There was also a crustaceous animal, of a violet colour, but I cannot determine the genus.

At 3 P.M. I again captured several *physalia* and *velella*, and a small fish of a very light greenish colour upon the back; the sides and abdomen silvery. There was also a small species of *physalia*, of which the bladder portion of the animal, as well as the tentaculæ, were of a light purple colour.

On the 12th of February, fine weather, with a range of the thermometer from 70 deg. to 70½ deg. and in latitude

27 deg. 9 min. S., and longitude 6 deg. 43 min. E., at 2 P.M. I captured in the towing-net several *salpa* and *physalia*. I found that an application of vinegar in some degree tends to alleviate the pain produced by the irritative fluid secreted by the tentaculæ of the *physalia*. The crest of the latter was very low, slightly tinged with carmine, and the furrows of the ridge, or crest, were on each side of a fine sea-green colour; the bladder had both of its extremities beaked, and of a bluish colour, terminated by sea-green; the purple tentaculæ came off (very short, however) from one of the beaked portions of the bladder, as well as from the usual situation at the under surface.

There was also in the net an animal, in an imperfect state, which bore some resemblance to *asterias*.

In the course of the day, I only succeeded in capturing, with the net, a large *salpa* and a small specimen of *physalia*. This small *physalia*, when placed in spirits with the larger specimen, manifested a rather curious attraction to it; for although it was placed at some distance from the larger one, yet it approached the larger or stationary one, and adhered to it; if shaken or placed away from the larger *physalia*, it still continued to re-approach and attach itself, as if by some magnetic power. When placed in spirits, the beautiful colours of these molluscous animals are lost, but without imparting any of their colouring matter to the alcoholic liquid.

On the 13th of February, in latitude 25 deg. 56 min. S., and longitude 5 deg. 10 min. E., and range of the thermometer 70 deg. to 75½ deg. At one P.M. I hauled the towing-net, which contained several times two *salpa*, both different; one having apparently pink ovaries, the other of different form, and quite transparent. On the 16th inst., as well as previous days, I captured similar specimens; but what I consider curious is to always find only two (as if male and female) together, and each different; and finding them together so very often has occasioned me at last to notice the circumstance: one specimen has pinkish bodies, which I regard as ovaries; the other was perfectly transparent.

On the 15th of February, in latitude 24 deg. 13 min. S., and longitude 3 deg. 17 min. E., with a range of thermometer from 71 deg. to 76½ deg. I was unsuccessful with the towing-net during the day, but in the evening, after dark, several small mullusca were caught, which emitted a phosphorescent light; they were vitreous and of peculiar form, having a resemblance to a horse's foot; on holding them to the lighted lamp, streaks of phosphoric light could be plainly seen. The specimens were of different sizes, and their peculiar form will enable them to be readily recognized: they appear to me to be probably *Biphora*.

On the 16th of February, having fine weather and light breezes from south, with a range of thermometer during the day from 73 deg. to 76 deg., and latitude 23 deg. 45 min. S., longitude 2 deg. 50 min. E., a number of *Physalia* were floating about, and often got entangled in my net, but during the day I succeeded in capturing no other molluscous or crustaceous animals; but after dark, at 7 P.M. on taking in the towing-net, I found, among several *Salpa* of a similar species to those before caught, some very fine specimens of *Hyalœa tridentata* (vulgarly called the Chariot anomia), of the class *Pteropoda*, and also a small but beautiful vitreous shell. The Cleodora of Peron contained the soft parts. The edges are irregular, as if broken, and are probably fractured, as the shells are extremely fragile and require to be handled with care.

At 8 P.M. I again captured three more specimens of the above shells, the whole of which contained the soft parts. There were also several fine specimens of *Hyalœa tridentata*.

On the 17th of February, in latitude 22 deg. 58 min. S., and longitude 1 deg. 56 min. E., at 3 A.M. the towing-net was cast, and at 6 A.M. it was hauled in, and found to contain a number of *Salpa*, differing from those before captured, by having a white and yellow canal, running in a longitudinal direction down the body, and a violet line on each side of the body: it in some degree accords with *Salpa pinnata* of Lamarck.

It is asserted that these animals have the voluntary power of inflating and depressing the bladder at will; this faculty in the habits of the animal, I have never yet been able to observe, although I have devoted some time and attention to the subject whilst engaged in watching the habits of this class of the *Acalepha*. On attempting to preserve

them in a dried state, the bladder did not collapse unless accidentally injured; to which they seem to be very liable if touched when placed for the purpose of desiccation. On placing one near a lighted candle, after it had been eight or ten hours out of the water, the air within became rarified, could be felt expanding in the bladder, and at last burst with an explosive, but not powerful, sound. This certainly must be sufficient to demonstrate that no aperture exists by which the air could escape, either at the will or upon the death of the animal.

At noon the net contained several small *Salpa*, of a beautiful pink colour, and several *Physalia*.

At 8 P.M. I found in the towing-net, among several small *Physalia*, some fine specimens of *Hyalæa tridentata*, *Hyalæa*, apparently of a different species.

On the 18th of February, latitude 21 deg. 57 min. S., and longitude 1 deg. 5 min. E. Two very fine *Albicores* were captured in the morning. At 6 P.M. on hauling in the net, I found it containing a very large quantity of a small *Salpa*, of a delicate pink colour; among which were several transparent animals, of a serpentine form, in some degree resembling embryo fish.

ON MAGENDIE'S THEORY OF THE SOUNDS OF THE HEART;

Two Letters addressed to the Academy of Sciences,

By M. Bouillaud,
Professor to the Faculty of Medicine of Paris, &c.

Letter I.— *In reply to the First Part of M. Magendie's Memoir* *.

The problem of the sounds of the heart has for some time engaged the attention of physiologists and physicians. In one of the late sittings of the Academy, a memoir was read on the subject by one of your most distinguished members—M. Magendie. However simple and ingenious the theory of this celebrated physiologist, I must confess that it does not appear to me to satisfy all the conditions of the problem: it only, at best, raises a doubt of that theory which attributes the double sound to the play of the valves. It was with a view to remove the uncertainty thus thrown on a question which has been for some years an object of attentive study with me, that I had recourse to direct experiment; and the following are the results which I arrived at.

Yesterday, the 23d February, 1834, I laid bare the heart of a strong healthy cock, having previously explored the double sound of the organ. I heard the sounds while the heart was still enveloped in the pericardium, and after that membrane was removed. I heard them with the naked ear, and also with the stethoscope: I repeated the trial several times, and I can aver that I very distinctly heard the double sound, or *tic-tac* of the heart, though there was no sort of contact between the organ and the parietes of the chest,—none whatever between it and the sternum. I even placed a pretty thick piece of linen between my ear and the heart, to avoid the blood flowing from the wound,—and still I heard the double sound: in the latter case, however, it was as if muffled *(voilé.)*

I observed that the friction of the heart against the end of the stethoscope produced a particular noise; but so different from that of the *tic-tac*, that there was no possibility of confounding them. I removed the heart from the chest, having divided all the vessels by which it is, as it were, suspended; and it continued to pulsate for some moments, either spontaneously, or by the influence of an artificial excitement. These pulsations *in vacuo (à vide)*, so to speak, were not accompanied by any sound. The cock, by the way, still continued to live for some moments after the excision of his heart.

I repeated the same experiment on two rabbits of moderate strength, and it was attended by the same results; that is to say, I heard the sounds of the heart perfectly, after the organ had ceased to have any connexion with the walls of the chest. I would merely add that I did not notice very distinctly in the cock the contraction of the auricles; but in the rabbits I did so in the most satisfactory manner: the contractions, however, were certainly better marked in the appendages of the auricles than elsewhere.

It is not my intention to draw any decisive inference from these experiments, which are at variance with those

* Med. Gaz. present volume, p. 440.

of one of the first physiologists of the age; but I am desirous of submitting them to the judgment of the academy, as well as to M. Magendie himself.

In conclusion, I have only to observe, that the results of the examination of the sounds of the heart, as I have obtained them, only tend to confirm me in the opinion that the double sound is principally owing to the play of the valves; and that the ingenious system of M. Magendie cannot be admitted without some reserve and drawback. Dr. Hope's experiments, I may add, on larger animals, as on the ass, are perfectly conformable to those I have just now described.

LETTER II.—*In reply to the Second Part of M. Magendie's Memoir* *.

The indulgence with which my first letter was received by the Academy, induces me to follow it up by some remarks also on the second part of M. Magendie's Memoir on the Sounds of the Heart. I trust that both the academy and the author of the memoir will excuse an importunity which is grounded simply on the importance of the subject and an anxious desire to ascertain the truth. Although I have been occupied for ten years with the problem of *living acoustics* now treated by M. Magendie, it is not without hesitation that I venture to offer some objections to the ingenious theory of this illustrious physiologist.

Previously to examining the principal facts contained in the second part of the memoir, I shall take the opportunity of mentioning that there is at least a triple source of anormal sounds of the heart.

1. The first cause of irregularity in the heart's sounds arises from *organic* lesions of the internal membrane of the heart, and of the valves round which it is reflected. The most remarkable of these lesions consists of different kinds of induration of the valves, with more or less of contraction in the apertures to which they are adapted. By these there is produced such an impediment to the course of the blood through the heart, that the various well-known bellows-sound, rasp, and saw, and file sound, as well as occasionally a chirping or whistling noise, as of a bird, (which, I believe, I was the first to discover,) are all generally traceable to this cause.

2. From lesions of the pericardium. Thus, for example, when the opposite surfaces of this sac have become wrinkled, uneven, and in some sort rough, there arises in the reciprocal movements of the one against the other a rubbing noise; and hence that diffused and superficial rasp sound, or that crackling, as of new leather, which is heard in certain cases of pericarditis. Effusions in the pericardium also modify in another manner the sounds of the heart; they dull them, and make them seem more distant, &c. I have met with a case in which, owing to an ossiform concretion on the surface of the pericardium, there was, during the whole time the patient was under observation, a grating—a *bruit de râclement* very distinctly marked.

3. Lesions of the muscular substance of the heart itself, or of its exciting principle, constitute the third source of anomalous sounds. Owing to lesions of this kind, the pulsation of the heart against the walls of the chest is in some instances augmented, in others enfeebled. As this pulsation only takes place, in general, during the systole, it is to this that the anormal sounds in this division are to be referred. In a considerable number of patients, especially of meagre habit, I have often heard, and caused others to hear, a clear silvery tinkling sound, depending, beyond question, upon the shock of the heart against that part of the chest to which the ear is applied.

We shall now see whether these anormal sounds, thus produced, are favourable or unfavourable as arguments for M. Magendie's system. In the first place, it is quite evident that the sounds of the rasp, the saw, the bellows, &c.—which modify so thoroughly, not merely the force or intensity, but the very character of the *tic-tac*, and which constitute our first variety or valvular lesion—these cannot support the new theory. In fact, if the double sound of the heart really depend on a double shock against the chest, why does it not continue in its proper form, instead of assuming those varieties of the rasp, saw, &c.? If, on the other hand, the sounds of the saw, rasp, and bellows, are only modifications of the normal *tic-tac*, is it not reasonable to conclude that the latter depends on the play of the valves, and on the passage of the blood through the

* Med. Gaz. 878, *ante*.

apertures which these serve to close, since it is owing to lesions of these parts that the modifications of the double sound must be attributed? M. Magendie, it is true, admits that the contraction of the orifices sometimes impedes the first shock, sometimes the second; and that it is to this circumstance that the absence of the first sound must be assigned in one case, and of the second in another. But I will venture to affirm, with all deference for the experimental tact of M. Magendie, that I have met with above fifty cases of contraction of the orifices, in which the organ, far from having ceased to strike against the precordial region, struck against it with augmented force. Thus, then, it appears to me that the modifications in the sounds of the heart, arising from lesions of the valves, cannot be put forward as lending the least support to the new theory.

In the next place, the sounds resulting from the different lesions of the pericardium are equally distant from supporting M. Magendie's views. To mention only the phenomena arising from effusion into the sac, has it not been demonstrated by numerous facts and observations, that the double sound is heard, enfeebled, and as it were afar off, although the heart can no longer strike, or, so to speak, *percuss* the precordial region? Just now, in fact, I have met with a case completely confirmatory of this observation.

And lastly, to revert to the anormal sounds of the third category. In those cases in which the heart pulsates strongly enough to produce a peculiar sound, the *tintement metallique*, for example, there is no hindrance to the hearing also, at the same time, the double sound. The tinkling is therefore a superadded sound, accidental, and independent of the normal *tic-tac*. Besides, this sound is simple, and only takes place during the systole, while the sound of the heart is double: consequently, it is impossible to rest the theory of M. Magendie on this fact. That proof on which our celebrated physiologist seems to place most reliance, for the triumph of his doctrine, is the absence of the sounds of the heart in considerable hypertrophy of that organ, where the volume has become so great that no interval is left between it and the chest. There is in this case, according to M. Magendie, no shock properly so called; there ought, therefore, to be no sound.

I am ready to allow, that if the circumstances attending hypertrophy were always such as they are described by M. Magendie, his system would be, if not more certain, at least more probable. But they are far otherwise. I have met with above two hundred cases of hypertrophy, and I am bound to declare that I have always heard, and that very distinctly, the sounds of the heart in every one of them, modified, according to the complications present, either with respect to their intensity, their nature, and, so to speak, their *timbre*, "every time the ventricles contract and dilate; when the blood penetrates them and is expelled; when the hand, applied to the precordial region, experiences violent impulses; and when the thorax itself is raised in a remarkable manner."

For my part, then, I must only conclude by observing, that the theory of M. Magendie on the sounds of the heart, however ingenious it may be, and however imposing the authority of its learned inventor and advocate, fails in supplying as good an account of the anormal sounds as that theory does which I had the honour of mentioning to the Academy in my former letter, and which assigns as the cause of the *tic-tac*, or double sound of the heart, the play of the valves and the passage of the blood through the apertures of the organ.

August 25, 1834.

CASES OF LITHOTRITY.

To the Editor of the Medical Gazette.

Sir,

Should the following selection of cases in lithotrity be deemed of sufficient interest, for insertion in your publication, they are entirely at your service.

Your obedient servant,
Edgar A. Spilsbury, M.R.C.S.

Walsall, Sept. 14, 1834.

Richard Hardin, bordering on eighty years of age, had been for fifteen years afflicted with stone: his sufferings determined him to seek relief in lithotrity.

Wednesday, Aug. 16, assisted by my friends, Mr. Holland, surgeon, of Can-

nock, and Mr. Dehane, surgeon, of Wolverhampton, the bladder was injected, and the percussor was introduced. The stone was readily grasped, and the scale of the instrument indicated its diameter full one inch and a half. Owing to its compactness, it was not until twenty minutes forcible hammering that disintegration was effected; but the vice and percussor being completely adjusted, *no pain* was experienced from this violent part of the operation.

Up to the Sunday following, not any unfavourable symptoms were apparent, but much pulverized sand, and portions of the size of a split pea, had by urine been voided. On that evening a shivering fit set in, and the following day, Monday, at 12 o'clock, a coated tongue and unfavourable pulse left no hope of the patient's safety.

The bladder was again injected, and many pieces of an inch and five lines were broken up: symptoms of exhaustion being apparent, the patient was put to bed; and at 6 o'clock, A.M. the following morning, he expired.

The operation of injection is painful, and occasionally the bladder contracts upon the percussor, with spasms.

Mr. Holland conducted the examination, and thus writes:—"By strenuous exertions, I gained permission to examine the bladder. The walls were thickened to the eighth of an inch, internally much corrugated. There was not the slightest vestige of inflammation or abrasion of the internal coat; but just above the pubes, between the coats of the bladder, was an ecchymosed patch, the size of a shilling; within, lay about two drachms of pulverized phosphatic calculus, many pieces the size of a split pea, and three portions the size of a small marble. The kidneys were not examined."

Such was the result of a case by no means fitted for lithotrity. The size of the stone, and the sinking state of the patient from long disease, forbade it; but I must affirm, it was an operation unattended with severe pain.

For the selection of the following successful case I am indebted to E. Dehane, Esq. who, in a letter to me, begged I would transmit his statement, accompanied by my own, for publication. I give it in his own words.

"William Cours, æt. 56, a healthy labourer, employed by J. Hellier, Esq. Barnehurst estate, near Wolverhampton, became my patient in the month of June. His complaint, a difficulty of passing his urine, with frequent retention. When distended, the bladder was highly sensitive, and relief gained by the use of the catheter. Suspecting the existence of calculus, I sounded him, and proved its presence. I considered this a fit case for lithotrity, and mentioned my opinion to my friend, Mr. Spilsbury, of Walsall, who had recently made use of Baron Heurteloup's instruments and apparatus upon a patient in his neighbourhood. On my solicitation he saw the case, and agreed with me in opinion as to its eligibility and fitness to be submitted to the Baron's process.

"Wednesday, the 20th August, Mr. Spilsbury, assisted by Mr. Holland, Mr. Coleman, and myself, proceeded with the operation.

"The patient being sounded, and the stone felt, he was placed on the Baron's bed, and the bladder injected: the percussor was then easily and gently passed into the bladder, and the stone immediately seized, the graduated scale of the instrument indicating nearly six of the spaces, shewing the correctness of the opinion — a small stone; and, as was afterwards proved, composed of lithic acid. The vice was now firmly fixed; the percussor adjusted to it: five moderate taps with the hammer crushed the stone, and the operation was over in a few minutes. Our patient expressed his intention of going to the harvest field, but was put to bed, and kept there.

"The bladder was washed out, and some particles came away; and, in a few days, many fragments passed with the urine. He has now resumed his employment, being quite recovered, and free from any inconvenience.

"It may be observed that this operation was unattended with any pain, excepting from that caused by the injection of the bladder. The patient was not sensible of any increase from the crushing."

Having within the last six years performed the operation of lithotrity thirteen times, and one case only being unsuccessful, (an encysted stone being removed,) owing to an abscess having burst among the bowels from the kidney on the third day when the patient suddenly sunk, entitles me to a hearing on the comparative merits of the operation.

The great evil of lithotomy, in my opinion, is the imperfect restoration of the apparatus of the bladder; for, let the case be as successful as it will, there most frequently is a defect—an occasional dribbling. This does not depend on the healing of the external incision in ten days or five weeks; or a small opening into the bladder, or an extended one; neither on much or little violence exerted in extraction; or whether you make use of the gorget or the knife; or the straight-grooved or curved staff*. If you make the opening very low down in the membranous part under the pubes, and leave, as you ought, the upper part whole, still, in all these cases, we do not make the valvular apparatus quite perfect, except in the horizontal position in bed, where the bladder is perfectly retentive. It is the hanging down of the penis that favours the jet, and this would suggest its being kept with the glans confined upwards. With children, however, it would be impossible to enforce this.

Do away with this evil, and lithotomy, I should say, will never yield to lithotrity, except in the early formation of stone. In such a case as that of Corns, it is a beautiful adaptation of mechanism; its success triumphant; and should stone never be suffered to get large, by the application of lithotrity there would be no occasion for lithotomy.

ON THE COMPARATIVE MERITS
OF
DR. BELLINGERI'S AND SIR C. BELL'S WRITINGS
AND OPINIONS ON THE FUNCTIONS OF THE FIFTH AND SEVENTH PAIRS OF CEREBRAL NERVES.

By G. Negri, M.D.

[Continued from p. 885.]

Concluding Remarks on the Physiology of the Fifth Pair of Cerebral Nerves.

With the last article has been brought to a conclusion the exposition of Bellingeri's doctrine on the complete physiology of the fifth pair of cerebral nerves. In doing this, I endeavoured impartially to select the most important parts, leaving out only those which I considered either of little or of no material importance to the fundamental principle of the subject; and, with the view of avoiding any misrepresentation on my part, I thought it better to give Bellingeri's opinions in the original language.

Now it remains for the impartial and enlightened part of the English medical profession to judge of their real merits, and of that of the question in general. They will now also be able to see more clearly if Mr. Alexander Shaw was in any way justified in advancing, against Bellingeri and his doctrine on the fifth, the following hazardous statements:—"Bellingeri ..... *recklessly made a bold guess* at what its functions (of the ganglionic portion) ought to be; he failed in his conjecture: *he pronounced it to be at once the nerve of motion and of sensation:* he has thus thrown a cloud of error over his only correct statement (viz. of having called the smaller portion a nerve of motion). We admire Paletta for his philosophical caution and forbearance, as well as for being the originator of the idea. *We must decline placing confidence in any assertion of Bellingeri*.*" "*This lucky foreigner—neither making experiments on the fifth pair, nor on the spinal nerves, nor hitting upon the true functions of the nerve in question*, but keeping all things in their pristine confusion—has gained the *unqualified applause* of his reviewer; nay, he is held up as one injuriously dealt with by the whole profession†."

I do not know, indeed, how Mr. Alexander Shaw can venture to insist so much on the experiments of Sir Charles Bell, performed, more particularly, on the fifth, and find such great fault because "Mr. Bellingeri," as he says, "*has not been at the pains to make a single experiment.*‡"

Were it not at present generally known, and even by Sir Charles Bell himself candidly admitted, that the first conclusions drawn from them were not entirely correct, I should be sorry to enter here on such a delicate subject.

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* I came to the conclusion that it is extremely desirable in all cases to open the urethra very low down, from the external wound readily healing, and from having observed that some cases healed in ten days, whilst others were protracted to five weeks; though some cases were retentive in three days, and the natural passage restored; but then, again, how much violence the perineum suffers, and yet gets well; whilst the upper part of the urethra, from slight causes, gives great constitutional disturbance. In a conversation on this subject with Mr. B. Cooper, he agreed with me, and referred me to his opinions stated in his work.

* Med. Gaz. July 19, p. 563.
† Id. p. 562. ‡ Id. p. 561.

It was, in fact, from the experiment on the very same infra-orbital branch of the fifth that Sir Charles Bell was principally led into error in ascribing to it both a power of motion and sensation; when Bellengeri, on the contrary, brought forward as a pathological argument, to prove that the ganglionic portion of the fifth was not a nerve of motion, the division of the same branch performed in instances of *neuralgia facialis*, which was not followed by paralysis of the muscles of the face.

But, after all, the best answer I can give to Mr. A. Shaw is in Sir Charles Bell's own words:—"Experiments," says he, "may take a colour from the preconceived idea, but accurate investigation of the structure will not deceive us."—(Last work, p. 133.) "Experiments have never been the means of discovery; and a survey of what has been attempted of late in physiology will prove that opening of living animals has done more to perpetuate error than to confirm the just views taken from the study of anatomy and natural motions."—(P. 218.) "I have had recourse to experiments, not to form my own opinions, but to impress them upon others."—(Id.) "I feel a hesitation when I reason upon any other ground than on the facts of anatomy."—(Id. p. 237.)

These were Sir Charles Bell's opinions on the subject of experiments, brought forward at different times after the presentation of his first paper to the Royal Society in 1821, and evidently expressing the important result of his own experience.

It is only to be regretted that Sir Charles Bell had not, at a former period, directed his particular attention to the minute anatomy of the fifth nerve. Had he not had his mind so much engaged with his favourite theory of the respiratory nerves, and not been satisfied merely with the analogy of the spinal nerves, and the fifth, by its double origin and ganglion, he would never have, as he stated in his last paper, read before the Royal Society in May 1829, "attributed to one of its branches a function which belongs to another branch of the same nerve."—(Last work, p. 94.) Of this very branch he had said before, in his first paper in 1821: "I am unable to decide whether or not the muscular branches of the fifth nerve go exclusively to the muscles of the jaws, and not at all to those of the cheeks."—Ibid. p. 82.

It is, in fact, only in his last paper, already mentioned, and after the lapse of nine years from the first, that Sir Charles Bell gave a short description of the smaller or muscular portion of the fifth, together with a plate, which merely shews its appearance in the face and its principal ramifications to the different muscles of the jaws; and just forty-five years after Paletta had published his classical work on the *Nervus Crotaphiticus et Buccinatorius*, with plates only to be compared to those of Scarpa, and had called it *a nerve of voluntary motion.*

[This imperfect knowledge of the anatomy of the smaller portion of the fifth nerve is not surprising, since we see that Mr. Mayo also, in 1829, in the second edition of his Outlines of Physiology, had stated, p. 335—"In pursuing this subject, I was led to observe, that there were muscles which received no branches from any nerve but the fifth; these muscles are the masseter, the temporal, the two pterygoids, and the circumflexus palati.....After some careful dissection, *in the greater part of which I afterwards found that I had been anticipated by Paletta*, I made out that the smaller fasciculus of the fifth is entirely consumed upon the supply of the muscles I have named." This impartial author has thus for the first time given credit to Paletta, more explicitly than any other in this country, for his careful inquiries on the minute anatomy of this part of the fifth nerve.]

What were the consequences of neglecting this important inquiry on the minute anatomical distribution of the fifth, may be known better by the candid statements that Sir Charles Bell exposed in his last paper. "Conceiving (says he) that there must be such an accordance (*between the motions of the lower jaw and the cheeks in mastication*), and contemplating the roots of the fifth pair, and their distinct functions, I had *imagined* that this office was performed by the branches of the second division of the fifth: but finding that the connexion between the motor root and the superior maxillary nerve proved to be only by cellular texture, and considering the affirmation of M. Magendie and those who followed him—that the infra-orbital branch had no influence upon the lips—I prosecuted with more

interest the *ramus buccinalis labialis*."—(Last work, p. 104.) This curious fact (*of the fifth being not only the nerve of sensibility to the head and face, but a muscular nerve to the muscles of the jaws active in mastication*), originally drawn from anatomy (*I should rather say from analogy*), and now confirmed by it, had nearly been obscured by experiment: since the external branches of the fifth nerve, those most exposed to the experimenter, are not muscular."—(Ibid. p. 107.) "I am bound to acknowledge here the correction by M. Magendie, in regard to the office of the suborbital division of this nerve, since it has given occasion to the revisal of the anatomy."—(Ibid.)

Nor had a more diligent attention been paid to the minute anatomy of the larger portion of the same nerve; since we found, in Sir Charles Bell's fifth paper, read before the Royal Society, in February 1826, the following statements: —"For a time I believed that the fifth nerve, which is the sensitive nerve of the head and face, did not terminate in the substance of the muscles, but only passed through them to the skin."—(*Id.* p. 228.)

"Still dissection did not authorise that conclusion. I traced the sensitive nerves into the substance of the muscles: I found that the fifth pair was distributed more profusely to the muscles than to the skin; and that, estimating all the nerves given to the muscles, the greater portion belonged to the fifth, or sensitive nerve, and smaller portion to the seventh, or motor nerve. On referring to the best authorities, as Meckel, and my excellent preceptor Monro, the extremities of the fifth were described by them as going into the muscles; so that of this fact there cannot be a doubt."—(*Id.* p. 229.)

Bellingeri, then, had not performed any experiments on the fifth, probably because, by its intricate distribution and connexions with other nerves, he might have been induced to draw erroneous deductions from them; he had not paid, at that time, a particular attention, or performed any experiment, on the spinal nerves, because the principal object of his inquiries was to establish the general pathology and therapeutic of the *Neuralgia facialis*. Instead of that, he had first most minutely studied the anatomy of all the nerves of the face; and of the fifth, in particular, he has given to the public, in 1818, one of the most complete and elaborate monographs, both in point of anatomy and erudition, of that important nerve of the face. Of all of them he has searched their *real original roots into the most recondite parts of the encephalon;* and from this source, and from their distribution to the different parts and organs of the face, as well as from their functions, both in a state of health and disease, he has *attempted* to establish their peculiar physiological power. In doing this he has always proceeded with that degree of philosophical caution, which is characteristic of men of learning; being satisfied of having, in his attempt, at least arrived at probability, if not certainty.

Having thus concluded with these remarks the physiology of the fifth pair of cerebral nerves, I shall in my next article proceed to the not less interesting subject of the seventh pair of nerves.

September 22, 1834.

ON THE FUNCTIONS OF NERVES.

To the Editor of the Medical Gazette.

SIR,

ALTHOUGH by far the greatest number of muscles receive similar nerves, yet there are some destined for particular functions which have their supply from different sources. Thus each of the three slender muscles, arising from the styloid process, receives its particular nerve. This arrangement cannot be mere convenience, as either of the nerves might on this account have furnished branches to the whole. But a muscle may perform more complex functions, and have two distinct nerves as the digastric, or one may be supplied by two nerves conjoined, as the cutaneous, omo-hyoidcal, and others. A muscle in the face may also receive two different nerves, which may be either separate or conjoined.

The superior portion of the digastric muscle receives a branch from the hard portion of the seventh, the inferior from the inferior dental nerve. If the inferior dental nerve, just before entering the jaw, can give off a branch for ex-

citing muscular action, there is no reason why others given by it to the muscular structure of the lower lip, after it has emerged from the foramen near the chin, may not perform similar functions; and if it have this power in the lower lip, why should not branches of the infra-orbital nerve, terminating in the upper-lip, have the same?

Although experimenters have decided that the infra-orbital nerve has only sensiferous faculties, as well as the termination of the inferior maxillary in the lower-lip, yet if branches of a nerve be found terminating in muscle, it may be reasonably concluded that they exist for exciting and controling the contractile fibres; and if the muscle be complicated as those of the lips, and receives two nerves, there seems to be no reason why it may not be excited by each of them for any particular occasion.

It is remarkable that the digastric muscle is supplied by two nerves in the same manner as the lips. In it the two different muscular parts are seen receiving different nerves. In the lips the destination of the different nerves cannot be so precisely determined, but their entire separation in this muscle allows us to conclude with certainty that more of the branches of the fifth have the power of exciting muscular action than is generally allowed, and also that the two portions of this muscle have to perform separate functions, and that it is employed in a similar capacity to those of the lips.

In the sheep there is a very slight communication between the fifth and the hard portion of the seventh; so that each nerve passes almost altogether separate to its destination. In the horse the branch of the third trunk of the fifth passing behind the condyle of the lower jaw, and corresponding with that giving off the superficial temporal in man, is very large, and becomes ultimately united with the hard portion; it is much less in the calf, and becomes also united with the hard portion; but in the sheep, the communication, when it exists, is very slight, and by a mere filament. Now this branch of the fifth in the sheep is distributed almost entirely on the levator muscle of the angle of the mouth and the cutaneous muscle of the face, but the levator also receives filaments from the hard portion. Branches from the second trunk of the fifth in this animal terminate in the muscles of the upper lip, and others from the third in the lower. It is very difficult to trace the branches of the hard portion throughout the lips, so as to determine whether the whole of these parts is supplied by its branches.

In the calf, the branch of the third trunk of the fifth passing behind the condyle of the lower jaw, before its junction with the hard portion, gives off a branch, which is joined by one from this nerve, and is then distributed to the levator muscle of the angle of the mouth; the remaining part of the branches given off from this nerve pass to the cutaneous muscle and skin.

When the distribution of the nerves of the face in the sheep is considered, it must be concluded that the fifth and the hard portion of the mouth are connected merely for sympathy, as the communications between the two are so slight; and it may be here remarked, that the union of the fifth and seventh will not anatomically bear a comparison with that of the anterior and posterior bundle of each spinal nerve.

The nerve of the fifth, passing from behind the condyle of the lower jaw in the horse and ass, has been conjectured to be a nerve of sensation, and imparting this quality to the hard portion. Experiments have been made to prove that it is so, but have not done it satisfactorily. This nerve in the sheep is distributed almost entirely on muscles, as previously stated. It may, therefore, be maintained, that it has motiferous power in this instance; and if it produce sensation altogether in man and some animals, it shows, at least, that the same nerve in different animals has different functions.

It has been supposed that a sensiferous nerve is required for the functions of the muscles; this, however, need not be separated from the motiferous. But if a similar branch of the fifth promote muscular functions in one animal, and sensitive in another, there may be some difference either at the origin of the nerve, or some different connexion in different animals. The ninth alone is sufficient for the functions of some muscles, and therefore the branches of the cervical nerves cannot be joined to its descending branch for merely promoting muscular functions.

In the calf, the cutaneous muscle and

skin above the eye-lid, on the forehead, and the anterior part of the nose, are supplied by the supra-orbital nerve, the branches of which communicate with the hard portion. Lower down on the nose they are supplied by branches of the infra-orbital nerve turned upwards; but these have not communicated with the hard portion before they are given to the cutaneous muscle. It appears, therefore, that part of this muscle is supplied by the hard portion, and part by the branches of the fifth; but in all the rest of the body, the cutaneous muscle is almost entirely supplied by the spinal nerves. The spinal nerves in man terminate at last altogether on the skin, as the cutaneous muscle does not exist except in the neck; but in animals they furnish the cutaneous muscle, as well as the skin; and branches of the first trunk of the fifth terminate in the occipito-frontal muscle and skin, whilst the extreme branches of the second trunk terminate on the skin, no cutaneous muscle existing in the face, as in animals. Thus the functions of the first trunk of the fifth in man are brought to resemble those of the spinal nerves, by terminating on both cutaneous muscle and skin, as in animals; whilst those of the second trunk correspond with the terminations of the spinal nerves in man, as there is an absence of the cutaneous muscle in the face.

In animals the cutaneous muscle is supplied with branches, after the nerves have pierced the several muscles, for terminating in the skin in man. Are we therefore to suppose that in animals the nerves are different from those in man, or that they are the same, the functions of the skin requiring nerves of similar qualities to those of the muscles?

If the nerves of sensation and motion are not mixed together for their mutual co-operation, it is difficult to conceive why they are so connected. If the motiferous portion be entirely for the muscles, and the sensiferous for the skin, why are they united? It seems that they require mutual co-operation, and perhaps the skin has an equal share of this with the muscles; for when a nerve has left the branches destined for the skin, and appears to be passing entirely to muscles, some of its filaments will be sent from this apparently muscular termination to the periosteum and bone, or to the joints, a plain proof that those branches have others besides motiferous properties, viz. in giving sensation to the bones and joints, and connecting these with the system generally.

It may be a question whether the cutaneous muscle, in animals, performs altogether voluntary functions; or whether the close connexion of its nerves with the sentient properties of the skin communicates these motiferous powers, making the contractions rather involuntary than voluntary, in the same way as the irritated nerves of the extremity of the foot in animals make the muscles of the limb contract. The sentient extremity of the nerve may convey the irritation to the brain, and the muscle be then excited by the will. This complex movement appears generally to be hardly necessary, but only on more urgent occasions, as after coming out of the water, when the animal can put the whole cutaneous muscle in action at the same moment. It is not difficult to conclude that the nerve supplying the cutaneous muscle and skin, on being irritated, may produce action like that of the sensitive plant, and contract in a similar manner. The bladder contracts on the application of cold to the skin.

Worms and insects have nerves for motion as well as sensation; and although these qualities may be modified, as compared with more perfect animals, there is good reason for presuming that their nerves may perform both these functions. In the centipede the brain has two lobes, clearly defined and different from the ganglia; the nervous cord is very large, and in its centre a fine white thread passes down, first through each portion of this prolongation, and then round each ganglion, so as to be distinct from the other parts. Whether this arrangement be for separating the nerves of sensation and those of motion, may be worth consideration.

I cannot believe that a great many of the lower animals are devoid of sensation; neither that muscles can perform voluntary motion without a brain or nervous centre. I can conceive that some of the lowest animals may possess sensitive qualities, and a capability for contraction, like the sensitive plant. It would not, perhaps, be necessary, in this instance, that there should be distinct nerves, but whether there must be nervous matter conjoined with the muscle or contractile membrane, or in what proportion, it is impossible to determine,

unless the peculiar properties of brain could be discovered by chemical analysis. The existence of brain and nerves has been denied to some soft animals, but it may not have been discovered from its transparency; as, even in the crab, it is somewhat difficult, on this account, to distinguish the nervous system from the muscular.

I am, sir,
Your obedient servant,
JOSEPH SWAN.

6, Tavistock-Square,
Sept. 23, 1834.

MEDICAL GAZETTE.

Saturday, September 27, 1834.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."
CICERO.

COLLEGE OF PHYSICIANS.

PERVERSION OF THEIR POWERS.

THE powers originally entrusted to the College of Physicians were so very ample, that, had they been wielded with honesty and discretion, it is very doubtful whether any other medical corporation would have been instituted in these kingdoms. Instead, however, of comprehending within their circle the greatest possible number of learned practitioners, their object has been to render their order as exclusive as possible. But those excluded having, in process of time, become the more influential as well as the more numerous body, the principle has acted against their oppressors; so that while the Fellows excluded others from their halls, they have given to those others both the power and the will to exclude *them* from a very large portion of that practice and emolument which they would otherwise have obtained. Many well-educated physicians are now apothecaries—licentiates of another body, and lending all that influence to the destruction of the temple in Pall-Mall East, which, under better management, would have been given to its extension and support. It may, perhaps, be said, that if well-educated men be engaged in practice, it matters little by what name they may be called; but we maintain, on the contrary, that there is a great deal in a name, and that the appellation in question—that of apothecary—has been too long associated with the idea of a mere dealer in drugs, for the public to use it in any other sense; nor can a stronger proof be adduced of this, than that the Licentiates of the Worshipful Society do not call themselves "apothecaries," but "general practitioners,"—shewing, most clearly, their consciousness of being superior in education and attainments to the order into which the mixed supineness and indiscretion of the College of Physicians, has driven them. No body of men can be expected to consent, that while the standard of their professional attainments is raised, their nominal rank should be lowered, and the consequences have been, first, that many young men of excellent education have been deterred from the pursuit of physic altogether; and, secondly, that of those who have entered it, not a few are influenced by feelings of hostility towards those to whom, in virtue of their office, the regulating of the profession has been intrusted, and by whom they think themselves aggrieved. We do not, indeed, go so far as Dr. Arnott, who attributes all, or nearly all, the evils of the profession to the College of Physicians, but we must allow that there is a great deal of truth in many of the remarks which followed.

"Have you considered the by-laws of the College which relate to the admission of licentiates to the fellowship?—I have.

"What view have you been led to take of the by-laws?—It appears to me, that the authority which was given to the College to make by-laws, instead of being used to raise the character of the profession, and increase its efficiency, has been perverted to

other purposes, and has produced the effect of throwing 19-20ths of the whole medical practice of the country—that is, the general practice of the healing art in this country—into the hands of persons not physicians, although the College of Physicians was originally established to embrace the whole.

" The effect of the by-laws of the College has been to restrict the number of physicians practising, and the consequence of that restriction has been to force another grade of practitioners into general practice?—Yes.

" Has not the effect of the laws in this country, with regard to medicine, been to transfer the great body of the practice to the general practitioner?—It has; but it appears to me that the result of the present law, if continual, would be chiefly to change the name of the regular medical practitioner from physician to surgeon or apothecary, with annoyance in many cases to the practitioner himself, and disadvantage to the public, owing to lower qualifications required. Apothecary originally meant a mere compounder of drugs; apothecary now may mean a well-educated physician,—for of late years well-educated physicians, owing to the difficulties there are in the way of practising with the denomination of physician, under the injurious by-laws of the College, have joined the inferior bodies; and the prospect is that before long the body called physicians will wear out.

" Is there any wisdom in, at the same time, lowering a class of men in point of rank, and raising them in point of mental acquirement?—No; there is great inconsistency in so doing.

" Has not that been the effect of the laws of this country, particularly the law of 1815?—It appears to me to have been the effect in a remarkable degree; and that as one consequence, many men of talent, who would be proud to belong to the medical profession, if well constituted, are now deterred from entering it, to the great injury of the public.

" So long as you require high qualifications of parties to enter into a branch of the profession which is considered low in point of rank, do not you think that dissatisfaction will exist in the great body of the profession?—There must be great dissatisfaction. Were an English apothecary, who may be a perfectly-educated physician, to visit the continent, and to declare that he was an apothecary, he would scarcely be admitted into good society.

" Has not the wholesome influence which it is desirable the College of Physicians should possess in this country, been diminished, in consequence of the dissatisfaction created by these exclusive by-laws?—I think so; and I believe that if those injudicious by-laws had not been made, there would at the present moment be little for the legislature to do but to extend the authority of the well-conducted college over the whole kingdom; the profession would have been in a satisfactory state, constituting a sufficiently numerous body of well-educated men, such as I have been contemplating.

It is quite clear that Dr. Arnott has a strong bias in favour of the one Faculty project—a result which we think has been produced in the minds of some learned and otherwise reasonable persons, by the re-action which the sense of exclusion is apt to produce. Indeed, the case of the physician before us is an illustration of the baneful consequences of the present system, which debars a gentleman from the highest honours of his profession, though he be at once an accomplished physician, and highly distinguished for his attainments in various departments of science not strictly medical.

We have, in former numbers, dwelt at some length upon the nature and bearings of the by-laws of the College of Physicians, as illustrated by the parliamentary evidence. We have endeavoured to show that the connexion between the College and the English Universities was one not unavoidably, though perhaps naturally resulting from circumstances; that they voluntarily, by the creation of certain by-laws, rendered their establishment rather an appendage of Oxford and Cambridge than maintained that independent position which their charter originally gave them; and that their illiberality towards their Licentiates has been no less injurious to physicians generally than to the Col-

lege itself. A sort of recapitulation of the leading points on these questions is contained in the following very elaborate answer of Dr. Arnott to an interrogatory on the subject of the petition of the Licentiates; and, with this summing up, we close a volume, and, for the present, our notice of this portion of the parliamentary evidence.

"Are you one of the licentiates who signed the petitions presented in this and last session to the legislature?—I am; I have signed both.

"What do you consider the leading grievances under which the licentiates at present labour?—The grievances are of two classes; one rather in relation to the public, the other more nearly affecting the licentiates. Legislation, with respect to the practice of medicine, could have in view only to ensure the public an abundance of the best medical service, and to protect the public against the practice of ignorant men and quacks. The charter originally granted to the College of Physicians seems to have been given with such intentions; in fact, it expressly states that those were its objects. The management of the College, however, instead of keeping these objects in view, seems almost entirely to have neglected them, and to have considered chiefly the personal advantage of the members, or corporate advantage of the body. That there might be abundance of the best medical talent in the country, it was necessary that the profession should be honourable and sufficiently lucrative, and that there should be free admission to its honours and advantages for those persons who, by acquiring the high attainments and character suiting a physician, were deserving of them. Thus young men about to choose a profession would have been attracted by the medical as readily as by any of the other honourable professions in the country — as of the church, the law, or the army. Instead, however, of the profession generally being rendered honourable, as it should have been, and attractive by the exclusion of the ill-educated and unworthy, the subsequent expulsion of persons who might become unworthy, the suppression of quackery, and there being free entrance for the deserving, many of the best-educated members of the profession have, by by-laws conceived in a spirit of monopoly, been severally excluding from the honours of the fellowship of the College, and have remained liable to be confounded with practitioners of very inferior acquirements and character, permitted by the College to exist, though I believe the College had the power of preventing them. Thus many high-minded persons have been prevented from embracing the medical profession, and some have even left it after they had embraced it. One consequence of this conduct of the College was, that the number of well-educated medical men, in the country, became very much less than was required for the service of the public. The eager demand of the public, however, for that medical assistance which the College could not supply, called into existence all kinds of irregular practitioners and quacks. Of these, the parties who naturally took the lead were the barber-surgeons, who had ready admission to families in the course of their ordinary employments; and the apothecaries, who were then merely drug-compounders, as the chemists are at the present day: these, finding it to their advantage to acquire more and more of medical information, did so, and almost imperceptibly, but very steadily, worked themselves into such favour with the public, that at last the surgeons have become a Royal College, rivalling the physicians themselves; and the apothecaries have followed close at the heels of the surgeons. About a century ago, when the College of Physicians attempted to prevent apothecaries from practising, on the plea that no man had a legal right to prescribe medicines who was neither fellow or licentiate, the College did not succeed; and from that day the apothecaries practised almost without hindrance, until, in 1815, an act of parliament was passed, giving them distinct authority. The education of the apothecary, in the meantime, has been constantly improving; and at last men of very high acquirements—indeed, members of the colleges at Oxford and Cambridge, and graduates of the Universities—have entered the class of apothecaries: thus raising the character of that class, while the number of physicians is in consequence still farther diminishing.

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THE MEDICAL SCHOOLS.

A WORD TO STUDENTS.

On our wrappers for some weeks past, and more particularly on those which envelope our present number, will be found the announcements of all the medical schools in the metropolis, as well as of the principal of those in the provinces,—besides the private advertisements of all those teachers who issue cards apart from the establishments to which they are attached: so that, by a simple inspection of our covers, the whole arrangements for the session may be known. This being the case, our readers will not expect *us* to serve them up the husk for the kernel—to transfer the contents of our wrappers into the pages of our volume; nor will they find us garbling the affiches of the schools into a catchpenny "advice to students."

We have only to request gentlemen, who are about to become pupils, to be on their guard against those volunteer advisers. One of our contemporaries, they should be aware, is notorious for his partialities to certain institutions, and his avowed enmity to others. Let them, if his paper fall into their hands, only observe this ill-disguised hostility: they will at once see the grossness with which he abuses and belies certain parties, while, with equal justice and discrimination, he bedaubs others with his fulsome praise.

If we were to venture any word of advice to the student, it should be, that he would exercise his own judgment, assisted by the experience of friends on whom he can rely, and by no means be induced to enter hastily to any school till he is perfectly satisfied as to the real character and pretensions of its conductors. He will find, perhaps, as the result of this proceeding, that those stand the test worst who are most patronized by the worthy Mentor to whom we allude.

We are sorry to observe, in taking a survey of the general announcements, that there are certain establishments—one or two at least—which endeavour to attract notice by assuming sounding titles to which they have no right, and making promises which they know are not worth a farthing. It is painful to have to advert to such conduct on the part of any metropolitan body—but the facts are glaring, and especially worthy of exposure, for the sake of those young persons and their friends who may have newly arrived in town, and are liable to be caught by puffing advertisements. All who are more at home in the metropolis, and can see through the trick, look at it with a smile, not, however, unmingled with some indignation, for there is dishonesty on the very face of it. A misnomer adopted for selfish purposes in one place, is just as knavish as the holding out, in another quarter, the empty lure of a diploma; they are both "springes to catch woodcocks." Our best advice to students is to beware of these industrious bird-catchers.

THE VALETUDINARIUM—CHAIR OF SURGERY.

We perceive that the *University*, having failed with regard to its promised degrees, is making very laudable efforts to get up a new attraction, as a substitute, in a building which we scarcely know how to describe, or even to name. It was called *Valetudinarium*, in an inscription very appropriately cut in brass, to assure future ages that it had once existed; the Secretary advertises it as the *North London Hospital;* and the Professor of Medicine calls it, in *his* advertisements, the *London University Hospital.* This last title is certainly the most correct; for, being wholly a private concern, and on much too small a scale ever to be regarded as a public institution, it should, in fact, be called after the school to which it belongs. The period appointed for its opening is

changed in almost every fresh announcement. It was to have been ready by the 1st of October last year,—then by the 1st of January,—afterwards it was postponed till October of this year, and now stands announced for next Christmas —perhaps, "latter Lammas." It can scarcely, we think, be very fit for the abode of the sick at present, for the walls in the interior are not yet plastered, nor are there even window-sashes in the lower story. The kitchens have yet to be created before a bowl of soup can be prepared; and we suspect that a tolerable proportion of acute and chronic rheumatisms will form the staple cases of the season—if, indeed, patients be admitted at all*.

A word further: we have heard of the odd hitch in this establishment, about getting somebody to "do" the operations. A *practical* professor of surgery is wanting, and it seems the office is going a begging; since a highly-intelligent surgeon of St. Thomas's, and we believe also a celebrated Edinburgh operator, though looking out for something to "do" in London, have declined it. This business rather surprises us; for we remember Dr. Elliotson, in his opening address, session before last, said distinctly, with reference to filling the chairs in the Valetudinarium,—"the appointment to most of the medical professorships implies that the professor is *qualified* to teach medicine and *surgery practically*;—if he be *not*, neither is he fit for his professorship, and *he should be compelled to vacate it.*" Whence, then, the difficulty?

PHRENOLOGY AT A DISCOUNT.

At the late Edinburgh meeting of the British Association, "some attempts were made to introduce the question of Phrenology, but the subject was manifestly distasteful to the majority. This gave offence to some of the enthusiastic votaries which this science has in Edinburgh; and they were still more annoyed when Professor Graham gave some sly and severe hits at the phrenologists, who had *actually proposed that the Association should establish a phrenological section!*"—*Athenæum.* Verily, the modesty of those people is edifying.

* As this is passing the press, we perceive that by the latest advertisement, the time of opening is once more changed—it is now *said* that the Valetudinarium will open the beginning of November!!! This gives double force to our preceding remarks.

SURGICAL REPORTS FROM THE LONDON DISPENSARY.

By R. R. Robinson, Esq.
Surgeon to that Institution.

Case I.—*Calculus Vesicæ—Stone impacted behind the Pubes.*

John Brocklebank, æt. 64, November 25th.—Has been very healthy, except when labouring under stricture some time since, for which instruments were passed with advantage. Applied at the Dispensary some weeks ago, in consequence of cutting pains in the perineum, frequent micturition, pain at the glans penis, scalding, &c. A sound was passed, and a stone distinctly felt impacted behind the pubes. Soda and uva ursi were taken with great benefit for a time, but after a month's trial the above symptoms increased, rigors occasionally appeared, and the urine became muco-purulent. His urine never stops suddenly, nor does jumping increase his pain. Leeches were repeatedly applied, copaiba, nitric æther, and laudanum, were taken, with temporary benefit. A sound was again passed, and a small irregular calculus again detected, which he was advised to have removed. Not liking this idea, he consulted some other surgeon, who told him there was no stone in the bladder. His sufferings not being relieved, he came back, and I requested Mr. Luke also to examine the bladder, who clearly detected the calculus in the above situation. The bladder was injected with warm water, the pelvis was raised, and the head depressed; by which means the calculus changed its place, and was felt towards the fundus (the upper part) of the bladder. The sound could be heard distinctly to press against the stone, and the man was enabled to feel it himself: still he would, on no account, submit to any operation. Muriatic acid was tried for a few days, but the vesical irritation increasing, and the bowels being purged, it was omitted.

May 9th, 1833.—Much reduced; has been for a week together free from uneasiness, when he passes urine freely, and then for weeks together greatly distressed; frequently micturates seven or eight times in the night, and six or seven in the day: fre-

quent diarrhœa, with bloody discharge, and prolapsus recti. Urine small in quantity, clearer than before, but still deposits a yellow thready lympho-purulent substance. Very weak, and confined to bed.

September 12th, 1834.—I have since heard that he continued to suffer severely up to the day of his death, which happened some months ago.

Remarks.—This case exemplifies the severe suffering of calculus cases when left to themselves; though, from the man's age, the irritation of the rectum, and the diseased condition of the mucous membrane of the bladder, it was not favourable either for lithotomy or lithotrity.

One of the surest signs of stone, independently of sounding—viz. pain on motion, particularly on jumping—was in this instance absent.

This case shews the impropriety of boldly stating that a calculus *does not* exist, because we are unable to detect it. This man was at first very ready to believe, from the grateful intelligence of the other surgeon, that I had mistaken his case; but soon found, from a continuance of suffering, that my opinion was too true. The situation of the calculous easily explains how it might elude detection; for being impacted behind the pubes, unless the beak of the instrument was much elevated and the handle depressed, it was not perceptible.

The advantage of injecting the bladder in doubtful cases, was very conspicuous here: by so doing, the coats of the bladder were more distended, and the pouch formed by the muscular coat, frequently contracting upon the calculus, was drawn out; the stone, therefore, being no longer grasped, was enabled, when the pelvis was elevated and the body depressed, to fall towards the fundus, where it could with facility be felt and heard by the patient himself, who could therefore no longer remain in doubt about the nature of his complaint.

The stone was also by this means placed in a far more favourable situation to have been laid hold of by the forceps, had lithotomy been performed; for had it continued firmly imbedded behind the pubes it would have been very difficult to remove.

Another case of stone in the bladder presented itself to my notice during the period these reports embrace. It also occurred in a man who declined submitting to any operation, as his sufferings were by no means great, and his age far advanced.

Case II.—*Retention of Urine, from Abscess juxta Anum.*

Thomas Greenaway, æt. 40, admitted August 16th; many years the subject of stricture in the urethra, for which instruments were passed, since which he has had no inconvenience till lately. Had an abscess formed to the right of the raphee of the perineum some months back, which broke and discharged matter. It healed, and subsequently broke again towards the rectum, leaving a fistulous canal, which discharges a serous fluid. Has experienced heat, pain, redness, and swelling, in the *left* side of the rectum, the last few days. Bowels confined.

Hirud. iv. Lot. Plumb. Pil. Hydr. gr. v. alt. noct. Mist. Cath. Ant. bis die.

August 23d. — The swelling has increased. A distinct abscess has formed to the left of the rectum; the perineum very tense; rigors; fever; no urine has been voided for many hours, though he makes constant efforts; no perceptible swelling in the hypogastric region: pressure there gives pain; a free incision gave vent to much healthy pus, and afforded much relief. Bowels still constipated.

Ol. Ricini, ℥i. Rep. Mist. et Pil.

Aug. 24th.—Soon after the operation, he passed nearly *three pints* of urine, which has passed freely since. Quite easy; a little pus discharged from the wound; slept well; less fever.—Perstet.

27th.—Wound looks well; swelling entirely subsided. Abscess does not communicate with the gut; neither does the fistula, which I laid open to-day.

29th.—Wound on the left side healing fast; fistula throughout rather callous; bowels open; fever gone.

Rep. Mist. p. r. n. Ung. Hydr. Nitri-oxidi.

Sept. 10th.—Wound on the left side quite healed; the appearance of the fistula improved.— Rep. Ung.

20th.—Fistula healed up. Discharged, cured.

Remarks.—Although abscesses are not unfrequently seen in the perineum, they are not so common as abscesses juxta anum: like them, they are frequently attended with pulmonary disease; but in this case nothing of that kind existed.

Abscesses in the perineum may occur either in the circumscribed or diffuse form; if they are situated towards the raphee of the perineum, or are exceedingly large, they may press upon the urethra, and impede the passage of urine; may even, as here, produce complete retention. In general, however, though with pain and difficulty, some, and perhaps the greater part, of the urine, is voided. It would be difficult to select a case which more forcibly illustrates the well-known surgical axiom—"Remove the cause, and the effects will frequently soon be removed also;"—for no sooner was the pus, which pressed

upon the urethra, evacuated, than the contents of the bladder, amounting altogether to *three pints*, were with facility voided. This abscess was very large, for it extended from the perineum to the verge of the anus; but as the matter was not retained long enough to injure the coats of the rectum, it readily healed.

CASE III.—*Retention of Urine during Utero-gestation—Partial Peritonitis?—Hysteria—Catheter slipped into the Bladder.*

Louisa Levi, æt. 29, admitted October 18th, 1832, under Dr. Frampton. Has been pregnant three months: difficult micturition and stillicidium urinæ three weeks, which she attributes to prolapsus uteri, her womb always coming down when she sits up, and which she has a difficulty in returning; has passed her urine only in drops the last few days; constant fruitless efforts to micturate; abdomen distended at the lower part—hard and tender to the touch; sickness; compressed pulse; fever. Upon examination per vaginam, the uterus is now in situ. A catheter was introduced, and three pints of highly-offensive urine was withdrawn; she went immediately into hysterics, and these were followed by syncope, upon recovering from which she suffered agonizing pain upon the least motion, or any kind of pressure upon the abdomen.

V.S. ad. ℥xvi.; Hirud. x.; e fotu abdomini; Hydr. Subm. gr. v.; Opii, gr. i. statim; dimid. tertia quaque horâ.

Oct. 19th.—Blood not buffy; rather less pain; abdominal respiration free; some sickness; no sleep; several motions; pulse feeble, 120; tongue pale; frequent hysterics. Urine drawn off night and morning, when she always goes into hysterics, and subsequently faints; bears pressure upon the abdomen better, but is still tender.

Rep. Hirud. Mist. Sal. c. Tr. Valerianæ, ℥ss. ter die; Pulv. Dov. gr. x. Hydr. Submur. gr. i. o. n.

Oct. 22d.—Complains of constant excruciating pain in the abdomen. She flinches when pressure is made upon the belly, and her attention drawn off; abdominal respiration free; countenance wan and pale; urine still retained—about a pint drawn off night and morning; ammoniacal; bowels rather confined.

P. P. ℈j. statim; Mist. Camph. c. Spirit Æther Sulph. ter die.

25th.—Uterus dilating—parts lax; more pain in the abdomen and back; frequently sick; ammoniacal urine copiously secreted, still retained in the bladder; bowels open; no fever; pulse not much above 70. In introducing the catheter to-day, I was bit by a flea, which caused me to withdraw my hand before all the urine was removed, when the catheter slipped into the bladder; I succeeded, however, in getting it out with my pocket forceps and probe, but not without difficulty, nor till after several unsuccessful efforts.

May 25th, 1833.—She called upon me to-day, in perfect health. The catheter was introduced for her night and morning for some time; but as the retention seemed evidently connected with utero-gestation, she was instructed to introduce it herself. She was admitted on the books of the Maternity Charity, under Dr. F. H. Ramsbotham; had another attack, apparently, of peritonits; was bled and leeched freely, with relief. She continued the catheter, except now and then, when she, with much straining, passed a little urine by the natural powers, until three weeks ago, when she was put to bed (the labour was lingering, but otherwise natural), after which she had no farther occasion for it.

REMARKS.—My chief object in narrating this case is to put on record, in this journal, a third instance of a catheter accidentally slipping into the bladder. In Mr. Toogood's cases, the instrument appears to have gone farther into that viscus than in mine, and was allowed to remain several days there; in one case it was found lying across the bladder; in both the urethra was dilated by sponge, and in that way the catheter was extracted: in one, incontinence of urine, of short duration however, followed. In my case I was fortunately able to reach the catheter (*a short sliding one*) with my forceps, and to extract it almost immediately after the accident.

Should such an accident occur to me again, which I do not think it will, as I have since attached a string to the wings of the catheter, I should prefer trying first of all a thin pair of long narrow forceps; and if these failed, the urethral dilator in preference to sponge.

This case appears to me to be further interesting, as it illustrates the difficulty, sometimes, I believe, felt even by experienced practitioners, in distinguishing between peritoneal inflammation and hysteria.

I think, as there was pain, increased upon pressure, and that, when the woman's attention was drawn off, sickness and fever, that partial peritonitis was really present, more especially as she had a relapse, which yielded to antiphlogistic treatment; but as respiration was still carried on by the abdominal muscles, as bleeding did not much relieve her, as the blood was neither cupped nor buffed, as antispasmodies were not injurious, and as she had well-marked hysterical attacks, I would ascribe a great part of the pain, and some of the other symptoms, to the state of the nervous system witnessed in hysteria.

The retention of urine appeared to me to be owing chiefly to the pressure of the uterus upon the neck of the bladder, as it did not cease until that viscus had got rid of its contents, but partly also to nervous excitement and inflammation about the neck of the bladder, as the urine was ammoniacal, and the pain, on introducing the instrument, so acute as to lead to well-marked hysterical attacks, and subsequently to syncope.

CASE IV.—*Retention of Urine—Paralysis of the lower extremities, cured by blistering and muriated tincture of iron.*

Moses Levi, æt. 52, admitted January 27th, had a fright four months ago; immediately after which he lost all power of motion, but not sensation, in his lower extremities; has been since unable to void any urine. A silver catheter was first employed, and now an elastic one; both of them pass with facility; urine clear, not ammoniacal. Can walk about with crutches, but not without them.

Tr. Ferri Muriat. m. x. ter die.

February 3d. — Feels stronger. Has passed urine for the first time this morning without the instrument. Much purged.

Vesic. Lumbis. Quin. Sulph. gr. i. ter die.

6th.—Has so completely regained the power of passing his urine, that the catheter has not been introduced these two days. Still purged.

Omittantur Medic.

March 3d.—Walks much stronger. Micturates very freely, indeed he states in preternatural quantities.

Pil. Hydr. Pil. Sap. c. Opio. gr. iiss. n. maneque.

20th.—Can walk with a stick only. Still discharges, according to his own account, large quantities of urine, especially at night.—Rep. Pil.

30th.—The quantity of urine has diminished, and is now not greater than natural. Discharged cured.

REMARKS.—This case possesses interest from the suddenness of the attack, from the paralysis having arisen from fright, and from its successful issue.

From the beneficial effects of iron, and the application of a blister to the loins, the paralysis would seem to have been owing to debility rather than to congestion or disorganization of the medulla spinalis.

CASE V.—*Retention of Urine—Paralysis of the Lower Extremities—Ramollissement of the Medulla Spinalis—Cystitis—Phthisis of the right Lung.*

John M'Donald, æt. 32, admitted March 28th, painter, formerly the subject of colica pictonum; pale and emaciated; confined to his bed two months with chronic inflammation of the joints, for which he was bled; took calomel, colchicum, &c. with some relief. On the 26th instant he had retention of urine, which was drawn off by a catheter to the amount of upwards of a quart. He has made none since. Has much pain in the hypogastrium, which is increased on pressure: the bladder can be distinctly felt, of a globular form. On tapping the abdomen, in the course of the descending colon, a tympanitic sound is elicited. Catheter No. 9 passed with tolerable facility into the bladder, when a quart of high-coloured offensive urine was withdrawn.

Ol. Ricini. ℥i. statim. Enema Terebinth.

March 29th. — No urine voided; but little pain; no power over his lower extremities; several motions, with some flatus; less tympanitis; thirst urgent; tongue coated, dry, and red; pulse irritable, 120; skin hot. Catheter passed with some difficulty, which appeared to arise from a flaccid state of the membranous portion of the urethra. A pint of high-coloured offensive urine was withdrawn; the bladder was subsequently injected with warm water.

Liq. Am. Acet. ℥ij.; Pulv. Dov. gr. v.; Mist. Camph. ℥i. sextis horis; Pil. Opii. gr. i. o. n.

April 1st.—No urine voided. Catheter has been daily used, and the bladder afterwards washed out. The tartarized antimonial ointment has been applied to the loins; motions passed involuntarily; appetite bulimious; tongue clean, less red.

Rep. Mist. et Pil.

2d.—Weaker; hectic; tongue aphthous; motions passed involuntarily, as also a pint of ammoniacal urine, tinged highly with blood, and loaded with mucus; pulse quick.

4th.—No urine passed yesterday; ℥viij. involuntarily to-day; two motions passed in the same manner; bladder notwithstanding distended with water, which was removed by the catheter.

5th.—Much weaker; a constant cough; respiratory murmur loud throughout the anterior part of the chest; two motions passed involuntarily, as well as a little high-coloured urine, which is, however, less ammoniacal, and not so deeply tinged with blood; catheter introduced; a pint of urine withdrawn; bladder subsequently injected with warm water, which brought away a few flakes of lymph, or mucus.

6th.—Breathing became laborious. He died this day; just previously (his wife states) he drew up his legs.

Dissection.—Cellular substance over the spinous processes of the lumbar vertebra largely infiltrated with serum; the muscles in this situation congested and softened; the medulla spinalis in the dorsal and lumbar regions quite pale, reduced to a pulp, and in a complete state of ramollissement; but a small quantity of fluid in the theca vertebralis. Four ounces of serum in the right side of the chest; right lung adherent to the ribs, of chocolate colour, fragile, and easily broken down, occupied in various parts by tubercles in different stages, and some small vomicæ in the upper part. Left lung unadherent, and healthy in structure; a few contractions on its pleura; no fluid in the left side of the chest.

The mucous membrane of the ileum and cœcum worm-eaten in several places, and of a deep green colour; the other chylopoetic viscera healthy; kidneys considerably larger than natural; the left nearly twice the size of the right; preternaturally congested; calyces very vascular, and filled with a purulent-looking urine; both ureters contracted.

Bladder distended by upwards of a pint of straw-coloured, highly ammoniacal urine, with shreds of lymph floating in it. The coats not particularly thickened; the mucous membrane, however, was lined with coagulable lymph, easily separable; in some parts quite white; in others, more especially about the cervix of the bladder, vascular; prostate, and membranous portion of the urethra undiseased.

Remarks.—We have here very extensive disease, viz. phthisis of the right lung, ramollissement of the medulla spinalis, disease of the bladder and kidneys, ammoniacal urine, and rheumatism,—all, perhaps, ascribable to paint, as M'Donald was some time ago the subject of painter's colic, which he appears never to have entirely recovered.

It is, perhaps, difficult to determine the order in which these several affections arose. Was the disease of the lung the primary affection?

I am inclined to think that tubercles were deposited in the lung a long time back, and that the irritation resulting from other disease and debility were conducive to their development. Was the state of the medulla spinalis connected with the disease of the lung—was it the metastasis of rheumatism, or was it owing to the effects of lead, with which his constitution appears to have been impregnated?—Phthisis is occasionally seen in combination with almost every morbid alteration of texture, and has consequently sometimes been noticed in conjunction with spinal disease, of which I have mentioned some examples in a former report; but there does not in this case seem any necessary connexion between the state of the lung and that of the spinal marrow.

Metastasis of rheumatism from the joints to the brain and its membranes, is sometimes witnessed, but I do not remember to have seen any thing like such metastasis to the spine. The pains in the joints did not suddenly cease, neither did the paralysis so suddenly follow, as to give proof of any such transition. I am therefore disposed to think, that the habitual use of paint, of which lead is a large ingredient, not only produced colic, but also irrecoverably destroyed the texture of the medulla spinalis. The congested state of the muscles of the back, and the œdematous condition of the integuments covering them, shew that great excitement had been going on here.

Was the diseased state of the vesical mucous membrane the result of irritation from the ammoniacal state of the urine, or was it owing to a loss of nervous energy? The inflamed state of the bladder frequently seen after injuries of the spine, and in some cases of paralysis, has by many surgeons been attributed to the former cause; but my friend Mr. Curling, in an interesting paper upon affections of the bladder in paraplegia, detailed in the last volume of this Gazette (p. 76), has suggested that it may be owing to the bladder, deprived of nervous energy, being no longer capable of withstanding disease, in which opinion I am inclined to concur; and which the case I related in my first report (June 7th), of sloughing of the cheek from pressure on the fifth nerve of that side, and the case of Mr. Stanley's, which I there quoted, seem to confirm.

SEVERE CASE OF NEURALGIA,

Successfully Treated.

By John Scott, Esq.
Surgeon to the London Hospital, &c.

On the first of December, 1833, I visited a young lady who was labouring under the most distressing neuralgic affection I ever witnessed. The left knee was the seat of a constant aching, gnawing pain, which was referred to a considerable extent of its surface, whence it darted into the centre of the joint, extending also upward to the middle of the thigh; and the surface of the knee was so exquisitely sensitive that she could not bear the slightest touch, nor even the weight of her ordinary clothing. Sometimes there was a distinct throbbing sensation, occasionally attended with a considerable blush upon the skin to some extent up the thigh; and this would generally be produced by any exertion, such as the change of position, during a meal; and it invariably ushered in a spasm, to

which she was also constantly subject, without its being produced by any exciting cause. These spasms in the knee were excessively violent, productive of intense agony, and generally occurred instantaneously, but they were sometimes preceded by a gradual increase of pain: they came on spontaneously, without any provocation, about every hour, and were usually of an hour's continuance, with scarcely an intermission of a moment: sometimes they would last only three-quarters of an hour; at others, an hour and a half; and then subside slowly, not instantaneously, but becoming gradually less and less severe.

These spasms were, in general, equally frequent day and night; sometimes, however, she would fall asleep for two or even three hours, and then awake with a start, which would be instantly followed by a spasm of extreme violence. In addition to this constant recurrence of the spasms every hour, they would be produced at any time by the most trifling circumstance; thus, sneezing, coughing, the slightest jar or motion of the limb, the least touch of the couch on which she was confined, or even the banging of a door, would be instantly followed by a very severe paroxysm of pain.

The disease commenced in February, 1831, when some degree of stiffness and uneasiness was first experienced in the joint; and about a month afterwards slight spasms were felt, which lasted a minute or two, and recurred very irregularly; but from that time they gradually increased in severity, as well as in frequency and the length of their continuance, until the following October, at which time they usually lasted for about a quarter of an hour. They then began to increase in a more regular and uniform manner. Thus they would continue without any material alteration for about three weeks, and then a more severe spasm would come on, and each succeeding one would be equally violent for about the like period; then a spasm of increased severity would again occur, and be succeeded by others of equal violence for about three weeks more, when one of still greater severity would come on; and in this manner her sufferings had been gradually becoming more and more distressing, until they arrived at the pitch above described: she had been confined to the same couch, day and night, for nearly three years, without the possibility of her being removed from it.

This patient, when eleven years of age, was the subject of a very painful affection of the left ear, which was so susceptible to impressions, that she could not bear the slightest noise, not even that of riding in a carriage; and the sound of music was productive of so painful and distressing a feeling, that she could not endure it for a moment. This complaint, which, according to her description appeared to be an affection of the chorda tympani, resisted every mode of treatment that could be devised, for many years, and then gradually wore itself out.

About a year and a half after the subsidence of the last named complaint, a neuralgic affection of the throat occurred, attended with most severe and distressing spasms, which were brought on by the act of deglutition, but not by speaking or mastication. This affection resisted every remedy that was employed, for nearly two years, when Mr. Pennington was consulted, who, observing that the spasms were brought on by swallowing, recommended her to abstain from making any attempt to do so for the next twenty-four hours, and directed an enema to be administered, containing a drachm of assafœtida and one hundred drops of laudanum. In consequence of this suggestion, the period above-mentioned was passed without any return of spasm, but it was again immediately produced by eating a morsel of bread.

Mr. Pennington then recommended her to abstain altogether from taking food by the mouth; and nutrient enemata were administered twice or thrice a day, composed of beef tea with an egg dissolved in it, for which veal broth and milk were afterwards occasionally substituted. At this time the secretions were much disordered; there had been no appearance of the catamenia for nine months; the pulse was frequent, quick, and irregular. After pursuing the plan just named for four days, very considerable excitement was produced, attended with some degree of fever, quick pulse, and flushed cheeks, &c.; but it shortly subsided, the pulse becoming less frequent, and the fever disappearing. At the end of a fortnight bilious secretions were observed, and in a month the catamenia re-appeared; the pulse was between 70 and 80, and not deficient in strength. After the lapse of five weeks and three days, without any nourishment being introduced by the mouth, the pulse suddenly sank to 35 in a minute, and it was then deemed advisable to give by the mouth a dessert spoonful of beef tea twice a day; and this being continued for four days without inducing any return of the spasms, a small piece of fish was allowed, and afterwards some chicken; and proceeding thus cautiously, in the course of a month she was able to eat and drink any thing without the slightest inconvenience.

On the 1st of December I applied the deuto-ioduret of mercury to the knee, thickly spread upon lint; and it is a remarkable circumstance, that this application was renewed several times, without producing the slightest effect upon the

skin. This being the case, on the 24th of December I rubbed the surface of the knee with camphorated spirits of wine, so as to make it quite red, and then applied the ointment very thickly spread on lint; the consequence of this irritation was a spasm which lasted without intermission thirteen hours and three quarters, when it suddenly subsided, and left her quite exhausted; but being free from pain, she slept for several hours.

The effect of the ointment was extremely violent; it blistered the surface to the extent of its application, and also produced copious salivation. The spasms continued to recur as usual for two or three days, and then began sensibly to lessen both in frequency and severity; and on the 1st of January she had no spasm at all, which was the first day she had been entirely free from them for nearly three years: the aching pain still continued, but was very considerably diminished in violence. She continued free from any recurrence of spasms until the 14th of January, when one or two were produced by some accidental movement of the limb, the effect of the ointment having at this time entirely subsided, the surface of the knee being perfectly healed.

The ointment was immediately re-applied, and renewed as frequently as would keep up some degree of soreness of the skin, but not carried to the extent of producing the slightest constitutional influence; and it was found necessary to continue the application for a considerable time, to wear out the morbid disposition which disease of so long continuance must necessarily have induced.

On the 21st of January she left the couch, and shortly afterwards the room in which she had been so long a prisoner. She then gradually acquired strength, and was soon able to walk on the limb, with the aid of crutches *.

LITERARY INTELLIGENCE.

A Treatise on Insanity, and other Disorders of the Mind; accompanied with numerous Cases exemplifying various descriptions of Madness, by S. C. Prichard, M.D., F.R.S., Physician to the Infirmary, and to St. Peter's Hospital, Bristol, is in the press.

In the press, a Dictionary of Terms used in Medicine and the collateral Sciences; containing the Etymology and Meaning, Nomenclatures, Classifications of Nosology, Materia Medica, &c., Tabular Sketches of Chemistry, Medical Botany, and Zoology. By Richard D. Hoblyn, A.M., late of Baliol College, Oxford.

* From Cases of Tic Douloureux, and other Forms of Neuralgia. By John Scott, Esq. *(Just published.)*

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *Sept.* 23, 1834.

Abscess	9	Hooping-Cough	18
Age and Debility	53	Indigestion	1
Apoplexy	9	Inflammation	42
Asthma	5	Bowels & Stomach	14
Cancer	3	Brain	3
Childbirth	17	Lungs and Pleura	4
Cholera	38	Insanity	14
Consumption	105	Jaundice	3
Constipation of the Bowels	2	Liver, diseased	6
Convulsions	59	Measles	7
Croup	1	Miscarriage	1
Dentition or Teething	20	Mortification	13
Dropsy	35	Paralysis	2
Dropsy on the Brain	15	Rheumatism	2
Dropsy on the Chest	1	Small-Pox	8
Dysentery	2	Sore Throat and Quinsey	5
Epilepsy	2	Spasms	3
Erysipelas	2	Stone and Gravel	1
Fever	22	Thrush	6
Fever, Scarlet	16	Worms	2
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METEOROLOGICAL JOURNAL.

Sept. 1834.	THERMOMETER.		BAROMETER.	
Thursday . 18	from 43	to 73	30·05	to 30·21
Friday . . 19	45	73	30·27	30·28
Saturday . 20	48	75	30·26	30·24
Sunday . . 21	45	70	30·22	30·21
Monday . . 22	55	67	30·19	30·11
Tuesday . . 23	45	61	30·12	30·13
Wednesday 24	42	62	30·11	30·09

Wind variable, N. prevailing.

Except the 19th, 20th, and 23d, generally cloudy; a little rain on the 18th, not measurable.

CHARLES HENRY ADAMS.

OUR NEW VOLUME.

WE have made arrangements for the ensuing volume, which we think will prove satisfactory to our readers, and add to the increasing circulation which the journal enjoys. Next number (which is the first of a new volume) will contain the commencement of a course of lectures on Diseases of the Chest, and the application of the Stethoscope to Diagnosis, by Dr. Davies, of the London Hospital, who is well known to have devoted much and successful attention to these subjects, and who has been good enough to correct the lectures, with a view to their publication in this journal. Also, a lecture by Sir Benjamin Brodie, on Encysted Tumors, being the first of numerous clinical papers by distinguished practitioners, which will appear during the season. A Case of Aneurism, in which the *Aorta* was tied; with an Engraving. Report from the Glasgow Infirmary, on Melanosis and Ossification of the Eye, from the clinic of Dr. Mackenzie, also with an Engraving;—together with numerous other articles of interest.

W. WILSON, Printer, 57, Skinner-Street, London.

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